THE SOCIO-PSYCHOLOGICAL

FOUNDATIONS OF SCIENTIFIC CHANGE

by

David Leslie Fairfax Williams

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PREFACE

With the rise of logical positivism the idea that scientific knowledge is objective and testable became widespread. Successfully challenging assumptions fundamental to the positivists' standpoint, Popper's famous critique nevertheless endorsed their views that there existed a method unique to scientific inquiry, adherence to which made objective knowledge possible. This alleged objectivity has been seriously challenged in recent discussions concerning the history and philosophy of science. Kuhn and Feyerabend are two philosophers who have spearheaded this movement. Focusing attention mainly on revolutionary developments in the history of science, these two philosophers have forcefully argued that such revolutions were only possible given the existence of subjective determinants.

This essay begins with a brief characterization of positivistic philosophy of science and the views of Popper. Following the claim that subjectivist elements enter into Popper's epistemology, attention is directed to the presentation of the explicitly subjectivistic philosophies of science of Kuhn and Feyerabend.

The relevance of these recent philosophic developments for the place of theory in psychology is demonstrated within the context of a critique of B.F. Skinner's behaviouristic approach to psychology.

Failing to appreciate the critical importance of subjectivist elements in the development of science, scientists in general still cling to positivistic and Popperian views. Social psychologists, still holding objectivist views, are amongst those who consider that the behaviour of the scientist *qua* scientist lies outside their domain of

inquiry. Consequently, social psychologists are not forthcoming with the requisite psychological theories considered by philosophers such as Kuhn and Feyerabend to be necessary to account for scientific change.

While emphasizing the *role* of subjective factors as determinants of scientific change, Kuhn and Feyerabend have nevertheless failed to suggest a *theory* accounting for such change in terms of these subjective factors. To rectify this state of affairs a socio-psychological theory of scientific change is proposed. The development of a socio-psychological understanding of the nature and function of scientific theory constitutes an integral part of this programme. In terms of these ideas an attempt is made to identify possible socio-psychological antecedents of scientific change.

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CHAPTER 1

INTRODUCTION

Many issues are decided by many people on a basis of party spirit, not of detailed examination of the problems involved. In particular, whatever presents itself as empiricism is sure of widespread acceptance, not on its merits, but because empiricism is the fashion.

Bertrand Russell

> "When the third pair of American astronauts landed on the moon in the summer of 1971, their activities included *a replication of Galileo's demonstration* that when effects of friction are eliminated a light object and a heavy object will, when dropped simultaneously from the same height, reach the ground at the same time. This *verification of a basic principle* of high school physics, delighted many viewers of the live televised broadcast, but probably few of them considered the fact that for hundreds of years before Galileo, Western scholars had accepted the Aristotelian position that heavy objects would fall faster than lighter objects. What is amazing about this fact is not that observers of Aristotle's intelligence would have held such a common sense notion, but that *it never occurred to any of them to test it*. The emphasis on *subjecting all theoretical concepts to empirical demonstration* is basically what distinguishes the scientific method from other forms of inquiry . . . ."
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> W.D. Crano and M.B. Brewer: *Principles of Research in Social Psychology*. (My emphasis)

This passage might have been taken from any of hundreds of texts introducing the nature of scientific inquiry, and indeed is quite typical of the approach to the nature of science found in psychological works. Furthermore the sentiments expressed would find favour with most scientists. There are arguments, however, which seriously question the passage's validity. Each of the four sentences contains a questionable assertion. Consider the claim that there had been "*a replication of Galileo's demonstration*...." Close historical examination of the relevant material reveals that the usual story concerning Galileo's demonstration is largely based on myth. For example, in his *The Copernican Revolution* Thomas Kuhn writes:

> "We are ... often told that it is only because medieval scientists preferred the authority of the written word, preferably ancient, to the authority of their own eyes that they could continue to accept Aristotle's absurd dictum that heavy bodies fall faster than light ones. Modern science, on this interpretation, began when Galileo rejected texts in favor of experiments and observed that two bodies of

> unequal weight released from the top of the tower of Pisa, struck the ground simultaneously. Today every schoolboy knows that heavy bodies and light bodies fall together. But the schoolboy is wrong, and so is this story. In the everyday world, as Aristotle saw, heavy bodies do fall faster than light ones. That is the primitive perception ... To verify Galileo's law by observation demands special equipment; the unaided senses will not yield or confirm it Probably he did not perform the experiment at the tower of Pisa. That was performed by one of his critics, and the results supported Aristotle. The heavy body did hit the ground first. The popular story of Galileo's refutation of Aristotle is largely a myth..."
>
> (Kuhn, 1957, p.95)

Even if Galileo had produced the experimental evidence in the manner stated by Crano and Brewer, and had consequently embarrassed the Aristotelian position, the finding would not have *verified* his own theory. Insofar as Galileo's law constitutes a universal statement having an unrestricted domain of application[1], no *finite* number of correct predictions based on the law can be said to verify or prove it true (Popper, 1959; Campbell, 1969). The most that could have been said is that the experimental results would have *confirmed* the theory. This means that the question of the theory's truth remains an open one.

It follows that Crano and Brewer's second assertion concerning the "*verification of a basic principle*" is based on two false suppositions: first, that Galileo performed the successful demonstration, when he did not; second, that had he so performed, that the principle would have been verified.

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1 Statements referring to *all* the members of some domain, class, or set, are called 'universal statements'. These can be one of either two kinds: restrictedly universal or unrestrictedly universal. The distinction corresponds to Popper's (1959) distinction

Consider the third assertion that it was amazing that Aristotelians held on to an incorrect principle and that "*it never occurred to any of them to test it*". What *is* amazing is that Crano and Brewer are not aware that everyday experience, still available to us today, supports Aristotle's position. Kuhn's reasoning would suggest in this instance that Crano and Brewer are victims of the authority of the written word in preference to the authority of their own eyes. The notion that heavy objects fall at the same speed as light ones cannot be sustained in the everyday world: a test of the very idea would support the opposing point of view. It is little wonder that Aristotle's theory was so widely held.

Interestingly enough, we have here an illustration which shakes the belief that scientific theories are distinguished by their testability. When Crano and Brewer argue in their fourth assertion that the scientific method subjects "*all theoretical concepts to empirical demonstration*" it would seem that they read "testability" for "demonstration". The idea that it is their testability that distinguishes scientific from other kinds of theories is one that in some form or another has been popular ever since the formation of the Vienna Circle[2] in the early 1920's.

The problems associated with the notion of testability will be discussed at some

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between 'numerical' and 'strict' universality. Strictly universal (i.e. unrestrictedly universal) statements claim to be true for any place and any time, while numerically universal (i.e. restrictedly universal) statements claim to be true for a finite class of specific elements within a finite spatio-temporal region.

2 Sharing the common concern of banishing all metaphysics from science a number of philosophers, scientists and mathematicians banded together and formed the famous Vienna Circle. The philosophy of science they developed became known as Logical Positivism. Founding members of this group included Moritz Schlick, Rudolf Carnap, Otto Neurath, Herbert Feigl, and Frederick Waismann. For a selection of papers representative of their views, see A.J. Ayer (ed.) *Logical Positivism*, 1959.

length in the following two chapters but it is considered convenient to introduce and identify one of these problems at this point.

It has already been noted that Aristotle's belief that heavy bodies fall faster than light ones is in better agreement with everyday observations than is Galileo's law. Nevertheless Aristotle's theory was rejected in favour of Galileo's. This in itself is sufficient to suggest that the testability criterion of scientific theories is suspect.

The nature of the problem we wish to raise here can very forcefully be demonstrated if one looks carefully at the strategy Galileo adopted in his attempts to rebut the 'observational' evidence that was claimed by the Aristotelians of his day to refute the heliocentric astronomy of Copernicus[3]. Again contrary to common belief, at the time of the initial debate between the Aristotelians and the Copernicans the observational evidence was strongly weighted in favour of the former. Indeed the empirical evidence so strongly supported the Aristotelians that no less than 100 years after Copernicus had first[4] put forward his heliocentric astronomy, Galileo was led to write:

> "... I am astonished that there have been any up to this day who have embraced and followed (the system of Copernicus). Nor can I ever sufficiently admire the outstanding acumen of those who have taken hold of this opinion and accepted it as true; they have through sheer

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3 An interesting and highly readable account of this period in the history of astronomy is to be found in A. Koestler's *The Sleepwalkers,* Hutchinson Press, 1959. For a more detailed historical treatment, see Thomas Kuhn's *The Copernican Revolution*, Alfred A. Knopf Press, 1957. A full discussion of the fundamental epistemological problems raised by the Copernican revolution is to be found in P.K. Feyerabend, 1970a and 1970b.

> force of intellect done such violence to their own senses as to prefer what reason told them over that which sensible experience plainly showed them to the contrary. For the arguments against the whirling earth are very plausible ... but the experiences which overtly contradict the annual movement are indeed so much greater in their apparent force that, I repeat, there is no limit to my astonishment when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became mistress of their belief".
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> Galileo, 1953, p.328

Nevertheless, in his *Dialogue Concerning the Two Chief World Systems* Galileo sets out to challenge the finality of the evidence-based arguments claimed to refute the Copernican theory. In doing this he adopts a strategy that involves the re-interpreting of "sensible experience" so that not only is this newly interpreted experience consistent with the Copernican theory, it in fact confirms this theory. This necessitates the development of a new 'observation language'[5] in other words, a new language with which to report basic observations.

The manner in which anomalous observations can be transformed such that they may be reconciled with a predetermined theoretical point of view itself demonstrates

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4 It was in his unpublished manuscript *Commentariolus,* dated approx. 1510-1514 AD that Copernicus first wrote about his heliocentric theory. In 1543 he published *The Book of the Revolutions of the Heavenly Spheres,* a book that contained a modified version of his earlier theory. Galileo's *Dialogue Concerning the Two Chief World Systems* was not published until 1621.

5 It is usual to distinguish between the 'theoretical' and the 'observational' terms of a given theory. The observational terms are those that apply at the most basic level of description and refer to the contents of observations. Theoretical terms operate at a more abstract level and, may be defined using observational terms. 'Observation statements' are statements written using a vocabulary of observational terms only. For a full discussion see R. Carnap, 1936, 1937.

that they do not constitute a neutral basis on which to decide between two competing theories. To illustrate this we look briefly at the famous tower argument and how it featured in the debate between the Aristotelians and the Copernicans.

According to Aristotle, the 'natural' motion of earthly bodies was vertical and straight downwards. Any component of horizontal motion was 'violent' motion and therefore unnatural, requiring some external force to bring it about and to maintain it. Furthermore all motion was *observed* motion, Aristotle's physics being based entirely on observables. With respect to the argument which was used by Aristotelians to refute the theory of the moving earth, Galileo writes that (for the Aristotelians) observation shows that

> "heavy bodies ... falling down from on high go by a straight and vertical line to the surface of the earth. This is considered an irrefutable argument for the earth being motionless. For if it made the diurnal rotation a tower from whose top a rock was let fall, being carried by the whirling of the earth, would travel many hundreds of yards to the east in the time the rock would consume in its fall, and the rock ought to strike the earth that distance away from the base of the tower".
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> Galileo, 1953, p.126

In considering the argument Galileo at once admits the correctness of the sensory content of the observation, viz., that "heavy bodies ... falling down from on high go by a straight and vertical line to the surface of the earth". Replying to an author who sets out to convert Copernicans by repeatedly mentioning this fact, he says:

> "I wish that this author would not put himself to the trouble of trying to have us understand from our senses that motion of falling bodies is simple straight motion and no other kind, nor get angry and complain because such a clear, obvious and manifest thing should be called into question. For in this way he hints at believing that to those who say such motion is not straight at all, but rather circular, it seems they see the stone move visibly in an arc, since he calls upon their senses rather than reason to clarify the effect. This is not the case ... for just as I have never seen nor ever expect to see the rock fall any way but perpendicularly, just so do I believe it appears to everyone else. It is therefore better to put aside the appearances, on which we all agree, and to use the power of reason".
>
> (Galileo, 1953, p.256)

In order to "save the phenomena" Galileo introduced an entirely *ad hoc* theory - his theory of circular inertia (see for details, Feyerabend, 1970b). According to this theory circular motion was as much a 'natural' motion for sub-lunar phenomena as for supra-lunar phenomena. In terms of this theory a stone dropped from the tower would continue in a state of circular motion and so rotate along with the whirling earth, not being left behind. In addition Galileo introduced a distinction between 'absolute' and 'relative' motion, where only the latter might be observed. Consequently although the stone, and the earth, might move with an absolute horizontal motion, in that there was no 'relative' motion between them, then any absolute horizontal motion they might possess could remain *unobserved* Hence the observation of only a vertical motion was reconciled with the Copernican theory. In short, by introducing a fundamentally new theory of motion along with a corresponding and new observation language Galileo was able to "save the phenomena".

As a consequence of this innovation both Aristotelians and Copernicans were able to truthfully claim that observations provided evidence which confirmed their own respective points of view. In view of the *disparity in the observations* yielded by their respective viewpoints and the *disparity in the understandings* yielded by their respective

theories, it becomes apparent that there exists an intellectually unbridgeable gap between the Aristotelian and Copernican points of view.

This is hinted at by Kuhn when he writes:

> "Copernicus's innovation was not simply to move the earth. Rather it was a whole new way of regarding the problems of physics and astronomy, one that necessarily changed the meaning of both 'earth' and 'motion'. Without these changes the concept of a moving earth was mad".
>
> Kuhn, 1962, pp.149-150

Kuhn (1962) and Feyerabend (1962) have each explored the epistemological implications they consider are introduced once this basic 'incommensurability'[6] between radically alternative theories is recognized. Following Popper (1959), Kuhn and Feyerabend both maintain the thesis that observation is theory-dependent (hence all observation terms are themselves theory-laden) and both argue that therefore incommensurate theories generate incommensurate world-views. In this way we might understand the problems the Copernicans encountered when confronted by their Aristotelian opponents who correctly pointed out that their observations confirmed the geo-centric theory and 'refuted' the heliocentric theory of Copernicus. To overcome these problems we have already seen how Galileo introduced a new theory of motion along with introducing a corresponding innovation in the observation language.

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6 The concept of incommensurability was first introduced into a discussion of conceptual developments in science simultaneously and independently by Kuhn (1962) and Feyerabend (1962). As a first approximation we might say that two theories having a more or less identical domain of application are incommensurable if and only if their

Yet the shift in point of view that took place in the thinking of the layman along with the Copernican revolution cannot be understood in terms of these intellectual developments alone. Where the required shift in point of view involves adopting a new understanding incommensurate with the old, then rational argument alone cannot be used to establish the desirability of such a shift in viewpoint. Given that there does not exist a theory-neutral base upon which to judge the verisimilitude of the respective theories, then it is not possible to establish that any theory describes reality any more accurately than a given existing alternative. (See for argument Kuhn, 1962, 1970; Feyerabend, 1970b, 1970c. Feyeràbend's basic argument will be presented in Chapter 3 of this essay.)

The epistemological problems generated by the phenomenon of incommensurability are largely at the basis of Kuhn's and Feyerabend's arguments attempting to demonstrate the relevance of psychological and sociological theory to the understanding of developments in science. Denying that rational argument alone can force a revolution in the scientists understanding, the dynamism behind revolution is sought elsewhere - in the psychology of the individual scientist and his social environment. On the question of the relevance of psychological and sociological theory to the understanding of developments in science Kuhn and Feyerabend are in agreement.

Now while Kuhn explicitly argues, in agreement with Feyerabend, that incommensurability introduces non-rational elements into the internal workings of the scientific enterprise, Feyerabend (1970b, 1970c) goes considerably further. For Feyerabend, recognition of the essential role played by these non-rational elements

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basic pre-suppositions differ to the extent that it is not possible to translate a sentence derived from one theory into a sentence, or sentences, of the other. In chapter 3 we shall have reason to focus attention on the epistemological problems generated by this phenomenon of incommensurability.

makes any attempt at distinguishing science from non-science at best arbitrary. The tenability of Feyerabend's claims will be assessed in Chapter 3 of the present essay.

Let us now summarize what we have found concerning the extract taken from Crano and Brewer's text. First, Galileo never experimentally demonstrated the correctness of his law at the tower of Pisa. Second, had he performed such an experiment it could not have verified his law. Third, the results of such an experiment would in fact have counted against the law. Fourth, it follows that Crano and Brewer's claim that "what distinguishes the scientific method from other forms of inquiry ... is the emphasis on subjecting all theoretical concepts to empirical demonstration" is, at best, problematic.

Of interest to the author is the fact that while current philosophy of science is more and more recognizing the relevance of psychological and sociological variables as determinants of developments in science, social scientists still adhere to a view of science in which these factors play no significant role. Indeed the methodology of the social sciences, both in theory and in practice, still largely adheres to the basic tenets of logical positivism (Moscovici, 1972). This is surprising in view of the fact that there exist socio-psychological and sociological theoretical frameworks that can provide and accommodate more adequate alternative conceptualisations of science and its knowledge. It is claimed that these alternative conceptualisations are more adequate in that they are better able to accommodate and account for developments in science as these are revealed to us by recent historical research.

In view of all this the present essay is written with a number of purposes in mind:

1. To outline some major developments in the philosophy of science from the time of logical positivism to the present day.

2. To suggest a socio-psychological interpretation of the nature and function of science.

3. To make explicit the connections between this socio-psychological interpretation and a recent development in theoretical sociology.

4. In terms of ideas put forward in 2 and 3 above, to suggest a number of areas in which this sociological theory contributes to an understanding of the nature and function of science.

5. In terms of a recent development in socio-psychological theory to suggest answers to a number of unresolved questions basic to the debate in current philosophy of science.

To best fulfil each of these purposes the essay proceeds in accordance with the following plan:

Chapter 2 looks at two major attempts to develop a philosophy of science in which science is viewed as a source of *objective* knowledge. It begins with a characterization of the basic tenets of logical positivism. Popper's (1959,1963)[7] main

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7 Sir Karl Popper's (1959) - *The Logic of Scientific Discovery* - a translation of his *Logik der Forschung,* published in 1934. It was this early work that contained the original criticisms that proved so influential in bringing about the rapid decline of logical positivism - rapid, that is, outside the domain of psychological theorizing.

criticisms of the logical positivist approach will be outlined before we in turn suggest a weakness in his own position. This weakness is revealed in the fact that although he argues that progress in science is in accordance with strictly rational principles, he himself is committed to the necessity of introducing psychological factors as being relevant in determining which scientific theories will survive. In short, Chapter 2 sees the emergence of non-rational elements into science.

Chapter 3 concentrates on presenting the views of two of the leading authorities in current philosophy of science, Thomas Kuhn and Paul K. Feyerabend. Both these thinkers, as noted above, emphasize the important role played by psychological and sociological variables in the development of science. We begin by presenting an overview of Kuhn's general approach. It is noted that although there is close agreement between Kuhn and Popper on a number of basic issues, Kuhn rejects both Popper's 'objectivism' and his advocacy of a theoretical pluralism. Indeed, Kuhn's characterization of science *qua* science is in terms of its adherence to ‘paradigms’, which demands a view of science, as essentially a theoretically monistic enterprise. Feyerabend has presented a radical criticism of the Kuhnian approach. He argues in favour of what he calls the "Principle of counter-induction". This principle advocates the introduction and elaboration of "hypotheses which are inconsistent with highly confirmed theories and with the evidence". (Feyerabend, 1970a, p275.) The chapter concludes in noting an important unresolved difficulty confronting both Kuhn's and Feyerabend's analyses of theoretical developments in science.

Chapter 4 illustrates how the ideas developed in chapters 2 and 3 throw light on the role of theory in psychology. B.F. Skinner among others has advocated the behaviouristic approach in psychology because of its alleged objectivity and scientific respectability. The main purpose of this chapter is therefore to concentrate on presenting and critically evaluating the ideas basic to Skinner's arguments in support of this approach. It will be contended that certain of Skinner's basic assumptions are

untenable and that in view of this the arguments he advocates in support of his radical behaviourist approach lack force. In arguing thus, it is claimed that although Skinner's views constitute a valuable alternative to those of the layman, they are nevertheless just as speculative or hypothetical.

Chapter 5 proposes a socio-psychological analysis of the nature and function of scientific theory. The proposal is based on the concept that theories in science possess an identical socio-psychological nature and function as do psycho-social norms. It is suggested that both are 'conjectured' or 'hypothetical' frameworks which function in making it possible for man to perceive his environment as structured, orderly, and predictable. The chapter opens with a presentation of ideas basic to Sherif's (1936) under-standing of the origins, nature and function of social norms. Similarities between these ideas and current views on the origin, nature and function of scientific theories are then identified. A brief outline of Berger's (1963, 1967) sociological viewpoint is then presented. It is suggested that since Berger's theoretical position can be viewed as involving an extension of Sherif's ideas, this viewpoint could be expected to generate insights into the social psychology of science and an attempt is made to identify some of these.

Chapter 6 presents a theory of scientific change. Using as a premise the socio-psychological view of science introduced in the preceding chapter, the idea that scientific change can be understood as an evolution of psycho-social norms is developed. It is claimed that so understood, scientific change can be analysed in terms of the social psychology of group norms. In other words, it is proposed that developments in science can be understood in terms of theories accounting for social change. The proposed theory is then contrasted with extant sociological approaches to the study of science. One recent development in theoretical social psychology is mentioned and, in terms of this, possible socio-psychological origins of innovations in

science are identified. The chapter closes with the contention that the theory of scientific change thus developed answers the difficulties raised at the end of Chapter 3.

Chapter 7 summarizes and concludes the essay.

CHAPTER 2

SCIENCE AND OBJECTIVITY

But as for certain truth, no man has known it. Nor will he know it, neither of the gods, nor yet of all the things of which I speak. And even if by chance he were to utter the final truth, he would himself not know it: For all is but a woven web of guesses.

Xenophanes

In a recent publication Serge Moscovici (1972) insists that the prevailing "scientific ideology" constitutes an obstacle to progress in theoretical social psychology. He maintains that one aspect of this ideology which is of particular importance is the predominance of a positivistic epistemology. Concerning this epistemology Moscovici writes:

> "Its main tenets are that facts are "given" in the environmental reality, that they can be inductively isolated through a description of regularities, and that experimentation is the hallmark of science. In this perspective, theory is a language and a tool which is both subordinate to the empirical method and subsequent to it chronologically".
>
> Moscovici, 1972, p.33

It is the writer's contention that Moscovici's criticism of the constraining influence exerted on current socio-psychological theorizing by the now dated positivistic philosophy of science is justified.

Logical positivism is a radically *empiricist* philosophy of science. Also important is the fact that it is, what Lakatos (1970) has called, a justificationist[8] epistemology - in this respect it may be likened to classical theories of knowledge. For centuries knowledge meant *proven* knowledge. Whereas the rationalist philosophers (e.g. Descartes, Leibniz) looked for the foundations of knowledge in the 'truths of reason', the empiricists (e.g. Locke, Berkeley, Hume) sought these foundations in experience.

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8 "According to the 'justificationists' scientific knowledge consisted of proven propositions". (Lakatos, 1970, p.93)

This justificationist approach is further evidenced in the attempts of the logical positivists to base scientific knowledge on firm foundations. As we have noted, the members of the Vienna Circle were united by the common concern of banishing any remnants of metaphysics from science (Ayer, 1936; 1959). At the same time they sought to develop positive arguments to support their view that science provided the one path to certain knowledge.

The positivists' programme immediately gave rise to the problem of how to delimit the boundaries of science. The solution to this problem was found in the infamous Principle of Verifiability. In one of its more primitive formulations this principle asserted "The meaning of a statement is the method of its verification". (Waismann, 1930, p.229.) In other words, a statement that cannot be verified is empty of cognitive[9] meaning and therefore cannot be said to be true or false, or 'known' to be true or false.

A later version of the principle asserts “A statement is held to be literally meaningful if and only if it is either analytic or empirically verifiable” (Ayer, 1936, p.9). Although there were many versions of the principal, common to them all was the claim “a genuine statement must be capable of *conclusive verification*”. (Schlick, 1931, p.150)

Now statements are provable, or verifiable, if and only if they can be either (i) deduced from the laws of logic and/or language (i.e. they are 'analytic' statements), or

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9 While denying that unverifiable statements lack any cognitive meaning, the positivists did not necessarily deny them all meaning. Their position was (see Ayer, 1936, 1959) that although religious, metaphysical, ethical and aesthetic statements lacked cognitive meaning, they could be *emotively* meaningful.

(ii) can be empirically demonstrated (i.e. they are 'empirical' statements). Examples of the first type of statement are "all objects are either black, or they are not black", "two plus two equals four", "all bachelors are unmarried men". Examples of the second type of statement are "copper oxide is green", "water boils at 1000 Celsius", "there are mountains on the moon".

The notion of a 'proof' was strictly understood in this context. A proof, by definition, consisted of a *finite* number of propositions which contained both premises and a conclusion. The conclusion was derived by using a finite number of strict logical transformations on one or more of the premises, the result of each such transformation constituting a line in the proof. The premises of arguments constituting a proof of an 'analytic' statement consisted of certain laws of logic and/or language. The premises of an argument constituting a proof of an empirical statement consisted of 'elementary' statements of experience, being called variously 'atomic propositions' (Russell, 1918; Wittgenstein, 1922), 'basic statements' (Ayer, 1936), 'protocol sentences' (Schlick, 1934; Neurath, 1932, 1933), and so on. Regardless of terminological differences, it was universally agreed that the truth-value of these elementary statements could be decided by simple observation.

Shortly after the formulation of the Verification Principle it was recognized that not only did it rule that metaphysical beliefs lacked cognitive meaning, but further, many principles considered central to science also lacked such meaning. For example, if viewed as unrestricted universal statements Newton's Laws of Motion were strictly meaningless according to this principle. With respect to this problem, Schlick writes:

> "... in asking for a logical justification of universal statements about reality ... we recognize, with Hume, that there is no such logical

> justification: there can be none, simply because they are not genuine statements".
>
> Schlick, 1931, p.156

This meant that such principles could at best be considered as heuristic devices, i.e. they became "prescriptions for the formation of statements" (Schlick, cited in Popper, 1959, p.37).

From the positivists' point of view scientific theories were, therefore, either the product of applying inductive logic to observation statements, or else an instrument (hence 'instrumentalism' - see Popper, 1959, 1963) or tool which the scientist used to derive predictions of future events. Yet the instrumentalist interpretation of the nature and function of scientific theory[10] is inconsistent with the positivistic credo that science and only science provides us with *knowledge*. Hence it became increasingly obvious that the positivistic philosophy of science had no alternative - it was committed to inductive logic as the essential component of scientific theory construction.

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10 Consistent with usage throughout this essay we use the term 'theory' in a very loose sense. It is used in a way that includes not only, for example, Copernican astronomy, Newtonian mechanics, etc as theories, but also, e.g. Newton's *laws of motion*, Galileo's *law of free fall*, hypotheses, universal statements of every kind, and even singular statements like "Gerald Ford is president of the United States". This is not consistent with general usage where 'theories', 'laws', 'hypotheses', 'principles', etc are usually distinguished. However, the present author considers these distinctions largely arbitrary, and that in the interests of increasing the generality of the present argument it is useful to ignore them. For a similar approach see Hanson (1958) and Feyerabend (1965, 1970b, 1970c). For a more detailed analysis of 'theories', 'laws', 'principles', etc from an instrumentalist point of view, see S. Toulmin (1953) and G. Ryle (1954).

Logical positivism as a philosophy of science was, then, committed to three basic principles:[11]

(i) the empirical (observational) base of scientific theories is itself theory-neutral and. therefore non-problematic.

(i) scientific theories are erected on this base with the help of inductive logic.

(i) any statement that cannot be verified is meaningless and, therefore, where a theory yields an un-provable prediction it must be ruled that the theory is non-scientific.

Each of these three principles is reflected in the philosophy of science of a number of recent socio-psychological texts. For example, following Kemeny (1959), Wrightsman (1972) maintains:

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11 Carnap (1950) saw *two* problems as being fundamental to philosophy of science. First, to establish a rigorous theory of meaning. Second, to establish a rigorous theory of induction. For Carnap an adequate theory of meaning would allow a linking of 'theoretical' terms with 'observational' terms so establishing the meaningfulness of theoretical terms and theoretical statements. Further, an adequate theory of induction would provide a method for deciding which of two statements (or theories) had better 'inductive support' and therefore which was the more scientifically acceptable. This idea that different theories might have different degrees of inductive support was largely developed in response to demonstrations (by Popper, 1934; etc.) that scientific theories were incapable of verification. Lakatos (1970) has called such an approach "neo-justificationist". He points out that just as theories are equally unverifiable so, considered as having an unrestricted domain of application, they are equally probable, or improbable.

> "... the scientific method involves three major steps. The first step is *induction* or the process of starting with *observed facts* (my emphasis) and constructing a theory that is consistent with those facts. Induction is a leap from some particular instances to a more general rule - a highly creative step since it involves dreaming up a set of principles that are adequate to account for the known facts. The second step is *deduction*. Constructing a theory that is consistent with some observed facts is not enough, since clever theory builders could probably develop different theories that would all explain the original facts. Thus the second step in the scientific method is a logical deduction of some additional consequences of the theory that was introduced in step one. These consequences are stated as predictions of what would happen under certain conditions, still at the abstract level. Thus an adequate theory must not only be verifiable but also falsifiable; we must be able to detect when its predictions are incorrect, too. The third step in the scientific method is *verification*".
>
> Wrightsman, 1972, pp.33-34

In the above passage Wrightsman assumes the following. First, the theoretical neutrality of the empirical (inductive) base. This is evidenced by his manifest belief in the theoretical autonomy of 'facts'. Science begins with an observation of the facts. Theories are then constructed that are consistent with these facts. Theories are tested in terms of how well they agree with the facts, and so on. This assumption is equivalent to the positivists' principle that the empirical or inductive base is theory-neutral.[12]

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12 See also: Stotland and Canon: *Social Psychology: A Cognitive Approach*. Saunders, 1972. These authors, in describing the methods of the social researcher, write: "It is from the observation of overt behaviour that his inferences stem and on the basis of which their validity is established or denied". (p.25) Becker and McClintock: "Scientific theory and social psychology" in McClintock (ed.) *Experimental Social Psychology* Holt, Rinehart and Winston, 1972, write: "In order to determine whether or not a theory is valid, the model or the theorem is compared with observed facts." We might also cite Crano and Brewer, 1973; Hendrik and Jones, 1972; Campbell, 1969; and many others.

Second, the dependence of science on the inductive method.[13] Third, that scientific theories are verifiable.

To be fair to Wrightsman it must be pointed out that he seems to equate verification with confirmation. This is evidenced in his idea that verification involves the collecting of "new observations to *support* or refute the predictions made during the deductive step". (Wrightsman, 1972, p.34; my emphasis)

Of the three basic tenets of logical positivism, doubtless it is the first that has the most pervasive influence on not only socio-psychological theorizing, but also the theorizing of philosophers of science. (Feyerabend., 1962, 1965, 1970b.) However it is not an assumption unique to positivism. It is an assumption basic to all justificationist theories of knowledge. Although rarely understood (Lakatos, 1970), it is this assumption that is as much a target of Popper's (1934) critique of positivist philosophy of science as their advocacy of induction and the verification principle. In characterizing his own standpoint Popper writes:

> "My point of view is, briefly, that our ordinary language is full of theories; that observation is always *observation in the light of*

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13 See also: E.L. Walker: *Psychology as a Natural and Social Science*, Brooks/Cole, 1970. In his highly readable little book *Psychology as Science and Art*, Harcourt Brace Jovanovich, 1972, James Deese writes: "To some philosophers induction does not seem to have any thoroughly rational basis. Therefore, because science is said to be rational and logical, induction remains a kind of whale among fishes in the philosophy of science. But no matter what their views are on the philosophy of science, all scientists accept on faith the proposition that knowledge may be increased through induction. The need to generalize from particular facts to universal laws is the sole reason why science is not purely rational. Induction distinguishes science from mathematics". (*Ibid.* p.6)

> *theories* that it is only the inductivist prejudice which leads people to think that there could be a phenomenal language, free of theories, and distinguishable from a 'theoretical language';"
>
> Popper, 1959, p.59

In his radical critique of the claim that inductive logic is fundamental to scientific theory construction it is important to recognize another important difference between Popper and the positivists.

We have seen above that from the positivistic standpoint a theory could only be scientific if its application was confined to a restricted domain in space and time (see footnote 1 above). Only then was it possible to verify its general statements. Popper rejected such a restriction of the explanatory potential of science and endorsed Einstein's sentiment that, "The supreme task of the physicist is to search for those highly universal laws ... from which a picture of the world can be obtained by pure deduction" (Einstein, 1935, p.125). For Einstein and Popper, these universal laws that science sought to uncover were statements of unrestricted generality. In other words, they were true in every region of space and time. Popper maintained that the 'universal' statements of a positivistic science were strictly 'singular'[14] statements in that they are logically equivalent to a conjunction of a finite number of singular statements.

How did one arrive at these "highly universal laws" spoken of by Einstein and Popper? According to Einstein, "There is no logical path leading to these ... laws. They

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14 A singular statement is a statement that refers to a single or finite collection of particulars located in a statements in that they are logically equivalent to a conjunction of a finite number of singular statements. finite spatiotemporal region. Examples of singular statements are "The man in this room is over 60 years old", "The five black cows in this paddock are too fat", and "The molecules in this container of sulphuric acid do not contain any carbon atoms".

can only be reached by intuition, based upon something like an intellectual love of the objects of experience" (Einstein, 1935, p125). The question then arises as to whether *scientific* theories are any different to those of metaphysics, aesthetics, and so on. If scientific theories are arrived at by "intuition", if they are the product of a search motivated by some highly subjective "intellectual love", then are they any different from metaphysics and myth?

This question Popper posed himself even before there was any Vienna Circle. It was the year 1919 "when I first began to grapple with the problem, 'When should a theory be ranked as scientific? or 'Is there a criterion for the scientific character or status of a theory?'" (Popper, 1963, p.33.)

It is important to note that this question to which Popper addressed himself right from the outset, is a fundamentally different problem from those considered by the positivists. Whereas the latter were basically concerned with whether a theory was *meaningful* and whether it was *true*, Popper attempted to find a criterion that would serve in the demarcation between *science* and *pseudo-science*:

> "The problem which troubled me at the time was neither, 'When is a theory true?' nor, 'When is a theory acceptable?' My problem was different. *I wished to distinguish between science and pseudo-science* knowing very well that science often errs, and that pseudo-science may happen to stumble on the truth".
>
> Popper, 1963, p.33

According to Popper then, while theories in metaphysics, theology, ethics, etc might not be 'scientific' theories, they could nevertheless be both meaningful and true.

Regarding the period immediately after the First World War and the events leading up to his concentration on the 'problem of demarcation', Popper writes:

> "After the collapse of the Austrian Empire there had been a revolution in Austria: the air was full of revolutionary slogans and ideas, and new and often wild theories. Among the theories which interested me Einstein's theory of relativity was no doubt by far the most important. Three others were Marx's theory of history, Freud's psychoanalysis and Alfred Adler's so-called 'inductive psychology' . ... It was during the summer of 1919 that I began to feel more and more dissatisfied with these three theories - the Marxists theory of history, psychoanalysis, and (Adler's) individual psychology, and I began to feel dubious about their claims to scientific status. My problem perhaps **[30]** first took the simple form, 'What is wrong with Marxism, psychoanalysis and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially the theory of relativity? ... What worried me was neither the problem of truth, at that stage at least, nor the problem of exactness or measurability. It was rather that I felt that these other three theories, though posing as sciences, had more in common with primitive myths than with science; that they resembled astrology rather than astronomy".
>
> Popper, 1963, p.34

The criterion of demarcation that Popper proposed is found in his famous Principle of Falsifiability; a theory is to be counted as scientific if and only if it can, in principle, be falsified.

Popper states:

> "I shall not require a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by

> means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience...*"
>
> Popper, 1959, pp.40-41

Popper believed the non-scientific nature of the Marxist theory of history and the psychological theories of Freud and Adler could be demonstrated, in terms of this principle of falsifiability. He maintained that insofar as these theories could be reconciled with any observation whatsoever, they could *not* be falsified.

He contrasts the situation with that surrounding Einstein's theory of general relativity, which led to the prediction (confirmed by Eddington) that light rays passing close to the sun would be deflected by its gravitational pull.

> "As a consequence it could be calculated that light from a distant fixed star whose apparent position was close to the sun would reach the earth from such a direction that the star would seem to be slightly shifted away from the sun; or, in other words, that stars close to the sun would look as if they had moved a little away from the sun; and from one another ... Now the impressive thing about this case is the risk involved in a prediction of this kind. If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain possible results of observation* - in fact with results which everybody before Einstein would have expected. This is quite different to the situation I have previously described, when it turned out that the theories in question were compatible with the most divergent behaviour, so that it was practically impossible to describe any human behaviour that might not be claimed to be a verification of these theories".
>
> Popper, 1963, p.36

To describe the *logic* of falsification we present in brief outline the standard analysis of the logic of scientific explanation.[15]

Suppose we are given, as a hypothesis, a theory T which contains at least one unrestrictedly universal statement of the form: All X are Y. As an illustration, we let T = All living organisms require oxygen. Given T it is possible to deduce that any living organism will die in the absence of oxygen. The logical schema for this can be set out as follows:

Let theory T = All living organisms require oxygen

Let statement A = This living organism is without oxygen

And statement B = This organism will die

Then: T entails (if A, then B)

By simple logic (the rule of 'importation'), this is equivalent to

(T and A) entails B

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15 More expanded discussions can be found in: E. Nagel *The Structure of Science,* London: Routledge Kegan Paul, 1961. C.G. Hempel *Philosophy of Natural Science,* New Jersey: Prentice Hall, 1966. C.G. Hempel "Explanation in Science and History" in E.G. Colodny (ed.) *Frontiers of Science and Philosophy,* University of Pittsburgh Press, 1962, pp.9-33.

Here we have the logical schema of 'explanation' and 'prediction' in science. Given the truth of A, we may, on the basis of T, *predict* the occurrence of B. Given the occurrence of B, we may, on the basis of T, *explain* this occurrence of B, should we be able to point to the truth of A.

Now the logic of falsification is demonstrated when a *prediction* generated as above turns out to be false. By the rule of *Modus Tollens*[16], if B turns out *false*, by a simple application of de Morgan's Laws[17], it follows that *either* T is false *or* A is false. Assuming that A is true, it follows that T is false[18].

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16 *Modus Tollens* is the common name for the argument-form:

If A, then B
Not-B
Therefore Not-A

The validity of such an inference is to be contrasted with the invalidity of the argument-form:

If A, then B
B
Therefore A

An argument of this form commits the fallacy of 'asserting the consequent'. In other words, while a false prediction can show a theory false, a true prediction cannot show that theory true! The asymmetry reflected in the logic of these argument-forms is basic to Popper's endorsement of falsificationist philosophy of science. See also D.T. Campbell, 1969, p.353.

17 There are two logical laws known as "de Morgan's Laws". One says that (p and q) is false, if and only if *either* p is false *or* q is false. The other says that (p or q) is false, if and only if *both* p is false *and* q is false.

18 Experimentation as a method of testing a theory obviously fits in the above schema. For example, a theory predicts that given a certain set of antecedent experimental conditions (conjoined to constitute 'A'), a certain consequent B will be observed. If B fails to eventuate this is considered a failure for the theory.

Now Popper's idea that "*the criterion of the scientific status of a theory is its falsifiabiljty* or *refutability* or *testability*" (Popper, 1963, p37) is widely shared by both social and non-social scientists, as well as many prominent philosophers of science. For example, Stotland and Canon state:

> "... theories can be disproved. In fact this is one of the hallmarks of a scientifically useful theory. It must delineate relationships in such a way that specific predictions may be made which are amenable to empirical test and which are capable of disproof. An explanation which cannot be tested is useless as it provides no means by which it can be properly evaluated. Disproof may take the form of a demonstration that the theory does not adequately account for all the events to which it is pertinent or that it leads to predictions which are inaccurate".
>
> Stotland and Canon, 1972, p.3

Similarly we find in Krasner and Ullman's *Behaviour Influence and Personality* that

> "theories are tested by contact with the world they describe ... a good theory leads to hypotheses that can be tested. Theories that do not permit contact with reality by having potential for proof or disproof are too vague to be used".

Endorsing the use of 'testable' theories[19] in social psychology Tajfel (1972) writes:

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19 For further expressions of similar views in socio-psychological writings see Lindgren (1969), Hendrick and Jones (1972), McClintock (1972), Rosenblatt and Miller (1972).

> "I believe that, amongst the approaches to social behaviour open to us, theories which can be tested experimentally contain the least doubtful promise for the future".
>
> Tajfel, 1972, p.69

The same author goes on to state:

> "There is no reason why socio-psychological. theories - or at least some of the hypotheses derived from them - cannot be tested in experimental settings, and there are good reasons why they should be".
>
> Tajfel, 1972, p.77

Yet, as pointed out in the Introduction, the testing of a theory is no simple matter. From the fact that a theory is used to derive predictions that turn out *not* to coincide with what one observes, it does *not* follow that the theory is false. Inaccurate predictions may be due to not having taken into account *all* the relevant antecedent conditions when making ones calculations. Alternatively they could be a result of an *error* in measurement or calculation. More importantly, as in the case of the Aristotelians who observed the falling stone to drop without any horizontal movement, (so 'refuting' the Copernican theory), it could be that the troublesome *observation* should be re-examined.

Popper himself recognizes that it is always a possibility that one can so rescue a theory from refutation by recalcitrant observations. Discussing this he writes:

> "It might be said that ... it is ... impossible, for various reasons, that any theoretical system should ever be conclusively falsified. For it is always possible to find someway of evading falsification, for example by introducing *ad hoc* an auxiliary hypothesis, or by changing *ad hoc* a definition. It is even possible without logical inconsistency to adopt the position of simply refusing to acknowledge any falsifying experience whatsoever. Admittedly, scientists do not usually proceed in this way, but logically such procedure is possible; and this fact, it might be claimed, makes the logical value of my proposed criterion of demarcation dubious, to say the least".
>
> Popper, 1959, p.42

Needless to say Popper claims that this 'difficulty' does not constitute an insurmountable obstacle to acceptance of his falsifiability principle. In an attempt to illustrate the reasoning basic to Popper's defence of this claim we turn to another story in the history of science.

In 1687 Sir Isaac Newton published his famous theory of gravitation. Even in Newton's day this theory was demonstrated to be capable of accounting for the observed orbits around the sun of the six then known planets. When in 1781 Sir William Herschel discovered Uranus serious attempts were made to calculate its orbit around the sun using Newton's theory. Repeatedly, each of these attempts ended in failure. Even after 50 years, no successful calculations had been made.

In the l830's a number of hypotheses were entertained as possible explanations for the 'anomalous' behaviour of this planet.[20]

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20 For an exhaustive historical study of this case see Morton Grosser: *The Discovery of Neptune*, Harvard University Press, 1962.

1. That its motion was subject to interference by Descartes' 'cosmic fluid'.

2. That its motion had been disturbed by a large comet passing by.

3. That there existed another *unobserved* planet having an orbit probably outside that of Uranus. The unknown gravitational influence introduced by this planet created perturbations in the movements of Uranus - hence its anomalous and unpredictable motion.

4. That Uranus had a large moon whose gravitational pull interfered with its own motion (as in 3 above).

5. That Newton's theory was false.

So even after 50 years of the theory's failure to generate successful predictions, the idea that it might be false was only one of five alternative explanations of this failure.[21]

In short, for half a century the theory had lead to false predictions and even then there were few who considered the theory had been 'falsified'.

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21 It is often overlooked that an *experimental* test of a theory always requires the assumption of the *'ceteris paribus'* clause (Lakatos, 1970). Because of this it is always possible to reject the negative outcome of an experimental test by denying that the *ceteris paribus* condition has been met. For example, one can explain away a negative experimental result maintaining that it was due to 'uncontrolled variables' interfering with the process or effect under examination.

Popper would claim that this example does not constitute much of a problem. He never *meant* that a theory could be refuted or falsified by just any recalcitrant observation but only by those that the scientist had previously 'agreed' he would accept as a potential falsifier. In short, the falsification of a theory is only possible if *scientists* have made certain 'decisions'. It is at this point that 'conventionalist' elements enter into Popper's philosophy of science. Although Popper believes that such a conventionalism creates no difficulty for his falsification criterion of demarcation, the arguments concluding this chapter are designed to demonstrate the untenability of this belief.

In defining his understanding of 'falsification' Popper writes:

> "I propose the following definition. A theory is to be called 'empirical' or 'falsifiable' if it divides the class of all possible basic statements unambiguously into the following two non-empty sub-clauses. First, the class of all those basic statements with which it is inconsistent (or which it rules out, or prohibits): we call this the class of the *potential falsifiers* of the theory: and secondly, the class of those basic statements which it does not contradict (or which it 'permits'). We can put this more briefly by saying: a theory is falsifiable if the class of its potential falsifiers is not empty".
>
> Popper, 1959, p.86

In other words 'potential falsifiers' consist of basic statements[22] which, because "Basic statements are accepted as, the result of a decision or agreement; and to that extent ... are conventions" (Popper, 1959, p.106), which means that:

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22 Popper identifies 'basic statements' with '*singular* existential statements', i.e. a *singular* there-is statement. These statements have the logical form 'There is a so-and-so in the space-time region k' (e.g. 'There is a black raven in this room') or 'such and such an event is occurring in the region k'. See Popper, 1959, Section 28, pp.l00-l05.

> "Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other which we *decide to accept*. If we do not come to any decision, then the test will have led nowhere".
>
> Popper, 1959, p.104

To repeat, there can be no ultimate basis on which one decides to accept a statement as basic. It can only be decided on the basis of convention. Basic statements, like all other statements, are theory-laden and therefore open to question. It follows then that 'scientific' theories are not refuted by theory-neutral facts, but by *what scientists agree to call the facts*.

This last point follows from Popper's thesis that all observation is theory-dependent[23] and hence that even observation-statements are not theory-neutral. Popper writes:

> "every statement has the character of a theory, of a hypothesis. The statement, 'Here is a glass of water' cannot be verified by any observational experience. The reason is that the *universals* which appear in it cannot be correlated with any specific sense-experience . ... By the word 'glass', for example, we denote physical bodies which exhibit a certain *law-like* behaviour, and the same holds for the word

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23 In a recent statement of his position on this point Popper writes: "I see even our physical bodies and our sense organs as being, for the most part, something like frozen theories. Our sense organs, and our physical bodies, are the result of trial and error elimination. The process of trial and error elimination characterizes both the evolution of our bodies and sense organs, where the error elimination is by natural selection, and the growth of our knowledge where elimination can take place by means of critical discussion". (Popper, 1974, p.92)

> 'water'. Universals cannot be reduced to classes of experience, they cannot be 'constituted'."
>
> Popper, 1959, p.94

Lakatos (1970) has drawn attention to the distinction between (i) falsification as a process in which a theory is shown to be refuted by theory-neutral facts, and (ii) "falsification" as a process in which a theory is shown to be "refuted" because it conflicts with the "facts". Lakatos (1970) termed the former brand of falsificationism "dogmatic falsificationism" and the latter "methodological falsificationism". In terms of this distinction then, Popper is a methodological falsificationist[24]. He views the falsification of a theory as involving more than just a demonstrated inconsistency between observation and predictions derived from the theory; an element of decision is also involved.

As we have already pointed out Popper recognizes that it is always logically possible to reconcile a recalcitrant observation with a given theory by introducing 'auxiliary hypotheses'. Yet, Popper maintains, a decision to so rescue a theory from refutation must not violate the rules laid down by what he calls the "empirical method".

> "According to my proposal, what characterizes the empirical method is its manner of exposing to falsification, in every conceivable way, the system to be tested. Its aim is not to save the lives of untenable systems but, on the contrary, to select the one which is by

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24 For dogmatic falsificationist views in socio-psychological writings, see Crano and Brewer (1973), Stotland and Canon (1972), Wrightsman (1972), Krasner and Ullmann (1973). The methodological falsificationist standpoint is rarely to be seen. However, see Guthrie (1950), and Moscovici (1972).

> comparison the fittest, by exposing them all to the fiercest struggle for survival."
>
> Popper, 1959, p.42

In short, auxiliary hypotheses introduced to save a theory from refutation must not be *ad hoc*[25] but rather must lead to an *increase* in empirical content. This means that if an auxiliary hypothesis is to be acceptable it must not only account for the negative outcome of earlier predictions, *it must give rise to novel predictions*.

We can illustrate this content-increasing nature of an auxiliary hypothesis by returning to the case discussed above - the story surrounding the anomalous behaviour of Uranus. Each of the four auxiliary hypotheses introduced to reconcile the observed motion of Uranus with Newton's theory of gravitation are to be a greater or lesser extent content-increasing. The cosmic fluid hypothesis would lead one to expect that with more precise measurement techniques a discrepancy between observed and predicted motions would be observed for all planets. The hypothesis that Uranus had an unobserved moon would lead one to expect (predict) that such a moon would be observed given more powerful telescopes.

Likewise with the unobserved planet hypothesis. In fact it was on the basis of this hypothesis that the French astronomer Leverier and the British astronomer Adams

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25 An *ad hoc* auxiliary hypothesis is one that introduces no new empirical content to the theory, i.e. it leads to no *new* predictions. As an example we might cite Galileo's Law of Circular Inertia. This law introduced to reconcile the Copernican theory with the observation that a stone falling from the tower of Pisa dropped "vertical and straight downwards" led to no predictions that allowed an observational test between it and the Aristotelian theory.

were able to derive predictions that facilitated their discovery of the planet Neptune. This discovery turned what had been a failure for Newton's theory into an outstanding success (Hanson, 1962), yet a success that would have not been possible without the help of the auxiliary hypothesis.

Returning once more to the discussion of Popper's "empirical method" we note the following. First, Popper's principle of falsifiability was designed to demarcate scientific from non-scientific *theories*. Second, in terms of Popper's analysis of falsifiability, in the absence of decisions made by scientists, *no* theory can be refuted. Third, *decisions made by scientists determine* the class of basic statements, hence the potential falsifiers, and therefore ultimately *what theory is open to falsification and hence scientific*. Fourth, an attempt to rescue a theory from refutation by introducing an *ad hoc* or a content-increasing auxiliary hypothesis is merely collateral to the verisimilitude of the theory in question. *It has a lot more to do with the scientists* making the attempt in particular, his intelligence, his professional skills, his motivation, etc.[26]

In short, the 'scientific' status of theories may be jeopardized only where their exponents or *defenders* violate the prescriptions laid clown by the 'empirical method'.

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26 It may well be little more than an historical accident that theories in physics have had defenders who showed great ability in devising content-increasing auxiliary hypotheses to *advance* their theories, while astrology has been landed with defenders lacking the necessary ability, intelligence, etc to do anything other than rely on ad hoc auxiliary hypotheses. No doubt such a story greatly over-simplifies the situation but it probably serves to get the basic point contained in the text across.

It follows from all this that Popper's falsifiability principle fails in its purpose as a means of distinguishing between scientific and non-scientific theories.[27]

Indeed Popper has prescribed a methodology of science that, should it be followed, means that advancements in science are contingent upon which theory has the adherents with, for example, the greater intelligence, the greater ingenuity, the more resources at their disposal, and so on, and so on. In other words Popper's evolutionary model of the growth of scientific knowledge as involving the rivalry of *theories* in which the principle of the "survival of the fittest" operates, is incomplete. In this struggle for survival it is not so much *theories* that compete for survival but the *scientists* who develop, maintain, and use the theories. It follows that the theory which survives in this struggle is largely determined by not only logical considerations but also historical, geographical, economic, sociological, psychological factors, etc. This line of reasoning takes the stress away from the philosophy of science (and of social science), as such, to the psychology of the practicing scientist. It is this argument that will be developed in the next chapter.

In conclusion we might summarize what we discovered regarding the views of the positivists and Popper. First, they both agree on the objectivity of scientific knowledge.

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27 Kuhn also makes this point. He writes “What is falsification if it is not conclusive disproof? Under what circumstances does the *logic* of knowledge require a scientist to abandon a previously accepted theory when confronted, not with statements about experiments, but with experiments themselves? Pending clarification on these questions, I am not sure that what Sir Karl has given us is a logic of knowledge at all.” (Kuhn, 1970, p.15)

Second, the positivist's principle of verifiability was intended not only to demarcate between science and non-science, but also between meaningful and non-meaningful statements. Popper was concerned only to formulate a principle by which it was possible to demarcate between science and non-science only. Third, the theories of the positivist although general in scope apply to only a restricted domain and are therefore in principle verifiable. For Popper, theories cover an unrestricted domain and hence are incapable of verification, although they might be 'falsified'. Fourth, for the positivist, theories are arrived at by induction; for Popper they are the product of pure guesswork, imagination, etc. Fifth, for the positivist careful observations mirror reality; for Popper the contents of observation are a function of the scientist’s presuppositions, his assumptions, theories etc.

In short, while agreeing with Popper's critique of positivism and his thesis regarding the dependency of observation on theory, the present author rejects his thesis that science is a source of *objective* knowledge. If was argued that Popper's denial of the existence of a theory-neutral empirical base on which knowledge could be founded suggested the importance of subjective elements as basic determinants featuring in the fight for survival of scientific theories. This argument is developed further in the following chapter.

CHAPTER 3

SCIENCE AND SUBJECTIVITY

He who does not expect the unexpected, will not detect it: for him it will remain undetectable, and unapproachable.

Heraclitus

The previous chapter was introduced with a quote from a recent article by Serge Moscovici (1972). A little further on in this same article Moscovici feels constrained to admit, "social psychology is not truly a science". His complaint seems to be that:

> "... a solid foundation for the future has not been laid ... the domain of the subject is split into 'topics', 'clans', 'schools', and 'establishments' which each have their own method of asking questions, their own language and their own interests; what is more, each develops out of its peculiarities its own criteria of truth and excellence".
>
> Moscovici, 1972, p.32

While in the preceding chapter we shared Moscovici's sentiments concerning the inhibiting effect positivistic ideas are still having on socio-psychological thought, reason will be given for rejecting the sentiments expressed in the above passage. It will be argued that the very characteristics Moscovici considers to demonstrate the *non-scientific* status of socio-psychological theory and research are in fact the very features that contribute to its development as a science. In other words, this thesis argues that it is to social psychology's advantage that it has "no unified field of interest", "no systematic framework of criteria and requirements", "no coherent body of knowledge" and "no common perspectives".

We begin with a presentation of the views of Thomas Kuhn (1962, 1970). This has a three-fold purpose. First, it has largely been through Kuhn's work that increasing recognition is now being given to the relevance of historical, sociological and psychological factors to an understanding of developments internal to science.

In this respect it will be seen that there is a marked similarity between Kuhn's ideas and the theme of the concluding arguments in the preceding chapter. Second, Kuhn's views provide a useful framework in which to *develop* the line of argument contained in the aforementioned conclusion. While the present chapter concentrates more on an exposition of the epistemological aspects of Kuhn's views, Chapter 6 attempts a *socio-psychological* explanation of scientific change and here Kuhn's views will be re-examined. Third, socio-psychological theorizing and research is often criticized along the lines expressed in the quote from Moscovici. Insofar as Moscovici is largely reflecting a Kuhnian appraisal of the state of affairs in this field, (see footnote 28), in criticizing Kuhn's views we are thus defending socio-psychological theory and research from this criticism.

Like Popper, Kuhn insists "an analysis of the development of scientific knowledge must take account of the way science has actually been practiced" (Kuhn, 1970, p4). In his *The Structure of Scientific Revolutions* Kuhn outlines the history of ideas relating to such phenomena as light, electricity, and physical optics. A characteristic feature of the development of research in these areas is what he describes as a transition from "immature" to "mature" science. With reference to these developments Kuhn writes:

> "... anyone examining a survey of physical optics before Newton may well conclude that, though the fields practitioners were scientists, the net result of their activity was something less than science. Being able to take *no common body of belief* for granted, each writer on physical optics felt forced to build his field anew from its foundations. In doing so his choice of supporting observation and experiment was relatively free, for there was *no standard set of methods or of phenomena that every optical writer felt forced to employ and explain.* Under these circumstances, the dialogue of the resulting books was often directed as much to members of *other schools* as it was nature. That pattern is not unfamiliar in a number of creative fields today, nor is it incompatible with significant discovery and invention. It is not,

> however, the pattern of development that physical optics acquired after Newton and that other natural sciences make familiar today."
>
> Kuhn, 1962, p.13, my emphasis[28]

Implicit in the passage above is the distinction basic to Kuhn's analysis of the historical development of science. For Kuhn the history of science can be divided into two kinds of period, the periods of "normal science" and the periods of "extra-ordinary science". By normal science Kuhn understands -

> "research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice."
>
> Kuhn, 1962, p.10

Normal science is science carried on amongst a community of scientists who share a "paradigm" - a term used by Kuhn 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 . ... Acquisition of a paradigm and of

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28 Notice in this passage the clear similarities between Kuhn's characterization of 'immature science' (emphasized) and the features Moscovici draws attention to in his comment on current socio-psychological theorizing and research.

> the more esoteric type of research it permits is a sign of maturity in the development of any given scientific field".
>
> Kuhn, 1962, pp.l0 -11

It is important to recognize at this point fundamental similarities and differences between Popper's and Kuhn's theories of knowledge. As already mentioned Popper's falsificationism is not a justificationist theory of knowledge - for Popper knowledge has no foundations, neither in experience nor reason. On this point he and Kuhn are in agreement. Essentially what distinguishes Popper's views from Kuhn's is that for Kuhn knowledge is never strictly objective. In particular, scientific knowledge, arising out of research in periods of normal science, always possesses subjective elements. This is because the paradigm itself is largely socially constructed and therefore its knowledge is always grounded in man's social condition. Kuhn writes:

> "Observation and experience can and must drastically restrict the range of admissible scientific belief, else there would be no science. But they cannot alone determine a particular body of such belief. An apparently arbitrary element compounded of personal and historical accident, is always a formative ingredient of the beliefs espoused by a given scientific community at a given time."
>
> Kuhn, 1962, p.4

It is the existence of a paradigm[29] that makes possible the practice of normal science. Characteristic of pre-paradigmatic or immature science are frequent and deep disagreements between different schools of thought over what constitutes legitimate

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29 Examples of paradigms in the history of science are Aristotelian dynamics, Newtonian mechanics, wave optics, and classical electromagnetism.

problems, methods and standards of solution. Although these disagreements do not completely disappear with the emergence of a paradigm they are almost non-existent during periods of normal science. The periods of normal science are characterized by a "puzzle-solving" tradition. Due to a commitment to the theoretical assumptions basic to a paradigm, along with its methods, problems and standards of solution, the scientist can afford to concentrate his research on puzzle-solving activities and to avoid time-consuming disputes over fundamentals. In other words, in periods of normal science the scientist's puzzle-solving activities are primarily concerned with expanding the range of phenomena amenable to analysis and explanation in terms of the theory to which he is committed, rather than questioning the adequacy of the theory *qua* a description of nature.

> "... the existence of this strong network of commitments - conceptual, theoretical, instrumental, and methodological - is a principle source of the metaphor that relates normal science to puzzle-solving. Because it provides rules that tell the practitioner of a mature specialty what both the world and his science are like, he can concentrate with assurance upon the esoteric problems that these rules and existing knowledge define for him. What then personally challenges him is how to bring the residual puzzle to solution. In these and other respects a discussion of puzzles and of rules illuminates the nature of normal scientific practice".
>
> Kuhn, 1962, p.42

Contemporaneous with periods of normal science there will generally exist some puzzles which, even after long periods of subjection to the concerted puzzle-solving efforts of scientists, prove extremely resistant to solution. It happens that under certain conditions - nowhere clearly specified by Kuhn - these anomalies could provoke a crisis. In such periods the basic adequacy of the paradigm is called into question. Often such a crisis is resolved only by the emergence of a new paradigm.

It is in his understanding of the logical relationship between the paradigm prevailing *prior* to a revolution and the paradigm that emerges *during* the revolution that we find Kuhn's most radical thesis: the thesis that these two paradigms are 'incommensurable'. By this Kuhn[30] means that because the concepts basic to one paradigm cannot be defined with the use of concepts found in the other, it is not possible to translate the language of the one into the language of the other. Consequently, with respect to degrees of verisimilitude, empirical content, or whatever, the 'knowledge' conceptualised in terms of the one *cannot be compared with* the 'knowledge' conceptualised in terms of the other.

As examples of two incommensurable theories we might cite Newtonian mechanics and Einstein's special theory of relativity. Depending on which of these two theories is used, we find that such basic terms as 'mass', 'velocity', and 'time' are differently understood. Referring to this Feyerabend writes:

> "Assume that an explanation is required, in terms of the special theory of relativity, of the classical observation of mass in all reactions in a closed system S. If m′, m″, m‴,…, m^i,... are the masses of the parts P', P'', P''',..., P^i, ..., of S, then what we want is an explanation of Σm^i = const. for all reactions inside S. We see at once that the consistency condition cannot be fulfilled. According to special relativity Σm^i will vary with the velocities of the parts relative to the co-ordinate system in which the observations are carried out, and the total mass of S will also depend on the relative potential energies of the parts ... Now let us turn to the meanings of the terms in the relativistic law, and in the corresponding classical law. The first indication of a possible change of meaning may be seen in the fact that in the classical case, the mass of an aggregate of parts equals the

30 See also Feyerabend, 1970c, pp.219-224.

> sum of the masses of the parts: $M(\Sigma P^i) = \Sigma M(P^i)$. This is not valid in the case of relativity, where the relative velocities and potential energies contribute to the mass balance. That the relativistic concept and the classical concept of mass are very different indeed becomes clear if we also consider that the former is a *relation* involving relative velocities, between an object and a co-ordinate system, whereas the latter is a *property* of the object itself and independent of its behaviour in co-ordinate systems".
>
> Feyerabend, 1965, pp.168-169

Like Popper (1959, 1963), Kuhn maintains that observation is theory-dependent. As a consequence of conjoining this thesis with the incommensurability thesis it follows that where scientists operate within different paradigms they will view reality differently.

> "... the proponents of competing paradigms practice their trades in different worlds. ... Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction. Again that is not to say that they can see anything they please. Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they see them in different relations one to the other. That is why a law that cannot even be demonstrated to one group of scientists may seem intuitively obvious to another. Equally it is why, before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, *the transition between competing paradigms cannot be made a step at a time forced by logic and neutral experience. Like the gestalt switch it must occur all at once (though not necessary in an instant) or not at all.*
>
> Kuhn, 1962, p.150, my emphasis

It was noted above that where two theories are incommensurable it follows that it is impossible to compare their respective degrees of verisimilitude. In other words it is

not possible to decide which theory better approximates to the truth. It follows then, in terms of their degrees of truth content, there is no possible *rational justification* for choosing one theory rather than the other. For example, taking the two theories discussed above, Newtonian mechanics and Einstein's special relativity, there is no possible way of establishing that the latter describes reality any more truthfully than the former. Hence scientific revolutions do not necessarily entail any progress in man's search for truth.

This aspect of Kuhn's thinking contrasts radically with Poppers views which maintain that *progress* in science is essentially *guaranteed* by its rational character. With respect to the example above, Popper maintains that special relativity constitutes an *advance* on Newton's mechanics in that it has greater empirical content. Popper would claim this can be seen in that one can derive both Newtonian mechanics from special relativity, as well as predictions that do not follow from Newtonian mechanics but nevertheless stand confirmed. In short, Popper would maintain that since special relativity generates both Newtonian mechanics (as a limiting case with velocity tending to zero) as well as novel predictions that stand confirmed, it has greater truth content. In view of this it is rational to choose special relativity rather than Newtonian mechanics.

Yet clearly Popper's comparison of the degrees of truth content of special relativity and Newtonian mechanics overlooks the problem of incommensurability. As we saw above, it is *not* possible to derive Newtonian mechanics from special relativity. Popper's attempt to compare their respective degrees of truth content[31] therefore fails in that it rests on a false assumption.

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31 It is interesting to critically examine Popper's notion of a 'content-increasing auxiliary hypothesis' in the light of incommensurability. Consider two incommensurable theories T and T′. Now adding a content-increasing auxiliary hypothesis to T, *provided*

For the same reasons that he rejects Popper's thesis that scientific knowledge develops in accordance with rational principles, Kuhn is committed to a rejection of Popper's characterization of scientific theories as 'falsifiable' theories. A theory can only be falsified if it can be *demonstrated* that, in at least some respects, it is not true. Yet such a demonstration always rests on certain theoretical assumptions. Consequently the end product of any demonstration is open to question. In view of incommensurability it becomes pertinent to question whether the demonstration challenges[32] the theory - or whether the theory challenges the demonstration (as happened with Galileo's critique of the arguments against Copernicus centering around the demonstration at the tower of Pisa).

While Kuhn's philosophy of science seriously challenges the *logic* of Popper's falsificationist views, his *historiography* of science also calls into question the historical accuracy of Popper's views. As we have seen, Popper considers revolutions take place when the prevailing theory is confronted by an alternative possessing greater empirical content.

Popper maintains that if one scientific theory is to replace another it must be able to account for the successes of the old theory as well as display new successes of

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it leads to novel predictions that are confirmed, *subtracts* from the truth-content of T′. However adding a content-increasing hypothesis to T′, *provided* it leads to novel predictions that are confirmed *adds* to the *truth*-content of T′. It follows that added *empirical* content does not entail increased *truth*-content. This means that Popper's empirical method does not serve as a guarantee to progress in science, science being viewed as the search for truth. Although it might not strike the reader as being obvious, this is essentially the same argument as developed in the latter part of Chapter 2.

32 Where the relevant theories are incommensurate it is even more problematic. Since sentences in the one cannot be translated into sentences of the other it follows that they cannot be *inconsistent* with each other. This problem is raised later in the present chapter.

its own. In such a case the empirical method requires the scientist to adopt the new theory. Should the new theory demonstrate lesser empirical content, then the scientist, *qua* scientist, is prohibited from adopting it. Yet in his description of what actually happens Kuhn writes:

> "*The man who embraces a new paradigm* at an early stage *must often do so in defiance of the evidence* provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the *many* problems that confront it, knowing only that the older paradigm has failed with a *few*. A decision of that kind can only be made on faith."
>
> Kuhn, 1962, p.158

Making a similar point Feyerabend writes[33]:

> "Thus, the Copernican view at the time of Galileo was inconsistent with facts so plain and obvious that Galileo had to call it 'surely false' ... Newton's theory of gravitation was beset, from the very beginning, by difficulties serious enough to provide material for refutation Bohr's atomic model was introduced, and retained, in the face of precise and unshakeable contrary evidence. The special theory of relativity was retained despite Kaufmann's unambiguous experimental results of 1906, and despite D.C. Miller's refutation (I am speaking of refutation because the experiment was, from the point of view of contemporary evidence, at least as well performed as were the earlier experiments of Michelson and Morley). The general theory of relativity, though surprisingly successful in some domains, failed to explain 10" in the movement of the nodes' of Venus and more than

33 References to documented evidence supporting those claims is found in original.

> 5"in the movement of the nodes of Mars; more over it is now again in trouble, due to new calculations in the motion of Mercury by Dicke and others."
>
> Feyerabend, 1965, pp.55-57[34]

These passages clearly demonstrate the untenability of Popper's falsificationist views. Nevertheless Kuhn agrees with Popper that there does exist a criterion allowing a distinction to be made between science and non-science. Kuhn formulates his criterion in terms of the research methods followed in the periods of 'normal science'

Using Popper's examples of astronomy and astrology Kuhn explains[35] in terms of his *own* criterion why the former is scientific and the latter is not:

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34 Feyerabend notes the following: With respect to Kaufmann's results, the leading physicists of the day thought it refuted the relativistic assumption of Lorentz and Einstein. Einstein alone regarded the results as improbable, because their basic assumptions, from which the mass of a moving electron is deduced, are not suggested by "theoretical systems which encompass wider complexes of phenomena". With respect to the failure of the general theory to predict accurately experimental results we find that, concerning Freundlich's analysis of the bending of light near the sun and the red shift, in 1952 Born writes to Einstein as follows (see Born, 1971, p190) "It really looks as if your formula is not quite correct. It looks even worse in the case of the red shift; this is much smaller than the theoretical value towards the centre of the sun's disc, and much larger at the edges ... Could this be a hint of nonlinearity?" Einstein (letter of 12 May 1952, *ibid,* p192) replies "Freundlich - does not move me in the slightest. Even if the deflection of light, the perehelial movement, or line shift were unknown, the gravitation equations would still be convincing because they avoid the inertial system (the phantom which affects everything but is not itself affected). It is really strange that human beings are normally deaf to the strongest arguments while they are always inclined to overestimate measuring accuracies."

35 Kuhn rejects Popper's view that astrology was not a science since *it could not be falsified*. Claiming this idea is impossible to support, he writes: "The history of astrology during the centuries when it was intellectually reputable records many predictions that categorically failed. Not even astrology's most vehement exponents

> "If an astronomer's predictions failed and his calculations checked, he could hope to set the situation right. Perhaps the data were at fault; old observations could be re-examined and new measurements made, tasks which posed a host of calculational and instrumental puzzles. Or perhaps theory needed adjustment, either by the manipulation of epicycles eccentrics, equants, etc. or by fundamental reforms of astronomical technique. For more than a millennium these were the theoretical and mathematical puzzles around which, the astronomical research tradition was constituted. The astrologer, by contrast, had no such puzzles. The occurrence of failures could be explained, but particular failures did not give rise to research puzzles, for no man, however skilled, could make use of them in a constructive attempt to revise the astrological tradition. There were too many possible sources of difficulty, most of them beyond the astrologer's knowledge, control, or responsibility. Individual failures were correspondingly uninformative, and they did not reflect on the competence of the prognosticator in the eyes of his professional compeers. Though astronomy and astrology were regularly practiced by the same people ... there was never an astrological equivalent of the puzzle-solving astronomical tradition. And without puzzles, able first to challenge and then to attest the ingenuity of the individual practitioner, astrology could not have become a science even if the stars, had in fact, controlled human destiny. In short, though astrologers made testable predictions and recognized these predictions sometimes failed, they did not and could not engage in the sorts of activities that normally characterize all recognized science's".
>
> Kuhn, 1970, pp.9-10

In short, science is a puzzle-solving activity and the scientist is a puzzle-solver by profession. The scientist has had a professional schooling which trains him into a particular way of viewing his subject matter, a knowledge of the kinds of puzzles he will be expected to solve, and the kinds of methods, techniques, etc he can legitimately employ.

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doubted the recurrence of such failures. Astrology cannot be barred from the sciences because of the form in which its predictions were cast". (Kuhn, 1970, p.8)

Working within a scientific community the scientist's professional reputation is largely determined by how he measures up compared to his colleagues as a puzzle-solver. Because "only the practitioner is blamed, not his tools" (Kuhn, 1970, p7; also see Kuhn, 1962, p79) the professional scientist strives to be *successful* in solving-puzzles. He does not, as Popper maintains, seek to disprove his theories by obtaining *negative* experimental results.[36] In more basic disagreement with Popper, Kuhn insists that the scientist's training does not provide him with the conceptual skills and the conceptual equipment that is required to critically evaluate the basic adequacy of his paradigm. In view of this we might expect that the scientist will find it difficult to know when a negative experimental outcome constitutes a potential threat[37] to the basic stability of his paradigm. On this subject Kuhn himself states:

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36 Rosenthal (1969) also disagrees with Popper. He maintains, "the expectations of the scientist are likely to affect the choice of the experimental design and procedure in such a way as to increase the likelihood that his expectations or hypothesis will be supported. That is as it should be. No scientist would select intentionally a procedure likely to show his hypothesis in error." (Rosenthal, 1969, p.195)

37 In his critique of Popper's views Lakatos makes this point very forcefully. Characterizing Popper's position vis-a-vis the untestability of psychoanalytic theory Lakatos writes: "Popper's basic rule is that *the scientist must specify in advance under what experimental conditions he will give up even his most basic assumptions*. Criteria of refutation have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst *not merely on particular clinical diagnosis but psychoanalysis itself*. And have such criteria even been discussed or agreed upon by analysts?" (Lakatos, 1974, p.2)

Comparing this situation with that surrounding the testability of Newtonian physics, Lakatos goes on "But what if we put Popper's question to the Newtonian scientist: 'What kind of observation would refute to the satisfaction of the Newtonian not merely a particular Newtonian explanation but Newtonian dynamics and gravitational theory itself?' And have such criteria even been discussed or agreed upon by Newtonians? The Newtonian will, alas, scarcely be able to give an answer". (Lakatos, 1974, pp.246-247)

> "It is Pickwickian to seek a methodological criterion that supposes the scientist can specify in advance whether each imaginable instance fits or would falsify his theory. *The criteria at his disposal explicit and implicit are sufficient to answer that question only for the cases that clearly do fit or that are clearly irrelevant These are the cases he expects the ones for which his knowledge was designed*.
>
> Kuhn, 1970, p.19, my emphasis

This raises the question as to what brings on a crisis? According to Kuhn these follow when "the (scientific) profession can no longer evade anomalies that subvert the existing tradition of scientific practice". (Kuhn, 1962, p6) But we have already seen that for Kuhn "Only the practitioner is blamed, not his tools". *What then makes an anomaly subversive*?

It is at this critical point in Kuhn's thinking that Feyerabend (1970c) has levelled criticism.

> "According to Kuhn mature science is a *succession* of normal periods and of revolutions. Normal periods are monistic; scientists try to solve puzzles resulting from the attempt to see the world in terms of a single paradigm. Revolutions are pluralistic until a new paradigm emerges that gains sufficient support to serve as a basis for a new normal period. This leaves unanswered the problem how the transition from a normal period to a revolution is brought about we have indicated how the transition could be achieved in a reasonable manner: one compares the central paradigm with alternative theories. Professor Kuhn seems to be of the same opinion. Moreover he points out that this is what actually happens. Proliferation sets in already *before* a revolution and is instrumental in bringing it about. But this means that the original account is faulty. Proliferation does not *start* with a revolution; it *precedes* it. A little imagination and a little more historical research then shows that proliferation not only *immediately precedes* revolutions, but that it is there *all the time*. Science as we

> know it is not a temporal succession of normal periods and periods of proliferation; it is their juxtaposition".
>
> Feyerabend, 1970c, pp.2ll-212

Irrespective of whether Kuhn's or Feyerabend's views are the more historically accurate, Feyerabend has presented powerful positive arguments to support his contention that a pluralistic science is likely to be more progressive than a Kuhnian monistic normal science. One of these arguments he illustrates (Feyerabend, 1965, pp.175-176) in terms of an analysis of how Brownian motion[38] came to be recognized as a refutation of the phenomenological second law of thermodynamics. Let us briefly describe the story.

In the mid-nineteenth century there existed two theories of heat - the phenomenological theory and the kinetic theory. According to the second law of the phenomenological theory entropy increased with time and therefore, it was claimed, mechanical processes were not strictly reversible. This law was the basis of much criticism of atomic theory which seemed to entail that mechanical processes *were* reversible, Kinetic theory, being an atomic theory, was likewise criticized from the standpoint of the phenomenological second law.

At the expense of the kinetic theory the phenomenological theory became more and more widely accepted. This continued until 1905 when Einstein took the peculiar

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38 Brownian motion is the name given to the motion that can be observed when, in a colloidal solution, a particle of pollen (or some other equally fine particle) seen through a microscope is observed to rapidly zigzag back and forth in all directions.

phenomenon of Brownian motion and. interpreted it within the framework of the kinetic theory. Einstein maintained that the observed motion of the Brownian particle was due to its being bombarded by the molecules making up the liquid. This analysis was confirmed when predictions when certain predictions concerning this motion, made by Einstein, were experimentally confirmed. As conceptualised in the manner proposed by Einstein, the Brownian particle is a perpetual motion machine, and its existence 'refutes' the phenomenological second law.

Feyerabend goes on to ask the question whether this relation between Brownian motion and the phenomenological second law could have been discovered had the kinetic theory not existed.

> "The answer to this question is, simply, no. Consider what the discovery of the inconsistency between the Brownian particle and the phenomenological second law would have required. It would have required (a) measurement of the *exact motion* of the particle in order to ascertain the changes of its kinetic energy plus the energy spent on overcoming the resistance of the fluid, and (b) precise measurements of temperature and heat transfer in the surrounding medium in order to ascertain that any loss occurring here was indeed compensated by the increase of the energy of the moving particle and the work done against the fluid. Such measurements are beyond experimental possibilities. Neither is it possible to make precise measurements of the heat transfer; nor can the path of the particle be investigated with the desired precision. Hence a "direct" refutation of the second law that considers only the phenomenological theory and the "fact" of Brownian motion is impossible. And as is well known, the actual refutation was brought about in a very different manner - via the kinetic theory and Einstein's utilization of it in the calculation of the statistical properties of the Brownian motion".
>
> Feyerabend, 1965, pp.175-176

Generalizing from this example Feyerabend goes on:

> "assume that a theory T has a consequence C and that the actual state of affairs in the world is correctly described by C′, where C and C′ are experimentally indistinguishable. Assume furthermore that C′, but not C, triggers, or causes, a macroscopic process M that can be observed very easily and is perhaps well known. In this case there exist observations, viz., the observations of N, which are sufficient for refuting T, although there is no possibility whatever to find this out on the basis of T and of observation alone. What is needed in order to discover the limitations of T, implied by the existence of M is another theory, T′, which implies C′, connects C′ with M, can be independently confirmed and promises to be a satisfactory substitute for T where this theory can still be said to be correct. Such a theory will have to be inconsistent with T, and it will have to be introduced not because T has been found to be in need of revision, but in order to discover whether T *is* in need of revision".
>
> Feyerabend, 1965, p.176

This is essentially the argument Feyerabend advances to support his Principle of Counter-induction which states, "it is not only possible but also desirable to introduce and elaborate hypotheses which are inconsistent with highly confirmed theories and with the evidence". (Feyerabend, 1970a, p275). The basic argument, in other words, is that the evidence which refutes a theory can often be found only with the help of an *alternative* so that the advice[39] to postpone alternatives until the first refutation has occurred puts the cart before the horse. An interesting case where counter-inductive procedures have been used successfully in *psychology* can be found in the field of perception. It has to do with the origins of the Gestalt movement. We allow Wolfgang Kohler to begin the story in his own words.

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39 Contrast this with Kuhn's "So long as the tools a paradigm supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through 11 confident employment of those tools. The reason is clear. As in manufacture, so in science - retooling is an extravagance to be reserved for the occasion that demands it". (Kuhn, 1963, p.76) See also Sheridan in his *Fundamentals of Experimental Psychology* He writes: "Does this mean that we are free to make sweeping

"When, about a hundred years ago, psychology began to develop as a new science, perception was naturally its most readily available subject matter. Those whom we now call Gestalt psychologists did their early work in this field. I will therefore now report on what happened in their investigations of perception. Almost immediately their studies developed in a direction of which most other psychologists of the time did not approve. Why? The way in which the Gestalt psychologists proceeded seemed to be incompatible with a basic principle of science. A young science, it was generally believed, must first consider the most simple facts in its field. Once these are known, the scientist may gradually turn to more complicated situations and try to discover how they can be understood as combinations of the simple elements already known. When applied to the perceptual material studied by the early Gestalt psychologists, the rule was formulated specifically in the following manner. When investigating perception one has first of all to examine the simplest nature of the elements. The early Gestalt psychologists ignored this rule. They proceeded in a different fashion, because they were not interested in these "simple elements", the so-called local sensations. First, they said, we have to inspect perceptual sciences quite impartially, to try and find in these sciences such facts as strike us as remarkable, if possible to explain their nature, to compare it with the nature of other interesting facts, and to see whether, in this fashion, we can gradually discover general rules which hold for many phenomena.

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generalizations without restriction? Of course not. We generalize principles derived from our observations, but always with the reservation that we will withdraw when contrary evidence arises. *We do not assume that differences exist until we are forced by the evidence to acknowledge such differences*. Although it may not be readily apparent, this is merely another way of stating the principle known to scientists as *Occam's razor* or the *principle of parsimony*. It is summarized in the Latin dictum *Entia non sunt multiplicanda praeter necessitatem* (new explanatory entities should not be introduced unless it is absolutely necessary). But why should it become necessary to introduce a new explanatory device? Clearly because of some new evidence that requires it. *We should introduce a new set of explanatory principles only when we are forced to do so*. or, put another way, we should assume that one set of principles explains a wide range of phenomena until we are forced by observational evidence to change our opinion". (Sheridan, 1971, p.23, my emphasis) and Wolman writes: "New hypotheses have to be introduced only when the old ones fail to account for empirical data or some other methodological considerations. Hypotheses should be as sparse as possible and as simple as possible". (Wolman, 1960, p.517)

> Obviously, in this programme the local simple elements or sensations were never mentioned. For this reason and for others, the Gestalt psychologists were soon suspected of being mystics."
>
> Kohler, 1969, pp.33-35

One of the "interesting" phenomena the Gestalts investigated was that of stroboscopic movement. Kohler goes on:

> "Wertheimer investigated the special conditions under which this phenomenon appears. Others had not done so, because they felt that stroboscopic or apparent movement was simply an *illusion* not only because it did not agree with the physical facts before the observer, but also because it disagreed with the thesis that perceptual facts consist of "independent local sensations". What did the term "illusion" mean? It meant that stroboscopic movement was not accepted as a perceptual fact at all; it was held to be the product of a mistake in the observer's thinking. Two equal perceptual facts seen in such a rapid succession, it was said are erroneously identified by the observer, and this leads to the illusion that a single object moves from one place to the other. Since nobody tried to discover whether this was really the right explanation of the observed movement, the explanation remained a mere excuse, an "explaining away" of the disturbing observation."
>
> Kohler, *op.cit.*, p.37

Along with research on such phenomenon as colour constancy, brightness constancy, gamma movement, the geometrical illusions, research on the stroboscopic effect led the Gestalt psychologists to an understanding of perception radically opposed to that based on the assumption that "psychological facts are merely aggregates of psychological atoms"[40] . In other words, by concentrating attention on phenomena that were incapable of explanation in terms of the generally accepted theory ("they were therefore explained away") yet were nevertheless 'interesting', the Gestalt psychologists

developed a theory radically challenging the adequacy of that generally accepted - even of the more usual perceptual phenomena.

The point demonstrated by these historical examples is clear. It is this: often major advances in science have followed the introduction and elaboration of an idea or theory that is incompatible with the generally accepted and well established point of view. These advances can be evidenced in that (1) *new* discoveries are made, and (2) *old* explanations are challenged. What is more important is that these advances are often contingent upon the introduction of *counter-inductive* ideas.

On this basis we might seriously question the value of a science that displays the monolithic character of Kuhnian normal science. Where a single theory stands unchallenged by alternatives it is unlikely to be confronted with 'evidence' that would ever be taken as constituting any serious threat to its basic adequacy. Confidence in the theory will increase along with a corresponding increasingly intolerant attitude toward alternatives. If it is true, as we have argued, that many facts become available only with the help of alternatives, then the refusal to consider them must result in the elimination of potentially refuting facts. This will further reinforce the belief in the uniqueness of the current theory and the complete futility of any account that proceeds in a different manner. In short, the theory becomes self-validating.

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40 Kohler records Ebbinghaus as saying, "I am not sure whether psychological facts are merely aggregates of psychological atoms, but being scientists, we must proceed as though this were true". Comments Kohler: "What a sad statement. It seems to tell us that certain alleged necessities of scientific procedure are more important than the nature of the facts that we investigate, with the consequence that we may ignore such facts as seem to be at odds with those scientific necessities". (Kohler, *op. cit*., p.45)

> "For how can we possibly test, or improve upon, the truth of a theory if it is built in such a manner that any conceivable event can be described and explained in terms of its principles? The only way of investigating such all-embracing principles is to compare them with a different set of *equally* all embracing principles - but this way has been excluded from the beginning. The myth is therefore of no objective relevance; it continues to exist solely as the result of the effect of the community of believers and of their leaders, be these now priests or Nobel prize winners. *Its "success" is entirely man-made* This is the most decisive argument against any method that encourages uniformity, be it now termed "empirical" or not."
>
> Feyerabend, 1965, p.179

By way of summing up we might note the following points: First, in the preceding chapter we rejected Popper's belief that scientific knowledge is objective knowledge, arguing that Popper's understanding of his falsifiability principle entailed the importance of subjective factors as major determinants in the evolution of this knowledge. Second, the idea that scientific knowledge is objective was again eschewed in the present chapter. Introducing Kuhn's view that scientific knowledge is knowledge that is generated by the practice of 'normal science' it was pointed out that this normal science was science practiced with the context of a 'paradigm'. Paradigms are not built on any objective base but rather are socially and psychologically constructed. As such scientific knowledge is not objective in the strict sense of the word; it is rooted in man's social condition and subject to all the exigencies that shape history. Third, Feyerabend's main criticisms of Kuhn's view were developed. Essentially those criticisms centre on the problem of how crises emerge in a science as monolithic as Kuhnian normal science. Feyerabend argues for a pluralistic science in which there would be, *contra* Moscovici, "no unified field of interest", "no systematic framework of criteria and requirements", "no coherent body of knowledge", and "no common perspectives". Such a science would follow the principles of *counter-induction*, and *tenacity*. In accordance with the principle of counter-induction the individual scientist would be encouraged to develop any idea or theory in which he felt any interest, enthusiasm, or whatever; even

where such an idea conflicted with well-established views and with everyday observations. To follow this difficult path he would be allowed to appeal to the principle of tenacity - making it legitimate for him to stick to any idea, theory, method, or whatever he liked and defend it in any way he liked, even if it meant making recourse to *ad hoc* hypotheses (as did Galileo with his Law of Circular Inertia!). Lessons learned from a close examination of the incommensurability thesis kill any fear of one's views not agreeing with more well-established views.

In conclusion we would endorse Feyerabend's advocacy of a theoretical pluralism but draw attention to a problem. While Kuhn's views on science make it difficult to understand how periods of crises emerge and revolutions take place, Feyerabend's advocacy of a pluralistic science as an answer to this problem is not sufficient. As Kordig (1971) has pointed out, the notion of incommensurability rules out the possibility of one theory *refuting* another. In short, Feyerabend's idea that proliferation of theories solves Kuhn's problem in that alternative theories bring to light new facts that *refute* more established theories cannot be supported as it stands (see Chapter 6 for argument).

To sum up, we have seen that Popper's view that observations are theory dependent made his notion of falsifiability a problematic one. Similarly it is claimed that *both* Kuhn's and Feyerabend's views on the nature of scientific change cannot be sustained as they stand.

In Chapter 6 we shall re-evaluate these views of Kuhn and Feyerabend from a socio-psychological standpoint. It will be argued that when supplemented with certain socio-psychological ideas, Feyerabend's views become more acceptable.

CHAPTER 4

OBSERVATION AND THEORY IN PSYCHOLOGY

> There is no inductive method which could lead to the fundamental concepts of physics. ... in error are those theorists who believe that theory comes inductively from experience.
>
> Einstein

All that is factual is already theory

Goethe

> "When a subject matter is very large (for example, the universe as a whole) or very small (for example, sub-atomic particles) or for any reason inaccessible, we cannot manipulate variables or observe effects as we should like to do. We therefore make tentative statements about them, deduce theorems which refer to accessible states of affairs, and by checking the theorems confirm or refute our hypothesis. ... Behavior is one of those subject matters which do not call for hypothetico-deductive methods. Both behavior itself and most of the variables of which it is a function are usually conspicuous. ... We can avoid hypothetico-deductive methods. ... by formulating the data without reference to cognitive processes, mental apparatuses, or traits. Many physiological explanations of behavior seem at the moment to call for hypotheses, but the future lies with the techniques of direct observation which will make them unnecessary".
>
> B.F. Skinner: *Contingencies of Reinforcement*

Much of the discussion so far has focused on questions relating to the nature and function of theory in science. For illustrations we have appealed mostly to physics and astronomy. This has been for one main reason. The major object of this thesis is to present a socio-psychological understanding of the nature and function of scientific theory. To do this we have sought to show that the usual story relating to the objectivity of science is largely a myth and in so doing, to make plausible the idea that socio-psychological factors constitute important determinants of processes internal to science. Arguments opposing this notion of the objectivity of science are no doubt more likely to be convincing should they seriously challenge the objectivity of the most "objective" sciences, e.g. such well established sciences as those of physics and astronomy. In this chapter, however, we seek to illustrate how the issues raised in the discussion so far have implications for *psychological* theorizing.

We have chosen to centre our demonstration around a critical examination of the behaviouristic approach of B.F. Skinner. This has been for two principal reasons. First, the alleged objectivity of Skinner's behaviouristic approach. Second, the wide-

spread influence of his views on socio-psychological theories and practices. (See, e.g., Kiesler, Collins and Miller, 1969; Deutsch and Krauss, 1965; Krasner and Ullmann, 1973; McGinnies and Ferster, 1971.)

In the above cited passage taken from his *Contingencies of Reinforcement* Skinner makes explicit the assumptions underlying his arguments in support of the behaviouristic approach. In the light of arguments thus far presented, it is maintained these assumptions are deficient. To highlight these, reference is made to two historical examples - Galileo's tower argument and Heisenberg's reinterpretation of phenomena observed in Wilson cloud chambers.

It hardly needs mentioning that the choice of these two examples is, once again, deliberate - Galileo's subject matter being very large (the universe) and Heisenberg's very small (subatomic particles). It will be demonstrated that we here have two clear and powerful counter-examples to Skinner's understanding of the nature and function of hypothetico-deductive theorizing in science.

Already the story surrounding Galileo's famous tower argument has been told. Turning to Heisenberg, we find in his book *Physics and Beyond* he tells the story surrounding the difficulty he and Niels Bohr had in reconciling quantum theory with certain well-known observations. Understood in terms of the quantum theory (a hypothetical system including in its domain very small phenomena), sub-atomic particles could not possess any well-defined spatial representation.

This conflicted with the readily available observations that one could make of the trajectories made by electrons and other sub-atomic particles as they passed through

a Wilson cloud chamber. If Skinner were correct, then Heisenberg and Bohr would simply have rejected their hypotheses regarding the nature of these very small and inaccessible particles. Heisenberg tells the story:

> "... neither of us could tell how so simple a phenomenon as the trajectory of an electron in a cloud chamber could be reconciled with the mathematical formulations of quantum or wave mechanics. Such concepts as trajectories or orbits did not figure in quantum mechanics, and wave mechanics could only be reconciled with the existence of a densely packed beam of matter if the beam spread over areas much larger than the diameter of an electron.
>
> Since our talks often continued till long after midnight and did not produce a satisfactory conclusion despite protracted efforts - over several months, both of us became utterly exhausted and rather tense. Hence Bohr decided in February 1927 to go skiing in Norway, and I was quite glad to be left in Copenhagen, where I could think about these hopelessly complicated problems undisturbed. I now concentrated all my efforts on the mathematical representation of the electron path in the cloud chamber, and when I realized fairly soon that the obstacles before me were quite insurmountable, I began to wonder whether we might not have been asking the wrong sort of question all along. But where had we gone wrong? The path of the electron through the cloud chamber obviously existed; one could easily observe it. The mathematical framework of quantum mechanics existed as well, and was much too convincing to allow for any changes. Hence it ought to be possible to establish a connection between the two, hard though it appeared to be.
>
> It must have been one evening after midnight when I suddenly remembered my conversation with Einstein and particularly his statement: "It is the theory which decides what we can observe". I was immediately convinced that the key to the gate that had been closed for so long must be sought right here. I decided to go on a nocturnal walk through Faelled Park and to think further about the matter. We had always said so glibly that the path of the electron in the cloud chamber could be observed. But perhaps what we really observed was something much less.
>
> Perhaps we merely saw a series of discrete and ill-defined spots through which the electron had passed. In fact, all we do see in the cloud chamber are individual water droplets which certainly must be

> much larger than the electron. The right question should therefore be: Can quantum mechanics represent the fact that an electron finds itself approximately with a given velocity, and can we make these approximations so\close that they do not cause experimental difficulties?
>
> A brief calculation after my return to the Institute showed that one could indeed represent such situations mathematically and that the approximations are governed by what would later be called the uncertainty principle of quantum mechanics".
>
> Heisenberg, 1971, pp.77-78

Taking this example along with Galileo's tower argument consider how they contradict Skinner.

We start with two conjectured theories - first, the Copernican heliocentric theory and second, quantum mechanics. Next, we find that each of these two theories is confronted with observational difficulties. The Copernican theory was considered refuted by observations at the Tower of Pisa (Tycho Brahe was one of those convinced of this refutation). The quantum theory seemed irreconcilable with the trajectories observed in Wilson cloud chambers.

The result in each of these cases was the same. *Contra* Skinner, *the observations were rejected in favour of the theory*. Phenomena accessible to direct observation (falling stones, trajectories) were reinterpreted[42] from the standpoint of the new theory. Direct observational evidence was then obtained that confirmed rather than refuted the new theory.

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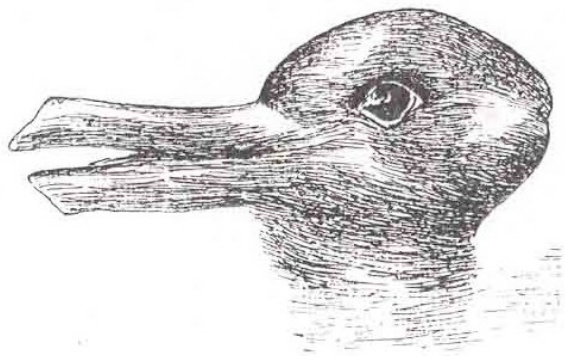
42 Contrast this with the statement that: "Scientists are always docile to the experimental evidence". (Sheridan, 1971, p.23)

It follows from this that Skinner's understanding of the nature and function of hypothetico-deductive theorizing in science is deficient in the following two respects:

First, Skinner's understanding of how theories in science are tested is too simplistic. His positivistic ideas on observation display a lack of appreciation regarding the crucial role that theory plays in the development of science.

Second, like the positivists, he assumes that observation itself is theory-neutral and remains immune to modification by any theoretical developments. Already we have argued at length that this assumption is not tenable. As the stories of Galileo and Heisenberg show, observations are heavily injected with theory. Later we hope to show that this erroneous assumption is the root of serious inadequacies in Skinner's thinking.

Hanson (1958) draws an analogy between the reversible perspective figures, which appear in any text on Gestalt psychology, and the manner in which "theories provide patterns within which data appear intelligible". In terms of this analogy he refers to theories as "conceptual Gestalts". As an illustration we might consider the following well-known ambiguous 'duck-rabbit' figure.



When viewing this figure people generally are able to switch back and forth between 'seeing' it as a duck or a rabbit: that is, they are able to "alternate" between seeing it as the one, and then the other.

It is to be noted that the Gestalt psychologists (see, e.g. Kohler, 1969) claimed that the overall pattern into which the stimuli were organized determine the categorization that the perceiver imposes on any stimulus making up the pattern (e.g. whether they 'see' a duck or a rabbit determines whether the 'head' they see is that of a bird or a mammal). In this regard, Kohler writes:

> "when we look at the molar unit which is called a square, four points in the boundary of this figure have the character, of being "corners". Precisely the same points in the location would not have this particular character if they were points of the boundary of a circle. Being a corner is, therefore, not a property which these points have as such; rather it is a *property which they acquire within a particular larger context*."
>
> Kohler, 1969, p.54, my emphasis

Similarly the theoretical standpoint of the observer determines the categorization he imposes on any single stimulus contained in his perceptual manifold.

> "A theory is not pieced together from observed phenomena it is rather what makes it possible to observe phenomena as being of a certain sort . . ."
>
> Hanson, 1958, p.90

It is in this important respect that Hanson's analogy captures the thesis of Popper, Kuhn, and Feyerabend that what is observed is *always* theory-dependent.[43]

Using Hanson's analogy it is suggested that Skinner's radical behaviouristic theory constitutes one such conceptual Gestalt and as such provides a fundamentally different picture of human behaviour to that familiar to the layman. To draw out some of the major differences we first present a brief characterization of Skinner's views.

Science, according to Skinner is based on a number of assumptions: these include the causal principle (the principle that every event has some cause) and physicalism (the thesis that reality can be *completely* described in the language of physics i.e., there is nothing in reality that cannot be fully described in physical terms).

Assuming the causal principle as a basis,[44] Skinner's reasoning is essentially as follows: Wherever there is a behaving organism we can divide the sequence of events occurring in this region of space and time into four classes:

1. *Stimulus events* - i.e. those events in the organism's external environment that impinge on its sensory receptors.

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43 The idea that observation can be theory-neutral would correspond to the case where the pattern (theory) imposed on the stimulus field did *not* dictate how each component stimulus was to be categorized (a view maintained by the structuralists).

44 Skinner's views on the nature of science and the scientific approach to the study of psychology can be found in B.F. Skinner: *Science and Human Behavior,* Macmillan Press, 1953, Section 1, esp. pp.33-36.

2. *Mediating events* - i.e. those events internal to the organism that are both caused by stimulus events and in turn cause behavioural events.

3. *Behavioural events* - i.e. those events constituting the behaviour of the organism.

4. *Irrelevant events* - i.e. those events not contained in any of the three classes above (e.g. causal antecedents of events in (1), causal consequents of events in (2), events taking place on the moon, etc).

Given the assumptions:

(a) that events in (3) are a function of events in (2), and

(b) that events in (2) are a function of events in (1),

it follows that there exists a functional relationship between events in (3) and events in (1) i.e. that events in (3) are a function of events in (1).

In addition to (a) and (b) we have, then, (c): *Behaviour is a function of environmental events*. This means that, at least in principle, it must be possible to find lawful relations between behaviour and antecedent environmental events. Due to the accessibility of these events to direct observation, to measurement and manipulation, Skinner maintains that it is not only sound *theory* but also sound *methodology* to confine an experimental analysis of behaviour to the investigation of the functional

relationships between environmental events and behavioural events. (See also MacCorquodale, 1970.)

Assuming further that human behaviour is continuous with that of infrahuman species, Skinner (1969, 1971) maintains that within phylogenetically determined limits, ontogenetic contingencies are responsible for the shaping and maintaining of behaviour. Thus, for Skinner, all learned behaviour is a consequence of 'respondent' and/or 'operant' conditioning.

Having outlined Skinner's position attention is now turned to highlighting some fundamental differences between his views and those held by the layman.

Perhaps most important is that whereas Skinner insists that a scientific explanation of behaviour must be a *causal* explanation, in everyday terms most human behaviour is understood to be *goal-directed* i.e. it is behaviour that is performed for reasons, or to fulfil certain intentions, purposes, desires, needs, etc.[45]

According to Skinner such explanations of behaviour are unscientific. In his *Beyond Freedom and Dignity* he maintains that major advances in physics and biology followed the replacing of teleological by non-teleological explanations. For example, new kinematic laws were discovered when the velocity of a free-falling object was examined not as a function of the *distance still to fall* (to reach its *proper* place, or *goal*

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45 Harre and Secord (1972) maintain that "social behaviour is meaningful behaviour ... it involves an agent with certain intentions and expectations". See also C. Taylor, 1964; R. Taylor, 1966; Hampshire, 1965.

but rather as a function of the *distance already fallen* (from its point of *origin*) In like fashion, Skinner considers a science of behaviour will only make progress when explanations of behaviour in terms of purposes, expectations, intentions, etc are eschewed.

Comparing the state of affairs in psychology with respect to that in biology Skinner writes:

> "An apparent resemblance concerns intention or purpose. Behavior which is influenced by its consequence seems to be directed toward the future. We say that spiders spin webs in order to catch flies and that men set nets in order to catch fish. The "order" is temporal. No account of either form of behavior would be complete if it did not make some reference to its effects. But flies or fish which have not yet been caught cannot affect behavior. Only past effects are relevant. Spiders which have built effective webs have been more likely to leave offspring and setting a net in a way that has caught fish has been reinforced. Both forms of behavior are therefore more likely to occur again, but for very different reasons.
>
> The concept of purpose has had an important place in evolutionary theory. It is still sometimes said to be needed to explain the variations upon which natural selection operates. In human behavior a "felt intention" or "sense of purpose" which precedes action is sometimes proposed as a current surrogate for future events. Men who set nets "know why they are doing so", and something of the same sort may have produced the spider's web-spinning behavior which then became subject to natural selection.
>
> But men behave because of operant reinforcement though they cannot "state their purpose", and when they can, they may simply be describing their behavior and the contingencies responsible for its strength. Self-knowledge is at best a by-product of contingencies, it is not a cause of the behavior generated by them, Even if we could discover a spider's felt intention or sense of purpose, we could not offer it as a cause of the behavior".
>
> Skinner, 1969, pp.193-194

In short the most fundamental difference between the Skinnerian and layman's notions on behaviour is that:

For Skinner, *all* behaviour can be understood as a function of environmental events alone.

For the layman, *some* behaviour is necessarily a function of attitudes, beliefs, intentions, etc.

This raises another important difference. Whereas for Skinner the concept of choice as usually understood cannot be used in the explanation of behaviour, in many common-sense explanations it is vital.

The notion of choice is basic to an understanding of all those concepts considered basic to moral discourse. A person cannot be held responsible for actions he performed that were not of his own volition and this is the line of reasoning Skinner develops in his *Beyond Freedom and Dignity*

> "By questioning the control exercised by autonomous man and demonstrating the control exercised by the environment, a science of behavior also seems to question dignity or worth. A person is responsible for his behavior not only in the sense that he may be justly blamed or punished when he behaves badly, but also in the sense that he is to be given credit and admired for his achievements. A scientific analysis shifts the credit as well as the blame to the environment, and traditional practices can then no longer be justified.
>
> Skinner, 1972, p.21

According to Skinner the difference between the commonly held view of behaviour and his own, results from the fact that they are based on fundamentally different approaches to the understanding of behaviour. The traditional view reflects a *pre-scientific* mode of explanation while he himself has developed a *scientific* view. Common sense, unable to detect lawful relations between the individual's overt behaviour and antecedent events in his external[46] environment, resorts to 'animistic' explanations. In other words, it postulates the existence of a mind located inside the individual and proceeds to explain behaviour in terms of the workings of this mind. The unscientific status of these explanations is revealed, according to Skinner in that they are either (1) not genuine explanations at all, or (2) they explain behaviour in terms of phenomena that cannot be shown to have anything other than a hypothetical existence.

Skinner claims that many traditional explanations are explanations of the first sort. For example, to say somebody wants to eat "*because* he is hungry" where, to be hungry means to want food, is circular.[47]

Traditional mentalistic explanations are of the second sort, Skinner maintains. In that the hypothetical 'mentalistic' phenomena referred to in mentalistic explanations

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46 "... the doctrines of animism, ... are primarily concerned with explaining the spontaneously and evident capriciousness of behavior. The living organism is an extremely complicated system behaving in an extremely complicated way. Much of the behavior appears at first blush to be absolutely unpredictable. The traditional procedure has been to invent an inner determiner, a "demon", "spirit", "homunculus", or "personality" capable of spontaneity change or of origination of action". (Skinner, 1956, p.79)

47 This in fact is not so. As Scriven (1956) points out, this *is* an informative explanation of why somebody wants to eat. That he wanted to eat *because he wanted to put on weight* might have been the *true* explanation.

(e.g. beliefs, intentions, desires) are neither available to observation, nor definable in terms of observables, Skinner characterizes them as "explanatory fictions" (Skinner, 1953, 1957, 1969) and the explanations are similarly fictitious.

To replace these "fictitious" explanations by those which are truly scientific Skinner maintains that it is best to seek lawful relations between directly observable overt behaviour and directly observable environmental events. In so doing it becomes unnecessary to resort to hypotheses. "We can avoid hypothetico-deductive methods ... the future lies with the techniques of direct observation which will make them unnecessary".

Now, basic to Skinner's attack on the commonly held view of behaviour in his assumption that there exists an observational base underlying scientific theorizing that is itself theory-neutral. However, as we have argued this assumption is untenable. Once this is recognized, Skinner's attack is seen to be weak as demonstrated below.

It was noted above that in his arguments supporting the behaviouristic approach, Skinner distinguished three classes of events that occur in any situation where there is a behaving organism, (forgetting for the moment the class of events irrelevant to the analysis). These three classes contain respectively:

1. *Stimulus* events in the external environment.

2. *Mediating* events inside the organism.

3. *Behavioural* events contingent on events in (2).

What Skinner fails to appreciate is that although the events in (1) and (3) are available to 'direct observation', this does not mean that what different theorists observe is going to be the same, irrespective of their theoretical standpoints. In other words, depending on their assumed theoretical position, observers will "see" behaviour quite differently. For instance the layman when watching an individual behaving in some situation will see him as being 'lazy', 'happy', 'anxious', 'decent', etc. The radical behaviourist when watching the same individual will observe 'avoidance behaviour', 'operant aggression', 'peak shift', 'behavioural contrast', 'extinction', etc. A similar argument holds for events in (1).

To further clarify the above argument we make use of Hanson's conceptual Gestalt analogy. Consider again a situation in which there is a behaving organism. Further, two opposing theories are introduced. First, the Skinnerian theory, and second the theory implicit in the common-sense view of things. Each of these two theories introduce a distinctive structure in terms of which it is possible to order all the events taking place in that situation. Now because the property ascribed to *any* stimulus within a stimulus field is acquired in terms of its location "within a particular larger context" (Kohler), *all* events in the above situation will be conceptualised and observed relative to the assumed point of view. Should we take an event from any of Skinner's three classes its properties are contingent upon its location within its wider context. This means that if an event is taken from class (3) say, it will be viewed as either "telling a lie", or an "avoidance response" depending on the wider context within which it is viewed. In other words how it is seen is a function of the observer's point of view.

A similar argument holds good as a defence of psychodynamic approaches to the understanding of human behaviour. In view of the foregoing arguments it should be apparent that a great deal of the recent criticism of the psychodynamic approach is based on the, very same positivistic assumptions claimed to underlie Skinner's views. In

defending his own psychodynamic views from the standard criticism that they lack empirical content, Fromm has written:

> "*The essential point* in this approach *is* to penetrate through the surface of past or present behaviour and *to understand the forces which created the pattern of past behaviour* ... What are these forces we speak of here? They are nothing mysterious, nor figments of abstract speculation. They are recognizable empirically if one studies the behaviour of the person in the proper way ... *one can observe them in many ways: by examining dreams, free association, fantasies, by watching his facial expression, his gestures, his way of speaking and so forth*. Yet they are often not directly observable but must be inferred. Furthermore *they can be seen only within the theoretical frame of reference in which they have a place and meaning*."
>
> Fromm, 1962, pp.l9-20, my emphasis

Fromm's belief that the 'forces' of which he speaks "can be seen only within the theoretical frame of reference in which they have a place and meaning" is exactly the position that has been argued, which in the words of Hanson is that "a theory is ... what makes it possible to observe phenomena as being of a certain sort".

It is suggested that surprising as it might seem, the views of Skinner are not more *based* on observations than either those of common-sense, or those of the psycho-dynamic theorist. In short, consistent with the line of reasoning maintained throughout this essay, it is suggested that Skinner's views are no less hypothetical than those either of the layman or of any 'mentalistic' psychologist.

Thus far we have argued that both Skinner's arguments in support of his own

views and those criticizing alternative views are largely unfounded in that they are based on the untenable assumption of a theory-neutral empirical base. However, there is yet another defect in Skinner's philosophy of science - indeed, one which often results in his views being exposed to criticism irrelevant to his basic standpoint. Essentially the problem results from Skinner's failure to fully appreciate the incommensurability between his own views and those of the layman. This is reflected in that at times he understands his programme as involving the development of a theory that will eventually replace that of the layman, while at other times it seems he views his programme as involving the development of a theory that not only generates greater understanding but also encompasses the understanding afforded in terms of the common-sense view. With regard to the first of these two possibilities Skinner writes:

> "We dispossess the Inner Man by replacing him with genetic and environmental variables. To avoid sneezing we ward off, not a devil, but the pepper. We trace multiple personalities to multiple contingencies of reinforcement. We replace the Super-ego and Id of Freud as well as the Conscience and Old Adam of Judeo-Christian theology with "good" and "evil" phylogenic and ontogenic contingencies".
>
> Skinner, 1969, p273

And:

> "It is often said that an analysis of behavior in terms of ontogenic contingencies "leaves something out of account", and this is true. It leaves out habit, ideas, cognitive processes, needs, drives, traits, and so on".
>
> Skinner, 1969, p183

And again:

> "We extrapolate from relatively simple conditions to relatively complex, not to confirm what someone claims to have seen in the complex case, but to begin for the first time to see it in a new light".
>
> Skinner, 1969, p103

Unfortunately Skinner is not consistent in his development of a new theory with which to *replace* the traditional viewpoint. This is clearly manifest in his repeated attempts to provide *translations* of traditional terms into his own language. Such attempts involve the *reducing* of traditional concepts to his own. For example, he writes:

> "Many classical distinctions can be reduced to the distinction between rule-governed behavior and contingency-shaped behavior".
>
> Skinner, 1969, p169

The distinctions for which he proposes such a reduction include: (1) deliberation/impulse, (2) intellect/emotion, (3) logical argument/intuition, (4) truth/belief, and (5) reason/passion.[48]

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48 Further such examples involving the attempt at such translations can be found in Skinner, 1969, pages 37, 44, 144, 152, 156, 165, 169, 193, 229, 245, and 265.

Consideration as to how Skinner defines any of these concepts indicates the disparity between his own understandings of same, and how they are traditionally understood. To illustrate this consider the following example. Skinner equates "logical thinking" with thinking according to rules: in other words it is a form of 'rule-governed' behaviour. But rule-governed behaviour according to Skinner, demands a "consciousness" of the rules (see Skinner, 1969, pp169-170). This would mean that since most people can intuitively distinguish most valid from invalid Aristotelian syllogisms they should be able to state the rules as to the manner these syllogisms are so distinguished. Yet in fact this is far from true, as an attempt to teach these rules will reveal!

Similarly, it can be clearly seen that "reasons" as usually understood, cannot be equated with contingencies of reinforcement (c.f., Skinner, 1969, p171). Reasons are either logically consistent or logically inconsistent, etc., but it does not make any sense to predicate logical consistency of contingencies of reinforcement. Needless to say, if any[49] of Skinner's attempts to provide such translations are examined, it will be found

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49 Chomsky (1959) in his famous and powerfully written critique of Skinner's *Verbal Behavior* has shown the fundamental inadequacy inherent in a large number of such translations proposed by Skinner. In discussing Skinner's attempt at reducing traditional grammatical classifications to a classification in terms of reinforcing contingencies, Chomsky writes: "The traditional classification is in terms of the intention of the speaker. But intention, Skinner holds, can be reduced to contingencies of reinforcement, and correspondingly, we can explain the traditional classification in terms of the reinforcing behavior of the listener. Thus, a question is a mand which "specifies verbal action and the behavior of the listener permits us to classify it as a request, a command, or a prayer". It is a request if "the listener is independently motivated to reinforce the speaker", and a command if the mand "promotes reinforcement by generating an emotional disposition". The mand is advice if the listener is positively reinforced by the consequences of mediating the reinforcement of the speaker; it is a warning if "by carrying out the behavior specified by the speaker the listener escapes from aversive stimulation", and so on. All this is obviously wrong if Skinner is using the words *request, command,* etc in anything like the sense of the

that it is not difficult to locate similar inadequacies. In view of the incommensurability between the views of the layman and the radical behaviourist it is to be expected that any such proposed translations cannot hope to succeed. Yet it must be realized that to criticize either Skinner's translations themselves or his theory as being deficient since it cannot provide such translation does not in any way count as criticism of his views qua a theory of behaviour. For Skinner's theory to be true it is not necessary for it to be able to encompass 'common-sense knowledge' within its framework. A reduction programme of this sort is ruled out by incommensurability. Yet in the very same manner, Skinner should realize that his views are based on assumptions[50] as open to question as those under-lying the views of the layman, and that any new understanding

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corresponding English words. The word *question* does not cover commands. *Please pass the salt* is a request (but not a question), whether or not the listener happens to fulfil it; not everyone to whom a request is addressed is favorably disposed. A response does not cease to be a command if it is not followed; nor does a question become a command if the speaker answers it because of an implied or imagined threat. Not all advice is good advice, and a response does not cease to be advice if it is not followed. Similarly, a warning may be misguided; heeding it may cause aversive stimulation, and ignoring it may be positively reinforcing. In short, the entire classification is beside the point. A moment's thought is sufficient to demonstrate the impossibility of distinguishing between requests, commands, advice, etc., on the basis of the behavior or disposition of the particular listener. Nor can we do this on the basis of the typical behavior of all listeners. Some advice is never taken, is always bad, etc, and similarly, with other kinds of mands. Skinner's evident satisfaction with his analysis of the traditional classification is extremely puzzling". (Chomsky, 1959, pp.567-568)

50 The hypothetical status of Skinner's views is overlooked not only by Skinner himself, but also by many of his sympathizers. For example, MacCorquodale and Meehl (1948) claim that "Since Skinner hypothesizes nothing about the character of the inner events, no finding about the inner events could prove disturbing to him. At most he would be able to say that a given discovery of internal processes must not be complete because it cannot come to terms with his (empirical) laws". (MacCorquodale and Meehl, 1948, p.608)

Skinner *does* make hypotheses about the character of internal mediating events. For example his theory, as we have already noted, assumes at least *causal determinism* and *physicalism.* Now both these assumptions may be false (in fact the

his theory provides does not establish any intrinsic inferiority of the common-sense view of things.

Let us now summarise and conclude the present chapter. We have attempted to make explicit some of the more important implications of issues raised in previous chapters for *psychological* theorizing. The behaviouristic approach of B.F. Skinner was chosen for critical analysis for two principal reasons: (a) the alleged objectivity of his approach, and (b) the widespread influence of his views on socio-psychological theorizing and practices. A number of weaknesses were exposed in Skinner's philosophy of science. It was maintained that one of these underlies Skinner's critique of the traditional approach to the understanding of behaviour - viz. he erroneously assumes that science is erected on a theory-neutral observational base. It follows from this that his argument that traditional explanations are pre-scientific in that they appeal to "explanatory fictions" lacks force. It was further maintained that Skinner's failure to appreciate the incommensurability between his own views and those of the layman

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first probably is false). Perhaps 'primitive' animist theories are closer to the truth than Skinner's views. To dogmatically insist that current theories of physics, chemistry, etc set the boundary conditions to which psychological theory must conform would prevent us ever finding out just how adequate such a psychology is. As we have already seen, to reveal weaknesses in any theoretical point of view it is desirable to invent and elaborate hypotheses inconsistent with well established observations to serve as *standards of comparison.* This is the course adopted by Copernicus, Galileo, Einstein, Heisenberg, Skinner, etc, etc, etc. Not only did, for example, Galileo and Heisenberg invent and use such hypotheses, they developed theories based on these hypotheses that radically changed our taken-for-granted knowledge about the everyday-world. The attitude adopted by MacCorquoclale and Meehl (1948) is further reflected when NacCorquodale (1970) again writes: "So far as I can tell, the behavioral facts of reinforcement are by now so well-known and dependable that theories of the detail of its mediation are no longer of great interest". (MacCorquodale, 1970, p.91) It is this very attitude that shows a lack of interest in new discoveries that inhibits the development of these new discoveries.

constituted another weakness. This latter weakness results in an inconsistency in Skinner's understanding as to the nature of his programme. Thus, on the other hand Skinner claims his programme to be that of developing a *scientific* view of man with which to ultimately *replace* traditional *pre-scientific* explanations. However, on the other hand Skinner attempts to *translate* traditional formulations into his own language and to encompass common-sense explanations within the boundaries prescribed by his own theoretical viewpoint. It was argued that owing to the incommensurability between the radical behaviourist standpoint and that reflected in the views of the layman, Skinner's attempt at such a *translation* could not succeed. In view of this, it is suggested that the radical behaviourist would be advised to concentrate on the development of an alternative theoretical framework in terms of which it would be possible to develop new insights. Such new insights would then serve to show up limitations in lay explanations, psychodynamic theories, ethological theories, etc. So construed, radical behaviourism is a viewpoint worthy of development. This is true even in view of the fact that there exist clear inadequacies in the theory as it stands.[51]

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51 It is not intended in this essay to criticize any psychological or social psychological theory *qua* a theory of phenomena in those domains. Nevertheless, the inadequacies referred are discussed in Scriven (1956), Schick (1971), Bandura (1969), Premack (1970), and in numerous other critiques of the radical behaviouristic point of view.

CHAPTER 5

SCIENCE AND THE NOMIZATION OF EXPERIENCE

> The order of history emerges from the history of order. Every society is burdened with the task, under its concrete conditions, of creating an order that will endow the fact of its existence with meaning in terms of ends divine and human.
>
> Voegelin

> "The socially constructed world is, above all, an ordering of experience. A meaningful order, or nomos, is imposed upon the discrete experiences and meanings of individuals. To say that society is a world-building enterprise is to say that it is ordering, or nomizing, activity. The presupposition for this is given, ... in the biological constitution of *homo sapiens.* Man, biologically denied the ordering mechanisms with which other animals are endowed, is compelled to impose his own order upon experience. Man's sociality pre-supposes the collective character of this ordering activity. The ordering of experience is endemic to any kind of social interaction. Every social action implies that individual meaning is directed toward others and ongoing social interaction implies that the several meanings of the actors are integrated into an order of common meaning."
>
> Peter Berger: *The Sacred Canopy*

In agreement with Popper (1959, 1963), Kuhn (1962, 1970), and Feyerabend (1962, 1965), it has been argued that *all* knowledge is conjectural, lacking any firm foundation on which it can remain forever secured. Rejecting Popper's view that there exist principles of rationality that provide an objective basis for deciding which of two theories is closer to the truth, we agreed with Kuhn and Feyerabend that such decisions are never free from the influence of subjective determinants. In other words, it was argued that given *any* two theories, it is impossible to make an objective decision regarding their respective degrees of verisimilitude. In concluding chapter 3, it was claimed that the very notion basic to Kuhn's and Feyerabend's arguments against an objective knowledge - viz., their notion of incommensurability - made their views regarding developments in science problematic. Kuhn fails to explain how within periods of normal science anomalies come to *subvert* the prevailing paradigm. Even more problematic is that, in view of incommensurability, Kuhn fails to explain how it is that a revolution takes place at all. It was also claimed that Feyerabend's (1965, 1970b, 1970c) view that the solution to Kuhn's problem lies in a pluralistic science is itself problematic; Feyerabend fails to explain how one theory refutes another. It would seem

then, that current philosophy of science fails to provide any convincing account as to the nature of scientific change.

The author's opinion is that a more adequate account can be provided when these changes are viewed from a socio-psychological standpoint. In the following chapter a current socio-psychological theory will be presented, and in terms of this theory the author will suggest a possible way of viewing developments in science. It is suggested that in terms of this theory it is possible to resolve the above-mentioned problems confronting Kuhn's and Feyerabend's views.

Before going on to propose this socio-psychological analysis of scientific change the author will suggest a socio-psychological theory of science and its knowledge. This chapter is concerned with the presentation of this theory.

Rather than developing arguments designed to convince the reader of the theory's validity, an attempt will be made to introduce the theory through a presentation of ideas already widely accepted. In short, the socio-psychological analysis of science proposed is not so much an innovation in theory as rather an extrapolation of current ideas to accommodate an as-yet unconsidered domain of their application - viz., the scientific enterprise.

The particular ideas referred to come mainly, if not originally, from the theories of two men - Muzafer Sherif and Peter Berger. In this chapter we concentrate therefore on presenting first, the basic ideas underlying Sherif's work on the psychology of social norms (see, e.g. Sherif, 1936; Sherif and Sherif, 1969), and second, the ideas central to Berger's 'sociology of knowledge'. We begin with the work of Sherif.

Basing his thinking very much on the ideas of Durkheim, Sherif set about devising an experiment that would allow a controlled study of factors influencing the formation of norms. A norm is defined as:

> "a standard or, better, as a scale consisting of categories that define a range of acceptable attitude and behavior, and a range of objectionable attitude and behavior, for members of a social unit in matters of consequence to that unit".
>
> Sherif and Sherif, 1969, p.184

Discussing the kinds of factors that were considered fundamental to designing the experiment Sherif and Sherif, (1969) write:

> "What are the essentials to be embodied as the minimum required for an experimental model of norm formation? The sociologist Durkheim wrote that norms take shape in out-of-the-ordinary situations, when the usual rules and routines of daily living are not applicable".
>
> Sherif and Sherif, 1969, p.210

In other words:

> "Norms form when individuals interact in problem situations that involve uncertainties and choice among alternative modes of action".
>
> Sherif and Sherif, 1969, p.201

The degree of 'uncertainty' or 'choice' in a situation is determined by the degree of 'structure' present in that situation.

> "Norms form when individuals interact in problem situations that involve uncertainties and choice among alternative modes of action".
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> Sherif and Sherif, 1969, p.201

The degree of 'uncertainty' or 'choice' in a situation is determined by the degree of 'structure' present in that situation.

> "Structured stimulus situations set limits to alternatives in psychological patterning. Stimulus structure refers to properties of objects that are objectively patterned or ordered in terms of specifiable criteria or dimensions. Such properties limit the number of alternatives that psychological patterning can take. Evidence for this proposition is clear in research on perception, that is, the experience of present objects and events".
>
> Sherif and Sherif, 1969, p.51

A digression is made at this stage to critically examine Sherif's notion of an 'objectively structured' situation. While for Sherif, the degree of structure is not an all-or-none affair, it is determined by objective properties pertaining to the stimulus. Included among these objective stimulus-properties are, for example, such physical properties as the intensity of the stimulus, its size, movement, and even the relative locations of some number of stimuli in both space and time. Such an analysis suggests that as well as there being instances in which the individual *imposes* structure on to the

stimulus field, there are other instances in which the individual perceives the structure that is *inherent within* the stimulus field. Such a position does not accord with the view we have maintained throughout this essay that what is observed is *always* theory-dependent; in other words, that any structure observed to pertain to some stimulus field is *always* contributed by the perceiver. Even the properties assigned to *physical* phenomena on the basis of observation are imposed by the perceiving subject. For example, the properties of length, size, and mass are not properties pertaining to physical phenomena themselves, but rather are contributed by the observer.

Perhaps most powerfully advocated by the German philosopher Immanuel Kant, this view has been at the basis of not only the views of such philosophers of science as, for example, Popper, Kuhn, and. Feyerabend, but also those of Gestalt psychologists and theorists in such fields as physics (Bohr, Heisenberg, Bohm) and ethology (Lorenz, Tinbergen).

Kant argued that perception of objects was always in terms of categories that had a *necessary a priori* validity. Lorenz (1965) however, is one who has argued that, although perception presupposes the existence of *a priori* categories of perception, these have no absolute validity. For Lorenz, these categories are phylogenetically programmed in the genome (i.e., the product of an evolutionary selection process) so as to enable the individual to adapt its behaviour to its environment in the course of its ontogenetic development.

> "Just as the hoof of the horse is adapted to the ground of the steppe which it copes with, so our central nervous apparatus for organizing the image of the world is adapted to the real world with which man has to cope. Just like any organ, this apparatus has attained

> its expedient species - preserving form through this coping of real with the real during a species history many eons long".
>
> Lorenz, 1941; cited in Campbell, 1974, p.447

Arguing that the particular categories that are a priori given for some species are relative to its phylogenetic history, Lorenz rejects Kant's view that they have any necessary validity.

> "But surely these clumsy categorical boxes into which we have to pack our external world "in order to be able to spell them as experiences" (Kant) can claim no autonomous and absolute validity whatsoever. This is certain for us the moment we conceive them as evolutionary adaptations ... At the same time, however, the nature of their adaptation shows that their categorical forms of intuition and categories have proved themselves as working hypotheses in the coping of our species with the absolute reality of the environment ...
>
> All the knowledge an individual can wrest from the empirical of the 'physical world-picture' is essentially only a working hypothesis. And, as far as their species-preserving function goes, all those innate structures of the mind which we call *'a priori'* are likewise only with working hypotheses. Nothing is absolute except that which hides in and behind the phenomena. Nothing that our brain can think has absolute, *a priori* validity in the true sense of the word, not even mathematics with all its laws".
>
> Lorenz, *op.cit.,* p.446

It is within the context of such views that we might understand how it is that certain categories are universally present in all the multiplicity and diversity of the worldviews man has created. This universality does not reflect their origins in everyday observations available to a *pre-theoretical* form of perception. Rather it reflects what is

perhaps a *genetically based* disposition for man to *interpret* his experience in accordance with a particular set of categories. Consequently an observed tendency for all humans to view simple physical situations in much the same way does not entail that the commonality of the structure they "see" in this situation is a property belonging to the situation itself.[52]

It is essentially this understanding of the ultimately subjective origins of all observations that can be used as a basis to criticize Sherif's concept of 'objective structure'. It is suggested that, when aligned with the views above, the generality of Sherif's views on the nature of function of norms is greatly increased.[53] More of this later.

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52 For example we might argue that even visual form and tactual solidity, perceived as properties of physical objects, are nevertheless properties imposed by the individual in accordance with a given theory built into his perceptual systems (see footnote 23). In his very interesting article "Evolutionary Epistemology" D.T. Campbell writes: "Thus the visual and tactual solidity of ordinary objects represents a phenomenal emphasis on the one physical discontinuity most usable by man neglect of other discontinuities identifiable by the probes of modern experimental physics. Perceived solidity is not illusory for its ordinary uses: What it diagnoses is one of the "surfaces" modern physics also describes. But when verified as exclusive, when creating expectations of opaqueness and impermeability to all types of probes, it becomes illusory. The different *Umwelten* of different animals do represent in part the differential utilities of their specific ecological niches, as well as differential limitations. But each of the separate contours diagnosed in those *Umwelten* are also diagnosable by a complete physics, which in addition provides many differentia unused and unperceived by any organism." (Campbell, 1974, p.448)

53 Similarly with Festinger's Social Comparison theory. Festinger's theory assumes a distinction between "social reality" and "physical reality". For Festinger, "physical reality" is a reality based on empirical observation, and "social reality" is a reality created by group consensus. In view of the arguments developed it is maintained that such a distinction cannot be sustained. (See Festinger, 1954)

Returning to Sherif; when:

> "Objective structure or order is lacking in some degree, the individual tries to give it some pattern or order. In other words, *whether* the external field is patterned or not, the psychological tendency is in the direction of some kind of pattern".
>
> Sherif and Sherif, 1969, p.56

In other words, because of a general psychological tendency to experience things in relation to some reference point or standard -

> "When such an anchorage or reference point is given in the objective situation, it will usually determine in an important way the structural relationships of the experience. All other parts are organized and modified by it. But when objective anchorages are lacking when the field of stimulation is unstable - the individual perceives the situation as shaped by his own internally evolved standards or anchorages."
>
> Sherif and Sherif, 1969, p.205

Because of its lack of any structure, Sherif decided on the basis of these ideas that the *autokinetic effect* was an ideal phenomenon with which to experimentally study the formation of norms.

> "The conditions that produce the autokinetic effect afford an excellent experimental situation. We can easily get the autokinetic effect. In complete darkness, such as in a closed unlighted room, as on a cloudy night in the open when no lights are visible, a single small light seems to move - and it may appear to move erratically in all directions. If you present the point of light repeatedly to a person, he

> may see the light appearing in different places in the room each time, especially if he does not know the distance between himself and the light. In a completely dark room a single point of light cannot be localized definitely. *There is nothing in reference* to which one can locate it."
>
> Sherif and Sherif, 1969, p.202

In such an 'unstructured' situation the individual was required to estimate the magnitude of "movement" the light passed through. Lacking any objective standard of reference the individual's estimates required a subjective determination.

The results of Sherif's experiment suggested the following. First, when confronted with an ambiguous or unstructured stimulus situation individuals develop 'norms' in terms of which they 'structure' the situation. Sherif and Sherif comment that this confirms "the tendency toward stabilization is rooted in basic psychological processes and is not a unique outcome of social interaction". (Sherif and Sherif, 1969, p.206). Second, when a person who independently develops a norm is put into a situation together with others who also enter the situation with their own norms, these norms tend to converge. Third, when a group of individuals are confronted with a novel and unstructured situation, a range and a norm (standard) within that range emerge which are peculiar to the situation. Fourth, when a member of such a group:

> "subsequently faces the same situation *alone*, *after* the range and norm of his group have been established, *he perceives the situation in terms of the range and norm that he brings from the interaction situation* ... This finding shows that the effect of the interaction situation is not just an immediate effect. The norm formed in interaction with others becomes the individual's own perspective".
>
> Sherif and Sherif, 1969, p.207

It is in this latter instance that the individual has "internalised" the norm[54] - i.e. should it be found:

> "that the common range and model print established in interaction are maintained by the individual on a different day, when he is alone, then we can say that the norm formed in interaction with others has become his own norm".
>
> Sherif and Sherif, 1969, p202

Using as a basis the assumed "general psychological principle" that experience is ordered or modified by what the Gestalt psychologists called "anchorages" - where these anchorages are supplied by an operative frame of reference - Sherif and Sherif venture to maintain that:

> "The psychological basis of established social norms - such as stereotypes, fashions, conventions, customs, and values - is the formation of common reference points or anchorages as a product of interaction among individuals. Once such anchorages are established and internalized by the individual, they become important factors in

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54 In these contexts, especially in view of the suggestions that follow, it is interesting to read Kuhn's (1962) description of the training of scientists. For example he writes: "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 range of natural phenomena; without them it could not be even a candidate for acceptance. ... If, for example, the student of Newtonian dynamics ever discovers the meanings of terms like 'force', 'mass', 'space', and 'time', he does so less from the incomplete though sometimes helpful definitions in his text than by observing and participating in the application of these concepts to problem-solution." (Kuhn, 1962, pp.46-47)

> determining or modifying his reactions to the situations that he will face later alone - social and even non-social, *especially if the stimulus field is not well-structured.* "
>
> Sherif and Sherif, 1969, p.207, my emphasis

In short, as understood by Sherif and Sherif, norms have two important functions. First, they serve to provide anchorages with which the individual might orientate himself in an otherwise unstructured and chaotic world. Second they serve as a basis on which the individual can structure his world, allowing him to observe it as being ordered and predictable. What is more, in fulfilling either of these functions norms 'determine' and 'modify' experience. Sherif writes:

> "... in the course of the life history of the individual and as a consequence of his contact with the social world around him, the social norms, customs, values, etc, become interiorized in him. These interiorized social norms enter as frames of reference among other factors in situations to which they are related, and thus dominate or modify the person's experience and subsequent behavior in concrete situations."
>
> Sherif, 1936, pp.43-44

By now it should be apparent that there exist fundamental similarities between Sherif's understanding of the nature and function of norms (standards) and the understanding we presented above of the nature and function of theories in science. First, neither norms nor theories are based on or derived from experience; rather they are what make experience possible. Second, they cannot be contradicted by experience. Third, they cannot be known to be 'right' or 'wrong', 'true' or 'false', or whatever. In fact their function is to serve as a basis on which to make such distinctions.

We are led then to suggest that:

1. Both scientific theories and norms *originate* in what Sherif sees as a general psychological tendency to experience things in relation to some reference point or standard.

2. Both share a similar *nature* in that they are not derived from, or constructed on any basis rather, both of them are hypothesized and tentative frames of reference.

3. Both of them *function* in making it possible to experience reality as structured, ordered and predictable.

These fundamental similarities are not surprising if viewed in the light of what many consider to be the origins of science as we know it - the cosmological theories of the pre-Socratic Greeks (see, e.g. Popper, 1963). Each of these theories consisted in an attempt to explain the nature of 'change'. For the Greek thinker at this time:

> "… his perception of change, of transition from life to death and from death to life, helped to lead him, in the person of the Ionian philosophers, to a beginning of philosophy; for these wise men saw that, in spite of all the change and transition, there must be something which is primary, which persists, which takes various forms and undergoes this process of change. Change cannot be merely a conflict of opposites; thoughtful men were convinced that there was something behind these opposites, something that was primary. Ionian philosophy or cosmology is therefore mainly an attempt to decide what this primitive element or *Urstoff* of all things is, one philosopher deciding for one element, another for another element each

> philosopher decided on as his *Urstoff* is not so important as the fact that they had in common this idea of Unity."
>
> Copleston, 1946, pp.l9-20

And, in Popper's words:

> "For all change is the change of something: change presupposes something that changes. And it presupposes that, while changing, this something must remain the same. We may say that a green leaf changes when it turns brown, but we do not say that the green leaf changes when we substitute for it a brown leaf. It is essential to the idea of change that the thing which changes retains its identity while changing. And yet it becomes something else: it was green, and it becomes brown; it was moist, and it becomes dry; it was hot and it becomes cold. This is the problem of change. It led Heraclitus to a theory which (partly anticipating Parmenides) distinguishes between reality and appearance. 'The real nature of things loves to hide itself. An unapparent harmony is stronger than the apparent ones.' Things are *in appearance* (and for us) opposites, but in truth (and for God) they are the same."
>
> Popper, 1963, pp.144-145

The world as it was presented to the senses - the world of "appearance and illusion" - was a world where irregularities abounded, where approximation, imperfection, variation, emergence and chance were the order of the day. For Parmenides and Plato this meant that true knowledge must reside in knowledge of a world that stood apart from the world of appearances, of a world in which there was no change, no irregularity and no imperfection. True knowledge consisted of a knowledge of "invariants".

For Leucippus and Democritus these invariants were atoms and the change

manifest in the world of appearances could be accounted for in terms of these invariants. In this fashion Leucippus and Democritus restored order and regularity to the world of appearances. In short, we have revealed in those early cosmological theories of the pre-Socratics what is still considered the fundamental function of scientific theories - an ordering function. By introducing order the theory makes possible the observation of regularities, which in turn make possible explanation and prediction (Nagel, 1961; Popper, 1958; Hanson, 1958; Braithwaite, 1953).

We have maintained that the socio-psychological nature and function of scientific theory is basically the same as the socio-psychological nature and function of norms. At this point we could stop and proceed to see in what sense research on the social psychology of norms could contribute to our understanding of developments in science. However it is considered that a more fruitful approach is possible.

This chapter began with a quote by Berger. As a sociologist Berger[55] has further developed the ideas central to Sherif's understanding of the nature and function of group norms. Having claimed the fundamental functional equivalence between scientific theory and psychosocial norms the author believes that sociological theories accounting for the nature and function of the norms will afford insights into the workings of the scientific enterprise. Berger's theory constitutes one such theory. It is maintained that in terms of his views it is possible to conceptualise, from a socio-psychological - cum - sociological viewpoint, facets of both science as an institution and the behaviour of scientists *qua* scientists. Attempts will be made to illustrate the possibilities of this.

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55 In presenting Berger's views, we also draw on material contained in the book he co-authored with Thomas Luckmann, titled *The Social Construction of Reality,* (New York: Doubleday, 1966).

In the introductory quote Berger states "the ordering of experience is endemic to any kind of social interaction". The reason Berger places such an emphasis on this ordering or nomizing activity derives from his assumption that man is "biologically denied the ordering mechanisms with which the other animals are endowed." That is, man is not born with a set of phylogenetically programmed behaviours.[56] It is the resulting flexibility of man's behaviour that makes him unique among the animal kingdom.

> "Man occupies a peculiar position in the animal kingdom. Unlike the other higher mammals, he has no species - specific environment, no environment firmly structured by his instinctual organization. There is no man-world in the sense that one may speak of a dog-world, or a horse-world. Despite an area of individual learning and accumulation, the individual dog or the individual horse has a largely fixed relationship to its environment, which it shares with all other members of its respective species. One obvious implication of this is that dogs and horses, as compared with man, are much more restricted to a specific geographical distribution. The specificity of these animals' environments, however, is much more than a geographical delimitation. It refers to the biologically fixed character of their relationship to the environment, even if geographical variation is introduced. In this sense, all nonhuman animals, as species and as individuals, live in closed worlds whose structures are predetermined by the biological equipment of the several animal species.
>
> By contrast, man's relationship to his environment is characterized by a world-openness. ... This does not mean, of course, that there are no biologically determined limitations to man's relations with his environment; his species-specific sensory and motor equipment imposes obvious limitations on his range of possibilities".
>
> Berger and Luckmann, 1966, p.65

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56 While it is probably true that the phylogenetic determinants of human behaviour allow more scope for learning than in other species, it is unlikely that man's behaviour is not subject to any phylogenetic controls. While Berger might somewhat over-simplify the position the author considers that this does not undermine the validity of his approach.

The fact that at the time of birth the human infant is still developing biologically means that he has a peculiarly unfinished character.

> "The unfinished character of the human organism at birth is closely related to the relatively unspecialised character of its instinctual structure. The non-human animal enters the world with high, specialized and firmly related drives. As a result it lives in a world that is more or less completely determined by its instinctual structure. ... Like the other animals, man is in a world that antedates his appearance. But unlike other animals this world is not simply given, prefabricated for him. Man must *make* a world for himself ... Only in such a world produced by himself can he locate himself and realize his life. But the same process that builds his world also "finishes" his own being. In other words, man not only produces a world, but he also produces himself. More precisely he produces himself in a world."
>
> Berger, 1967, pp.5-6

Berger equates the product of man's world-building activity with culture and sees the purpose of culture as one of providing man with a stable and structured environment in which to be born, to live, and die.

> "Biologically deprived of a man-world, man constructs a human world. This world, of bourse, is culture. Its fundamental purpose is to provide the firm structures that are lacking biologically ... Culture consists of the totality of man's products. Some of these are material, others are not ... society is, of course, nothing but part and parcel of non-material culture. Society is that aspect of the latter that structures man's ongoing relations with his fellow man. As but an element of culture, society fully shares in the latter's character as a human product."
>
> Berger, 1967, pp.6-7

Of fundamental importance to Berger's views is his belief that the relationship between man and society is a dialectical[57] one. Not only is society a product of human activity, but also human activity is itself determined by society.

> "Society is a dialectical phenomenon in that it is a human product, and nothing but a human product, that yet continuously acts back upon its product. Society is a product of man. It has no other being except that which is bestowed upon it by human activity and consciousness. There can be no social reality apart from man. Yet it may also be stated that man is a product of society. "
>
> Berger, 1967, p.3

The dialectic between the individual and society, as with all dialectical processes, consists of three basic moments[58] or steps.

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57 In their discussion of the nature of this dialectical process, Berger and Luckmann compare it with Karl Marx's understanding of the dialectic between what he terms "substructure" and "superstructure". They write: "It is here particularly that controversy has raged about the correct interpretation of Marx's own thought. Later Marxism has tended to identify the 'substructure' with economic structure *tout court* of which the 'superstructure' was then supposed to be a direct 'reflection' (thus Lenin, for instance). It is quite clear now that this misrepresents Marx's thought, as the essentially mechanistic rather than the dialectical character of this kind of economic determinism should make one suspect. What concerned Marx was that human thought is founded in human activity ('labour', in the widest sense of the word) and in the social relations brought about by this activity. 'Substructure' and 'superstructure' are best understood if one views them as, respectively, human activity and the world produced by that activity". (Berger and Luckmann, 1966, p.18)

58 The three moments characteristic of dialectical processes are (i) the thesis, (ii) the antithesis, and (iii) the synthesis. The final moment - the synthesis - consists of a development which resolves the conflict introduced when the antithesis rises to confront the thesis.

> "These are externalization, objectivation, and internalization. Externalization is the ongoing outpouring of human being into the world, both in the physical and mental activity of man. Objectivation is the attainment by the products of this activity (again both physical and mental) of a reality that confronts its original producers as a facticity external to and other than themselves. Internalization is the reappropriation by men of this same reality, transforming it once again from structures of the objective world into structures of the subjective consciousness. It is through externalization that society is a human product. It is through objectivation that society became a reality *sui generis.* It is through internalization that man is a product of society".
>
> Berger, 1967, pp.3-4

To illustrate this dialectical process in simple form we might return to Sherif's autokinetic experiment. When first confronted with this 'unstructured' situation, individuals in the group condition each responded in the absence of any obvious constraints. These responses of each individual could be viewed as instances in which the individual 'externalised' himself. The emergence of the group norm constitutes an instance of 'objectivation'. That individuals carried this norm with them from the group setting to situations requiring them to act alone demonstrated that the norm had become 'internalised'.

Earlier it was suggested that scientific theories are nothing other than more or less sophisticated normative systems. Further to this it is now suggested that the relation between man and science is dialectical. In the light of Berger's proposals regarding the dialectical relationship between the individual and society we suggest the following analysis. First, the theorizing and research of individual scientists consists of *externalising* activities. Second, the emergence of scientific 'theories', 'laws', 'principles', etc, evidences *objectivation*. Third, in that the individual scientist learns to see the world and understand it in terms of the objectivated norms prevailing in a given scientific community, he has *internalised* this normative system.

Having set the stage we now proceed to highlight a number of instances using Berger's views to understand aspects of science and its practice from a socio-psychological - cum - sociological standpoint.

In Chapter 4 we introduced Hanson's (1958) notion of a 'conceptual Gestalt', suggesting an analogy between reversible Gestalt figures as imposed perceptual patterns and scientific theories serving a similar patterning function. In Berger's terminology we might say that competing scientific theories - e.g. classical and relativistic physics - constitute alternative 'meaning systems' between which it is possible to 'alternate' in one's conceptualisation of phenomena.

Berger's concept of a 'meaning system' derives largely from his understanding of how the internalising of social norms determines the individual's understanding of himself and his world. In other words, Berger maintains that it is the norms which an individual internalises during the socialization process that determines not only his interpretation of 'objective reality' but also his interpretation of 'subjective reality'.

We might cite Catholicism and Communism as examples of highly developed meaning systems. By an examination of either of these two systems, one can gain

> "a truly frightening understanding of the way in which these systems can provide a total interpretation of reality, within which can be included an interpretation of the alternate systems and of the ways of passing from one system to another. Catholicism may have a theory of Communism, but Communism returns the compliment and will produce a theory of Catholicism. To the Catholic thinker the Communist lives in a dark world of materialist delusion about the real meaning of life. To the Communist, his Catholic adversary is helplessly caught in the 'false' consciousness of a bourgeois mentality.

> To the psychoanalyst both Catholic and Communist may simply be acting out on the intellectual level the conscious impulses that really move them. And psychoanalysis may be to the Catholic an escape from the reality of sin and to the Communist an avoidance of the realities of society".
>
> Berger, 1963, pp.64-65

The similarities between Berger's alternate 'meaning systems' and competing theories in science are obvious. Most importantly they both function to locate all the phenomena encompassed in their respective domains of application within some organized framework of understanding.

This leads to our second example of how Berger's views on society apply to an understanding of the socio-psychological nature and function of science.

With the experience of alternation one develops a dawning realization of the *relativity* of alternate meaning systems and consequently a realization of their precariousness. To recognize this precariousness is to come face to face with the threat of *anomie* (Durkheim) - the state of disorder, meaninglessness and chaos. But the very function of society is to introduce stability, order, meaningfulness. It is for this reason that we find in the more fully developed meaning systems tools to combat any doubts about their 'correctness'. For example, Catholic confessional discipline, Communist 'auto-criticism' and the psychoanalytic techniques of coping with 'resistance' all fulfil this same purpose of preventing alternation out of a particular meaning system and allow the individual to interpret his own doubts in terms derived from the system itself, thus keeping him within it.

Because socially constructed meaning systems have the purpose of introducing

stability into an otherwise unstable world, it is better that they remain unquestioned and unexposed to criticism.

> "The social world intends, as far as possible to be taken for granted. Socialization achieves success to the degree that this taken-for-granted quality is internalized. It is not enough that the individual look upon the key meanings of the social order as useful, desirable, or right. It is better (better, that is, in terms of social stability) if he looks upon them as inevitable, as part and parcel of the universal "nature of things". If that can be achieved, the individual who strays seriously from the socially defined programmes can be considered not only a fool or a knave, but a madman. Subjectively then, serious deviance provokes not only moral guilt but the terror of madness."
>
> Berger, 1967, pp.24-25

Now just as Berger maintains that "socialization achieves success to the degree that a taken for granted quality is internalised", if Kuhn (1962, 1970) is right, then the education of scientists is a success to the extent that its fundamental tenets are internalised with a similar taken for granted quality. Further, just as the questioning of social mores, laws, attitudes, etc, constitutes an attack on the foundations on which a given social system is built and relies for its existence, so criticism of fundamentals in science limits the effectiveness of its taken-for-granted assumptions in their function as providing a basis for normal scientific research.

> "With respect to normal science, then, part of the answer to the problem of progress lies in the eye of the beholder. Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see. That, however, is only a part of the answer and by no means the most important part. We have, for example, already noted that once the reception of a common paradigm has freed the scientific community from the need constantly to re-examine its first principles,

> the members of that community can concentrate exclusively upon the subtlest and most esoteric of the phenomena that concern it. Inevitably, that does increase both the effectiveness and the efficiency with which the group as a whole solves new problems."
>
> Kuhn, 1962, pp.163-164

Furthermore,

> "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. Other problems, including many that had previously been standard, are rejected as metaphysical, as the concern of another discipline, or sometimes as just too problematic to be worth the time."
>
> Kuhn, 1962, p.37

Not only do social and scientific communities avoid questions directed at an examination of the fundamental adequacy of their respective bases, but also they actively discourage such questions. This is-seen in that those whose behaviour violates social norms are judged deviant.[59] Similar social sanctions are embodied in the use of such judgemental concepts as 'adjusted', 'mature', 'sane', etc.

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59 Defining conforming and deviating behaviour, Sherif and Sherif write: "conforming behaviour or deviating behaviour means that the behavior in question falls within the latitude of acceptance or within the latitude of rejection defined by a norm (or set of norms) prevailing in a social unit of which the individual is defined as a member with some role and status in the organizational scheme." (Sherif and Sherif, 1969, p.191)

> "Every one of these terms - 'adjustment', 'maturity', 'sanity', and so on - refer to socially relative situations and become meaningless when divorced from these. One adjusts to a particular society. One matures by becoming hibituated to it. One is sane if one shares its cognitive and normative assumptions".
>
> Berger, 1963, p.79

This suggests a third way in which science manifests a basic likeness to society at large. We have seen that for Kuhn most of the history of science consists in periods of 'normal' science. Leaving aside the question as to whether Kuhn is correct in characterizing the science practiced in these periods as being essentially monolithic, it would be difficult to deny the accuracy of his view that in these periods science proceeds without any significant degree of questioning of fundamentals. In these periods there exist scientific communities whose existence is made possible because of a "strong network of commitments conceptual, theoretical, instrumental, and methodological". (Kuhn, 1962, p.42) - i.e., a paradigm. But paradigmatic science is 'mature' science.

Commenting on 'maturity' from a socio-psychological standpoint, Berger writes:

> "Maturity is the state of mind that has settled down, come to terms with the *status quo* given up the wilder dreams of adventure and fulfilment. It is not difficult to see that such a notion of maturity is psychologically functional in giving the individual a rationalization for having lowered his sights. ... In other words, we would contend that the notion of maturity really begs the question of what is important and what is unimportant in one's biography. What may look like mellow maturity from one point of view may be interpreted as cowardly compromise from another."
>
> Berger, 1963, pp.69-70

The usual interpretation of Einstein's and Plank's unwillingness to accept the Copenhagen interpretation of the quantum theory readily demonstrates the truth of the last statement in this passage.

So we could go on identifying such similarities. Yet the author considers ample illustrations have been given to make apparent the content of the view sought to be put forward in the present chapter - viz., that science, like society, is essentially a world-building enterprise. Its socio-psychological *origins* are, as with norms in general, rooted in man's general psychological tendency to pattern or structure his experience. The socio-psychological *nature* of scientific theories is evidenced in their status as hypothetical frameworks which *function* to enable the individual to adapt his behaviour to an otherwise unstructured and chaotic world.

To summarize - this chapter was aimed at presenting a socio-psychological theory of science. It began with a presentation of the ideas of Sherif on the social-psychology of norms.

Using Lorenz's ideas regarding phylogenetic determinants of perception as a basis, it was argued that Sherif's ideas could be modified to bring them more into line with the general argument of earlier chapters. It was suggested that in so doing, it was possible to extend Sherif's views on the nature and function of norms to provide a socio-psychological analysis of the nature and function of theories in science. An attempt was then made to clarify this view by illustrating it with a look at the early cosmological theories of the pre-Socratic Greeks. The suggestion was then made that the viewpoint of Peter Berger could be considered as a substantial development of the approach taken by Sherif and that in terms of Berger's theory it was possible to recognize fundamental similarities between the nature and functioning of science, and the nature and functioning of society - both constitute world-building enterprises.

CHAPTER 6

SOCIAL PSYCHOLOGY AND SCIENTIFIC CHANGE

> The more solid, well defined, and splendid the edifice erected by the understanding, the more restless the urge of life ... to escape from it into freedom.
>
> Hegel

> "Once again it was driven home to me how terribly difficult it is to give up an attitude on which one's entire scientific approach and career have been based. Einstein had devoted his life to probing into that objective world of physical processes which runs its course in space and time, independent of us, according to firm laws. The mathematical symbols of theoretical physics were also symbols of this objective world and as such enabled physicists to make statements about its future behaviour. And now it was being asserted that, on the atomic scale, this objective world of space and time did not even exist and that the mathematical symbols of theoretical physics referred to possibilities rather than to facts. Einstein was not prepared to let us do what, to him, amounted to pulling the ground from under his feet. Later in life, also, when quantum theory had long since become an integrated part of modern physics, Einstein was unable to change his attitude - at best, he was prepared to accept the existence of quantum theory as a temporary expedient. "God does not throw dice" was his unshakeable principle, one that he would not allow anybody to challenge. To which Bohr could only counter with: "Nor is it our business to prescribe to God how He should run the world."
>
> Werner Heisenberg: *Physics and Beyond*

In the introduction it was pointed out that the popular story surrounding the famous tower of Pisa demonstration of Galileo's law of free fall is largely myth. In fact it was suggested that such a demonstration would have supported the Aristotelian views - i.e., the views of Galileo's opponents. Furthermore it was maintained that in the great debate between Aristotelians and Copernicans as to whether the earth or the sun was at the centre of the universe, the Aristotelians could appeal to everyday observations as a source of evidence supporting their views.

Needless to say the difficulties confronting the Copernican theory were not all removed with Galileo's reinterpretation of the observed trajectory of falling objects. The Aristotelians marshalled many and diverse arguments purporting to establish the

superiority of their geocentric theory. These included arguments ranging from principles in dynamics[60], physics[61], theoretical astronomy[62], cosmology[63], to optical considerations[64], accuracy of tables[65], and son on. Summing up the general position

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60 In Aristotelian physics, any horizontal or non-vertical motion is 'violent' motion and requires the existence of some external force to sustain it. Consequently if the earth is moving through space then it must be moved along by some external force. The Aristotelian's queried the nature and origin of this force. For Copernicus' reply, see Koestler, 1959, p.199.

61 Again, from the Aristotelian point of view, the 'natural' motion of sub-lunar bodies is vertically upwards for fire, vertically downwards for water and earth. Such motion was explained on the assumption that the earth constituted the centre of the universe. By removing the earth from the centre of the universe, Copernicus had destroyed such explanations. For Copernicus' reply to this objection, again see Koestler, 1959, p.199.

62 By the time Copernicus had modified his system to give it sufficient power to cope with the observed motions of all the planets it became more complicated and clumsy than the system he set out to replace. See Koestler, 1959, pages 195 and 198, and Kuhn, 1957, p.172. Koestler points out that contrary to popular belief, Copernicus did not reduce the number of circles, but increased them.

63 In removing the earth from the centre of the universe, Copernicus undermined the basis of the cosmological thinking of his time. Perhaps the most complete presentation of all that this cosmology represented to the views of the ordinary man of that day is found in Dante's *The Divine Comedy*.

64 Peyerabend writes: "According to the Copernican theory, Mars and Venus approach to and recede from the earth by a factor of 1:6 and 1:8 respectively (these are approximate numbers). Their change of brightness should be 1:40 and 1:60 respectively (these are Galileo's values). Yet Mars changes very little, and the variation in the brightness of Venus "is almost imperceptible". These experiences overtly contradict the annual movement of the earth." (Feyerabend, 1970a, p.290. He quotes Galileo.)

65 Koestler comments: "From the seafarers' and the stargazers' point of view, the Copernican planetary tables were only a slight improvement on the earlier Alphonsine tables, and were soon abandoned." (Koestler, 1959, p.126) For Kuhn (1957), the Copernican system "did not give more accurate results." (Ibid, p.169)

regarding the Copernican point of view at the time of these arguments, Kuhn writes:

> "In the early decades of the seventeenth century it was at best an astronomical innovation. Outside astronomy it raised a host of problems just as perplexing and far more obvious than the questions of numerical detail it had resolved. Why do heavy bodies always fall toward the surface of a spinning earth as the earth moves in its orbit about the sun? How far away are the stars, and what is their role in the structure of the universe? Copernican astronomy destroyed traditional answers to these questions, but it supplied no new substitutes. A new physics and a new cosmology were required before astronomy could again participate in a unified pattern of thought."
>
> Kuhn, 1957, p.230

As pointed out above, the difficulties confronting the Copernican view were so great that one hundred years after it was first announced, Galileo was led to describe it as "surely false".

In view of the many disadvantages that the Copernican theory displays when compared to the then prevailing Aristotelian-Ptolemaic theory, we might well ask how Copernicus could continue to maintain his belief in it. In an attempt to answer this question attention is first directed towards an examination of what led Copernicus to postulate his theory in the first place.

It is in the opening passage of his *Commentariolus* that we find Copernicus makes explicit the reasons that led him to entertain the hypothesis of the heliocentric universe. He writes:[66]

"Our ancestors assumed, I observe, a large number of celestial spheres for this reason especially, to explain the apparent motion of the planets by the principle of regularity. For they thought it altogether absurd that a heavenly body, which is a perfect sphere, should not always move uniformly. They saw that by connecting and combining regular motions in various ways they could make anybody appear to move in any position …

Yet the planetary theories of Ptolemy and most other astronomers, *although consistent with numerical data* seemed likewise to present no small difficulty. For these theories were not adequate unless certain equants were also conceived; it then appeared that *a planet moved with uniform velocity neither on its deferent nor about the centre of its epicycle Hence a system of this sort seemed neither sufficiently absolute nor sufficiently pleasing to the mind.*

Having become aware of these defects, I often considered whether there could be found a more reasonable arrangement of circles, from which every apparent inequality would be derived and in which everything would move uniformly about its proper center, as the rule of absolute motion requires. After I had addressed myself to this very difficult and almost insoluble problem, the suggestion at length came to me how it could be solved with fewer and much simpler constructions than were formerly used, if some assumptions (which are called axioms) were granted me."

Copernicus, cited in Rosen, 1959, pp.57-58, my emphasis

What is interesting about this passage is that nowhere does it contain a reference to any observational inadequacies in the Aristotelian-Ptolemaic system; indeed Copernicus himself proclaims that this system is *consistent* with the numerical data.

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66 This quote is taken from the translation by E. Rosen – see his *Three Copernican Treatises,* New York: Dover Publications, Inc. 1959, p.57.

Here we have another clear instance where consideration of historical detail radically challenges the tenability of popular notions concerning the methods of science, and in particular the story surrounding the origins of the Copernican revolution. Such stories would have us believe that Copernicus developed his theory in an attempt to provide a system that was not confronted with the serious observational difficulties ostensibly refuting the Aristotelian-Ptolemaic theory. In fact Copernicus did not challenge the consistency of the Aristotelian-Ptolemaic theory with what was observed but also as a theoretician he himself was not given to carrying out observations and the collecting of *new* observational evidence. In drawing attention to this Koestler writes:

> "Canon Koppernigk was not particularly fond of stargazing. He preferred to rely on the observations of Chaldeans, Greeks, and Arabs - a preference that led to some embarrassing results. *The Book of the Revolutions* contains, altogether, only twenty-seven observations made by the Canon himself; and these were spread over thirty-two years ... All in all, Canon Koppernigk noted down between sixty and seventy observations in a lifetime. He regarded himself as a philosopher and mathematicus of the skies, who left the actual work of stargazing to others, and relied on the records of the ancients."
>
> Koestler, 1959, pp.125-126

Indeed, Copernicus himself was the last person to question the accuracy of the observations of his predecessors. In reply to Johannes Werner, the Nuremberg mathematician who questioned the reliability of certain observations by Ptolemy and Timocharis, Copernicus states with venom:

> "It is fitting for us to follow the methods of the ancients strictly and to hold fast to their observations which have been handed down to us like a Testament. And to him who thinks that they are not to be entirely trusted in this respect, the gates of our science are certainly

> closed. He will lie before that gate and spin the dreams of the deranged about the motion of the eighth sphere; and he will get what he deserved for believing that he can lend support to his own hallucinations by slandering the ancients."
>
> Cited in Koestler, 1959, p.203

In short, Copernicus assumed that the traditional observations were satisfactory and believed the Aristotelian-Ptolemaic theory to be consistent with these observations. Consequently the idea that Copernicus proposed his heliocentric astronomy to provide an alternative more in agreement with observational evidence than the Aristotelian-Ptolemaic geocentric astronomy is clearly false. Copernicus' reasons for proposing his theory were motivated by a dissatisfaction he experienced with regard to what he viewed as an inconsistency fundamental to the Aristotelian-Ptolemaic approach. Briefly, this inconsistency stems from the fact that: (1) Aristotelian physics demanded that the motion of heavenly bodies was 'uniform circular motion'. In other words, motion around a circular path with uniform speed. (2) Ptolemy's system of astronomy required the notion of an equant - an equant being a point removed from the centre of the circle (prescribed by the planet in orbit around the earth) from where the planet would be observed as having constant *angular* velocity. Copernicus correctly pointed out that the introduction of the equant to describe the motion of the planets was inconsistent with Aristotle's assumption that the motion of the planets was uniform circular motion. To Copernicus such a system "seemed neither sufficiently absolute nor sufficiently pleasing to the mind". In short it was the *incoherence* of the Aristotelian-Ptolemaic astronomy for Copernicus that constituted its weakness and led him to look for an alternative.

It was his pre-occupation with this very same problem that resulted in Copernicus overlooking the problems raised by his own heliocentric views. As noted above, in allowing the earth to be set in motion Copernicus' own theory created serious problems for cosmology, dynamics, physics, and even theoretical astronomy itself –

along with creating a host of observational difficulties. Discussing this point Kuhn writes:

> "Copernicus displayed a similar indifference to cosmological detail when he failed to note the incongruities of a moving earth in an otherwise traditional universe. For him, mathematical and celestial detail came first, he wore blinders that kept his gaze focused upon the mathematical harmonies of the heavens. To anyone who did not share his speciality, Copernicus' view of the Universe was narrow and his sense of values distorted.
>
> But an excessive concern with the heavens and a distorted sense of values may be essential characteristics of the man who inaugurated the revolution in astronomy and cosmology. The blinders that restricted Copernicus' gaze to the heavens may have been functional. ... They gave him an eye so absorbed with geometrical harmony that he could adhere to his heresy for its harmony alone, even when it had failed to solve the problem that had led him to it. And they helped him evade the non-astronomical consequences of his innovation, consequences that led men of less restricted vision to reject his innovation as absurd."
>
> Kuhn, 1957, p.184

Returning to the socio-psychological analysis of scientific theory presented in the preceding chapter, let us re-conceptualise the nature of Copernicus' innovation from this standpoint. Viewing scientific theories as normative systems of greater or less complexity, it is suggested that Aristotelian-Ptolemaic astronomy constituted such a normative system. In Hanson's terms this theoretical system constitutes one particular structure was not "sufficiently absolute" nor was it "sufficiently pleasing to the mind". This was because when carefully examined this structure revealed itself to be based on incongruent assumptions - the Aristotelian notion that the motion of the planets is uniform circular motion and Ptolemy's notion that planets move with uniform *angular* velocity about an equant.

We have already seen that like Durkheim, Sherif claimed that it is in 'unstructured' or 'problematic' situations that norms evolve. In view of their inconsistency it is clear that any domain where Aristotle's 'uniform circular motion' principle operates in conjunction with Ptolemy's theory of motion about an equant will not present itself as well structured and orderly. Copernicus' recognition that the Aristotelian-Ptolemaic view presented a basically *incoherent* structure served to set the stage for his development of an alternative system of norms generating a structure more "pleasing to the mind" - viz, his heliocentric theory. If nothing else, his theory dispenses with the equants and so promised the possibility of a more coherent structure. Thus, the Aristotelian-Ptolemaic viewpoint presented one conceptual Gestalt and the Copernican theory another.

Now as noted the Aristotelian-Ptolemaic system was in better agreement with observation than the Copernican system and in this respect was clearly superior to the Copernican view. Yet it was inferior to the Copernican view in that it relied on two mutually inconsistent assumptions. In short, there were advantages and disadvantages in both approaches. Commenting on the implications of this Kuhn writes:

> "Judged on purely practical grounds, Copernicus' new planetary system was a failure; it was neither more accurate nor significantly simpler than its Ptolemaic predecessors. But historically the new system was a great success; the *De Revolutionibus* did convince a few of Copernicus' successors that sun-centered astronomy held the key to the problem of the planets, and these men finally provided the simple and accurate solution that Copernicus had sought. ... in the, what reasons were there for transposing the earth and the sun? The answer to this question is not easily disentangled from the technical details that fill the *De Revolutionibus* because, as Copernicus himself recognized, the real appeal of the sun-centred astronomy was aesthetic rather than pragmatic. To astronomers the initial choice between Copernicus' system and Ptolemy's could only be a matter of taste, and matters of taste are the most difficult of all to define or debate. Yet, as the Copernican Revolution itself indicates, matters of taste are not negligible. The ear equipped to discern geometric harmony could

> detect a new neatness and coherence in the sun-centred astronomy of Copernicus, and if that neatness and coherence had not been recognized, there might have been no revolution at all."
>
> Kuhn, 1957, pp.171-172

Emphasizing the role of subjective factors as major determinants in the scientist's choice between alternative theories Kuhn feels constrained to ask "How do the scientists make the choice between competing theories? How are we to understand the way in which science does progress?" (Kuhn, 1970, p.19.) Considering himself to be ignorant of the answers to these questions Kuhn nevertheless believes he sees the directions in which such answers might be sought.

> "Until we can answer more questions like these, we shall not know quite what scientific progress is and therefore cannot hope to explain it. ... Already it should be clear that the explanation must, in the final analysis, be psychological or sociological. It must, that is, be a description of the value system, an ideology, together with an analysis of the institutions through which that system is transmitted and enforced. Knowing what scientists value, we may hope to understand what problems they will undertake and what choices they will make in particular circumstances of conflict. I doubt that there is another sort of answer to be found."
>
> Kuhn, 1970, p.21

The preceding chapter advanced the suggestion that scientific theories have an identical nature and function to psychosocial norms and that both originate in what Sherif and Sherif (1969) view as the universal psychological characteristic of human beings to perceive their environment in terms of distinct structures or patterns. The author contends it is possible to use this suggestion as a basis for a new approach to the study of scientific change. In brief it is submitted that just as scientific theories can be

viewed as complexes of psychosocial norms, so scientific change can be conceived in terms of the evolution of psychosocial norms. So understood, the author believes that there already exist socio-psychological theories that contribute answers, even if incomplete, to the questions Kuhn raises.

Maintaining that conceptual and other developments internal to science can be accorded a socio-psychological explanation the approach adopted here is to be distinguished from what Whitley (1972) has called 'black box' theories of science. Drawing attention to the fact that "the link between the philosophy and history of science is becoming stronger as it is realized that theories of scientific knowledge interact with histories of scientific knowledge" (Whitley, 1972, p62), Whitley maintains that current sociology of science ignores the theorizing activities of scientists *qua* scientists. Citing the studies of Storer (1966) and Cole (1970) as two recent examples of research characteristic of current approaches to sociology of science in North America, Whitley writes:

> "These approaches ... exclude any discussion of the subject matter of science. Ignoring the cognitive aspects of scientists' activities, they restrict sociology to a discussion of social relations and processes. Ideas are taken as given, they are objectified as citation or paper counts where each paper is taken to be of equal importance.
>
> By assuming that the cognitive aspect is non-problematic for sociology, the sociologists of science have implicitly adopted a view of scientific knowledge. If all ideas and discoveries are seen as basically the same, if social processes are assumed to have no bearing on what is discovered or how it is discovered, then scientists are assumed to be perfectly rational in their cognitive activity. This perfect rationality assumes that there is but one way of understanding the world and that every scientist knows his method and can apply it. Application of this method necessarily results in scientific knowledge and so a scientist is not defined by his production of a particular kind of cultural artefact but his practice of *the* scientific method. Scientific knowledge is thus demarcated from non-scientific knowledge by those who produced it. The Sociology of Science in this view, is the study

> of who practices *the* scientific method, how they learn it and what rewards they receive. The nature of this scientific method seems to consist principally of diligent, systematic observation of nature while eschewing speculation (Merton, 1968, pp.628-637). Knowledge is the accumulation of such observations which provide the basis for 'empirically confirmed and logically consistent predictions'. (Merton, 1968, p606.)"
>
> Whitley, 1972, p.62

In short, it is the assumption that there exists a unique method called *the* scientific method which makes possible a unique body of knowledge constituting "scientific knowledge" that Whitley considers is responsible for much of the sociology of science treating science as a 'black box'. Unwilling to look inside science to examine its theories, ideas, and methods from a sociological viewpoint, the 'black-box' approach "restricts research to the study of currently observable inputs to, and outputs from, a system". (Whitley, 1972, p.63.) Viewing the validity of even scientific knowledge as entirely relative to a particular community, Whitley advocates "a 'translucid box' sociology of science which seeks to answer the question: how do social and cognitive factors interact to produce knowledge and what effect do different forms of scientific knowledge have on society?" (Whitley, 1972, p.64)

Arguing that conceptual and other developments in science can be explained within a socio-psychological framework, the author is advocating - to use Whitley's terminology - a 'translucid box' approach to the social psychology of science. Furthermore, in submitting the view that scientific change can be conceptualised in terms of the evolution of psychosocial norms, it is contended that there already exist socio-psychological theories (Tajfel, 1972; Thibaut and Kelley, 1959; Lemaine, 1974) providing possible insights into the social psychology of scientific change. Introducing and briefly outlining one of these theories an attempt will be made to illustrate how such theories might contribute a socio-psychological understanding of innovation in science.

The French social psychologist Gerard Lemaine has developed a theory of social differentiation and social originality based on assumptions similar to those fundamental to Darwinian evolutionary theory:

> "For Darwin the diversity of the inhabitants of the same region is to be explained by natural selection (the survival of the fittest), that is to say, the preservation of favourable individual differences and variations and the elimination of harmful variations. As the struggle being referred to here is a struggle for survival in an environment possessed of insufficient resources, it is easy to see the advantages that are to be gained in diversity of structure amongst the inhabitants of the same region. In a word, Darwinian competition is above all competition between close relations, and natural selection comes into play when, in a given region, there exist what might be called *vacant places* which can be better occupied when some of the existing inhabitants have undergone certain modifications. The central idea of the present article is that these 'vacant places' are the product of the activity of social agents in competition in an environment possessed of scant resources. Diversity is no longer considered the result of favourable 'variations' but as the outcome of more or less conscious strategies."
>
> Lemaine, 1974, p.19

In other words, in over-populated regions of scant resources, individuals actively create 'vacant places' into which they may move and thereby increase their chances of survival. The analogy between biological and social survival mechanisms holds even further.

> "Darwinian ideas have found an application in modern ecological research. Lorenz (1966) shows that one of the roles of intra-specific aggression is to distribute individuals of the same species in space, in different 'ecological niches', that is to say, in the space necessary for these individuals to live and survive. The struggle for life leads therefore to a distribution of homologous individuals

> such that they will be able to survive. In the field which concerns us we may surmise that this 'distancing' will not take place without activities being modified and, consequently, in the social agents becoming progressively heterogeneous."
>
> Lemaine, 1974, p.19

From a more explicitly socio-psychological standpoint, Lemaine states his hypothesis as follows:

> "... reference to others leads to differentiation when it results in a threat to identity and that re-establishment of this identity is achieved by way of the search for difference, for *otherness* by the creation and subsequent accentuation of heterogeneity."
>
> Lemaine, 1974, p20

Although the following quote is lengthy this author considers its inclusion justified in that it provides a convenient presentation of Lemaine's ideas.[67]

> "Pitts (1969) in an article on the hippies sees this movement as an expression of the concern of certain young people, brought up on the values of the middle class from which they originate, to escape failure and the resultant misfortune which fate usually holds in store for them by refusing the criteria of success and the pressures of the dominant meritocracy (which leave little room for the possibility of there being any excuses for failure), and by creating or moving to zones of life in which success abounds and is no longer a rarity (obviously the term success here takes on a different meaning), to zones in which everyone can have a unique position and escape classification, where everyone, as he says, can win.

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67 It goes without saying that whether or not the analysis in the illustration is correct it is irrelevant to an assessment of Lemaine's own views.

> … it emerges that ... social agents who take the initiative with regard to differentiation are, as it were, repudiated by other social agents who are their 'superiors', in the social space such as it is defined by the dominant agents, they do not enjoy the positions to which they are entitled or, more precisely, they always find themselves in auxiliary or subordinate positions and are thus in a situation which tends to eliminate them from the social interplay of the agents with whom they compare themselves. In other words, a comparison which reveals to those who make it that their situation is a hopeless one, that they occupy, in relation to agents who are not essentially different, positions which cast doubt on their own identities or which do not allow them to attain that to which they consider themselves entitled, makes them want to mark themselves off from those with whom they compare themselves and to create for themselves codes, activities and dimensions of being or doing which make them different, other and, it may be said, non-comparable.
>
> … To compare ourselves with someone on the basis of set criteria is to take stock of that which separates us from him and also to form an opinion concerning the possibility of occupying a place, a position on which a social premium has been set by the agents of the reference system or system of affiliation. It is this position which defines at least partially the identity to which one aspires, and one can only aspire from such a time as one is a member, and sometimes simply a potential member, of the system.
>
> If the comparison results in a feeling of inferiority conceived here simply as a pessimistic evaluation of the chances of attaining that which is valued and coveted, of succeeding in occupying the position or positions which are the recognized definition of 'existence' in the system, then this inferiority feeling is the source, in certain conditions, of strategies of compensation, to use a term dear to Adler. ... But these strategies cannot be limited to the search for superiority, power, etc. All sorts of other possibilities are offered to social agents, and it seems to us that the search for incomparability, the shift over to new activities (into 'vacant places') or to different dimensions of evaluation is a way of compensation, the explanation of which has become essential to social psychology."
>
> Lemaine, 1974, pp.20-22

Assuming the present author's view that both science and society have an identical socio-psychological nature and function let us consider the relevance of

Lemaine's theory to an understanding of innovation in science.

With Berger, it has been contended that:

> "Society not only defines but creates psychological reality. The individual *realizes* himself in society - that is he recognizes his identity in socially defined terms and these definitions become reality as he lives in society."
>
> Berger, 1966, p.107

Having argued that science and society are equivalent in their socio-psychological nature and function it is contended that *science* also functions to create, shape and maintain the individual's psychological reality (via the internalisation of scientifically fashionable theories, etc) and the world in which he *realizes* and *recognizes* himself. Seen in this way Lemaine's analysis of social originality applies *mutatis mutandis* to scientific differentiation[68] and scientific originality.

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68 As manifestation of social differentiation in science we might cite the opposing standpoints of behaviourism and psychoanalysis, comparative psychology and ethology, Coperhagen and Hidden Variable interpretations of quantum mechanics, etc. It is especially interesting to view this differentiation in the light of a number of experiments performed by Lemaine and his colleagues (see Lemaine, 1974, pp34-35). These studies required different groups of children to build huts, the group building the best hut being given a reward. One group was given a handicap - "The handicap lay in the fact that one group, chosen at random and after the rules of the game had been accepted by all the children, did not receive an important element for the execution of the task. Amongst other developments it was observed that the handicapped group showed a wish and determination "to introduce another criterion of judgement than that or those which were implicitly contained in the instructions and to differentiate themselves from others by doing 'something different' from the favoured group. For instance in the experiment with the huts the handicapped group started off protesting that the other group's hut wasn't really a hut; they said it was more like a house since

While perhaps not providing a socio-psychological theory of *all* social, and therefore scientific, change the author contends that Lemaine's views do contribute ideas in terms of which it is possible to understand how socio-psychological factors could be responsible for generating innovations in science. In particular, it is proposed that in terms of such a socio-psychological theory we can suggest a possible avenue of approach to the problem raised in Chapter 3 - viz., in view of incommensurability, how can we account for the existence of revolutions in science?

In his book *The Justification of Scientific Change*, Kordig discussed this problem and argues as follows:

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there were several rooms in it ... After wasting a lot of time on account of bad organization the group decided to make a little garden around the house and engaged in a vigorous discussion with the children in the other group and the judges - the adults - to get them to admit what they wanted to do was legitimate. After the initial divergence, based here on the certainty that the competition, in terms of its initial definition, was lost in advance, it therefore became necessary to argue with the other agents in order to get the new activity and the new criteria 'recognized' as it were in the rough; then, subsequent to this, the idea of *equivalence* became the central issue - in other words, the children in the group which had been set at a disadvantage were willing to admit the superiority of the other group with regard to the hut proper (forgetting the earlier semantic quarrel) so long as the favoured group, in return admitted their superiority on the basis of the new criterion which they had introduced and then obtained recognition for. It is possible to say that the disadvantaged group which had no chance of winning by respecting the rules of the game differentiated itself, shifted its activity and the criteria of evaluation to another field, got them recognized as legitimate and managed after several stages to make themselves incomparable to the other group." (Lemaine, 1974, p.35.) Substituting the constructing of new scientific theories for the building of huts provides illustration of how Lemaine's views allow a socio-psychological conceptualisation of scientific differentiation. In fact the generality of such an approach is extended by Tajfel's (1972) findings that differentiation between groups occurs even in the absence of a shortage of resources. Tajfel maintains that the mere recognition of another as belonging to an out-group is sufficient to bring about social distancing.

> "Feyerabend's, Hanson's, ... and Kuhn's views, I maintain, prevent the scientific theory after a revolution, T_2, from being a *rival* or *alternative* to the scientific theory T_1, before the revolution. On their view T_1 determines experience E_1 which is different than E_2, the experience determined by T_2. But given this it becomes more difficult, if not impossible, for the scientist accepting T_1 to have professional disagreement about the experience E_1 with the scientist accepting T_2. This is because they are not talking about the same experience or world; in the sense relevant to science it is claimed that they don't experience the same things; $E_1 \neq E_2$. And each scientist is talking about what he experiences. Their beliefs about experience and the world are not, therefore, *rival* beliefs. Nor are T_1 and T_2 *alternatives*. What is T_1 an *alternative* to? It is not an alternative to T_2 for on their views T_2 is talking about something different (E_2) than T_1 is (E_1). T_1 and T_2 are not alternative views of the same world. On their views the world has radically changed. Nor are they alternative views of experience, for this is radically different."
>
> Kordig, 1971, p.22

The author agrees with Kordig's basic argument that if Kuhn's, Feyerabend's and Hanson's epistemological theories are correct, then where two theories T_1 and T_2 are incommensurable, T_1 cannot be strictly considered to *rival* T_2. The author also concurs with the view that all observation is theory-laden and hence that experience E_1 (determined by T_1) $\neq$ experience E_2 (determined by T_2).

In maintaining the view that scientific theories can be conceived as complexes of psychosocial norms and that scientific change consists in the evolution of these norms, the author considers it possible to answer the objection raised by Kordig. It is not scientific *theories* that rival and compete with each other. Rather it is *scientists* both as individuals and as members of groups (defined in terms of the theories, methods, etc,

to which they give their allegiance) who rival and compete among themselves. It is the scientists who succeed in their struggle for survival in the scientific community, the groups which gain the most members, possess the greatest intellectual and financial resources, the keenest determination, etc, etc, who determine which theories are most likely to gain acceptance and survive. Thus the author contends that the evolution of scientific knowledge may be largely understood and explained in terms derived from the social psychology of group dynamics.

It is further suggested that Kordig's critique of Feyerabend's principle of counter-induction can be answered in similar fashion. It will be recalled that this principle advocates the invention and elaboration of hypotheses inconsistent with the accepted point of view, even where the latter should happen to be highly confirmed and generally accepted. Kordig writes:

> "This principle is central to Feyerabend's positive methodology ... This principle, under the name of 'theoretical pluralism', is "assumed to be an *essential feature* (Feyerabend's italics) of all knowledge, for, "criticism must use alternatives" Yet it is precisely his radical observation variance position which, as we have seen, precludes two different theories from being inconsistent. Rather than being "an *essential* feature of all knowledge", the principle of proliferation would instead be an *impossible feature* of all knowledge. And on a normative interpretation, it would in effect, call for the impossible given the doctrine of radical observational variance."
>
> Kordig, 1971, p.23

Kordig is correct in pointing out that if two theories, T_1 and T_2, are incommensurable then they cannot be inconsistent. In this respect his argument does undermine any *epistemological* justification Feyerabend has given to support the proliferation of theories as essential to progress in man's search for truth. However, as

noted, Feyerabend's (1965) main argument in support of theoretical pluralism is *not* an epistemological argument. It is rather that given the existence of a Kuhnian-like monolithic science, the belief-system becomes so well entrenched in the thinking (both conscious and sub-conscious[69]) of both scientist and non-scientist that its truth comes to be taken for granted. This in turn leads to a 'closed society' in which the freedom of individuals is restricted[70]. In short, Feyerabend's main argument in support of theoretical pluralism is based on ethical considerations which it is not the purpose of the present essay to question.

While we might agree with Kordig that Feyerabend has given no good *epistemological* reasons to support his principle of counter-induction it is nevertheless contended that Kordig's arguments criticizing Feyerabend's advocacy of this principle are unsound. Even though Feyerabend advocates the proliferation of *incommensurable*

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69 Discussing the same point, Bem and Bem (1970) write: "We have seen what happens when an individual's reference groups conflict. Alternative ideologies are suddenly brought into his awareness, and he is forced to select explicitly his beliefs and attitudes from among the competing alternatives. But what happens when all his reference groups agree, when his religion, his family, his peers, his teachers, and the mass media all disseminate the same message? The consequence is a non-conscious ideology, a set of beliefs and attitudes which he accepts implicitly but which remains outside his awareness because alternative conceptions of the world remain unimagined. As we noted earlier, only a very unparochial and intellectual fish is aware that his environment is wet. After all, what else could it be? Such is the nature of a non-conscious ideology." (Bem and Bem, in Bem, 1970, p.89.)

70 Bem and Bem also make this point. Carrying on from where the passage quoted in note 69 left off, they write: "A society's ability to inculcate this kind of ideology into its citizens is the most subtle and most profound form of social influence. It is also the most difficult kind of social influence to challenge because it remains invisible. Even those who consider themselves sufficiently radical or intellectual to have rejected the basic premises of a particular societal ideology often find their belief systems unexpectedly cluttered with its remnants." (Bem and Bem, in Bem, 1970, p.89.)

theories, just because they are incommensurable it does not follow that they *cannot* contribute to the overthrow and rejection of extant alternatives. In acknowledging that they cannot be used to establish the *falsity* of alternatives, the possibility of their playing vitally important role in the competition for survival amongst scientists is not thereby excluded. As it has already been argued, the scientist's perceptions and behaviour *qua* scientist are largely determined by his theoretical viewpoint. From this it is clear that the scientist's theoretical viewpoint plays a necessary role in determining the behaviour of scientists competing *qua* scientists.

This point is well illustrated in the great debate between Einstein and the Copenhagen group (Heisenberg, Bohr, Pauli, Born, etc.) of physicists regarding the scientific status of the quantum theory. Steadfastly refusing to relinquish the causal principle, Einstein insisted that quantum theory was necessarily incomplete as long as it allowed indeterminism. As Heisenberg points out, this argument reflected a difference in viewpoints so basic that without more or less rejecting his scientific viewpoint *in toto* Einstein could not have reconciled himself to the quantum theory. In short, any attempt to get Einstein to change his mind would have "*amounted to pulling the ground from under his feet*" (Heisenberg).

To sum up: The chapter opened by briefly outlining Copernicus' reasons for rejecting Aristotelian-Ptolemaic astronomy. Using this story as an illustration, it was suggested, on the assumption that scientific theories constitute complexes of psychosocial norms, scientific change could be conceived in terms of the evolution of these norms. Such a theory constitutes a radical departure from extant approaches to the study of scientific change. On the assumption that the author's view is correct, it was further submitted that there already exist socio-psychological theories contributing possible answers to the questions posed by current philosophy of science. In terms of one such socio-psychological theory, an illustration was given of the form these answers might take. Finally it was argued that while certain attacks on the views of Kuhn,

Feyerabend and Hanson are partially justified, such attacks could be successfully countered by arguments premised on the author's views relating to science and scientific change.

CHAPTER 7

SUMMARY AND CONCLUSIONS

I do not know what I may appear to the world but to myself I seem to have been only like a boy playing on the seashore and diverting myself in now and then finding a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me.

Newton

Social psychologists tend to share the widespread opinion that scientific knowledge is *objective* knowledge, i.e., (the opinion) that scientific knowledge is based on impartial observation and is objectively testable. Such ideas, it was maintained, stem largely from the influence of positivistic and Popperian philosophies of science.

Initially an outline of the basic tenets of positivism was presented. Making explicit Popper's disagreements with these tenets, his own views were then described. Although, like the positivists, Popper claimed that there existed a *scientific method* which made possible a body of objective knowledge, it was noted that his attempt to eliminate *subjective* elements from science was not successful.

The alleged objectivity of scientific knowledge has been seriously challenged in view of subsequent developments in the philosophy of science introduced by Kuhn and Feyerabend. These authors at approximately the same time introduced their views regarding incommensurability, these views implying that no knowledge is free from subjective elements. While both suggested the fundamental importance of subjective factors as determinants of scientific change, it was pointed out that neither of these authors supplied a theory of any real generality capable of explaining such change in terms of subjective factors.

The implications of these recent subjectivist philosophies of science for theorizing in psychology were discussed. For purposes of illustration, the discussion centered on the behaviouristic approach to psychology advocated by B.F. Skinner. It was contended that as a consequence of Kuhn's and Feyerabend's arguments, the alleged objectivity of the behaviouristic approach might be exposed as a myth.

Following this digression attention was again turned to the nature and function

of theory in science. An explicitly socio-psychological viewpoint was adopted. Presenting Sherif's views on the nature and function of social norms, it was noted that so conceptualised, these possessed the identical nature and function common to scientific theories. Therefore the proposal was put forward that scientific theories consist of (complexes of) social norms.

In terms of such a socio-psychological understanding it was deemed likely that valuable insights into the social psychology of science and scientists would result were science to be viewed from the standpoint of Berger's sociology of knowledge. Introducing Berger's views, illustrations of how these provide insight into the social psychology of science were presented.

Having submitted his socio-psychological theory of science, the author went on to present his socio-psychological theory of scientific change claiming that scientific change could be conceived in terms of the evolution of psychosocial norms. It follows from this theory that an understanding of the origin and formation of psychosocial norms would generate an understanding of innovation in science. Lemaine's theories on social differentiation and social originality were used to demonstrate this point. In terms of Lemaine's theory it was suggested that a possible socio-psychological antecedent of developments in science lay in the individual's tendency to socially differentiate himself from those he sensed threatened his social identity. In other words, differentiation and originality in science may stem from scientists - either individually, or as a group - struggling to establish and maintain their reputation as scientists.

In conclusion we make the following comments. First, a radically new approach to the understanding of science and scientific change has been proposed. It is contended that in terms of the views advocated some questions at the centre of attention in current philosophy of science can be answered. Second, in terms of the ideas

developed, the relevance of much socio-psychological research to the understanding of developments internal to science is apparent. On the assumption of these views the possibility of applying socio-psychological theories emerging out of research in such fields as inter-group processes, attitude formation and attitude change, persuasive communications, leadership, conformity, etc, etc, to the socio-psychological understanding of developments in science clearly follows. (The author intends to pursue some of these possibilities in a programme of postgraduate research.) Third, the proposed socio-psychological theory of science and scientific change does not entail that the truth or falsity of the scientist's beliefs is determined by their socio-psychological origins. Socio-psychological theories of man and his behaviour are never other than hypothetical. Consequently there is no justification for accepting such theories as an ultimate basis on which to demarcate between truth and error, right and wrong, good and evil, etc. More important, we would maintain with Bohm:

> "... in all human relationships, we have to be free of the constraining and distorting notion that human nature is some well-defined sort of 'thing' that can in principle be known and specified in terms of models of the self. Human nature in its totality and all the essential abstractions from it such as beauty, truth, rationality - are not 'things', but aspects of a whole movement. 'Things' can properly be conceived in terms of models. But the whole movement of human nature cannot be contained in any models. Rather it is capable of continually revealing itself anew in fresh and unexpected ways that are in essence inexhaustible."
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> Bohm, 1973, p 108

BIBLIOGRAPHY

Ammerman, R.R. [1965], *Classics of Analytic Philosphy*, New York: McGraw-Hill.

Anscombe, G.E.M. [1966], *Intentions*, Ithaca: Cornell University Press.

Aronson, E. and Carlsmith, J.M., [1968], "Experimentation in Social Psychology" in Lindzey, G. and Aronson, E. (eds.) *The Handbook of Social Psychology*, 2nd. Edition, 2. Reading, Massachusetts: Addison-Wesley Publishing Company. 2nd Edition, 2, pp.l-79.

Aronson, E. [1972], *The Social Animal*, San Francisco: W.H. Freeman & Co.

Ayer, A.J. [1936], *Language Truth and Logic*, London: Gallancz.

Ayer, A.J. (ed.) [1939], *Logical Positivism*, Glencoe, Illinois: Free Press.

Bandura, A. [1969], *Principles of Behavior Modification*, New York: Holt, Rinehart and Winston

Bandura, A. (ed.) [1971], *Psycho1ogical Modeling: Conflicting Theories*, New York: Aldine. Atherton, Inc.

Bandura, A., and Walters, R., [1963], *Social Learning and Personality Development,* New York: Holt, Rinehart and Winston.

Becker, G., and McClintock, C.G., [1972], "Scientific Theory and Social Psychology", in McClintock. [1972](ed.), pp.5-20.

Bem, D. J., [1970], *Beliefs Attitudes and Human Affairs*, Monterey, California: Brooks/Cole Publishing Company.

Benthal, J. (ed.) [1973], *The Limits of Human Nature*, London: Allen Lane.

Bernstein, J. [1973], *Einstein*, Bungay, Suffolk: Wm. Collins Sons and Co. Ltd.

Berger, P.L. [1963], *Invitation to Sociology*, New York: Doubleday and Company.

Berger, P.L. [1967], *The Sacred Canopy*, New York: Doubleday and Company.

Berger, P.L. [1969], *A Rumour of Angels*, London: Allen Lane.

Berger, P.L., and Luckmann, T., [1966], *The Social Construction of Reality*, New York: Doubleday.

Bohm, D., [1973] "Human Nature as a Product of our Mental Models", in Benthal, J. (ed.) [1973], pp.92-l14.

Born, M. [1971] *The Born-Einstein letters: Correspondence between Albert Einstein and Max and Hedwig Born from 1916 to 1955 with commentaries by Max Born*. Translated by Max Born. New York: Macmillan.

Braithewaite, R.B. [1953], *Scientific Explanation,* New York: Harper and Row.

Broadbent, D.F. [1961], *Behaviour*, London: Butler & Tanner Limited.

Broadbeck, N. (ed.) [1968], *Readings in the Philosophy of the Social Sciences*, London: Macmillan.

Bunge, M. (ed.) [1968], *The Critical Approach to Science and Philosophy*, London: The Free Press.

Campbell, D.T., [1969], "Prospective: Artifact and Control" in Rosenthal, R. and Rosnow, R.L., (eds.) [1969], pp.35l -382.

Campbell, D.T. [1974], "Evolutionary Epistemology", in Schilpp, P.A. (ed.) [1974] pp.4l3-463.

Carnap, R. [1930/1931], "The Old and the New Logic", originally in *Erkenntnis*, I, 1930/1931; translated and reprinted in Ayer, A.J. (ed.) [1959], pp.l33-146.

Carnap, R. [1932], "The Elimination of Metaphysics through the Logical Analysis of Language", *Erkenntnis*, 2; Reprinted in Ayer, A.J. (ed.) [1959], pp.60-8l.

Carnap, R. [[1932/1933], "Psychology in Physical Language", *Erkenntnis*, 3, 1932/1933; translated and reprinted in Ayer, A.J. (ed.) [1959], pp.l65-l98.

Carnap, R. [1936/1937] "Testability and Meaning", *Philosophy of Science*, Vols. III and IV. Reprinted in Ammerman, R. (ed.) [1965].

Carnap, R. [1959], *The Logical Foundations of Probability,* Chicago: University of Chicago Press

Carnap, R. [1956], "The Methodological Character of Theoretical Concepts", in Feigl, H. and Scriven, M., (eds.), 1956, pp.38-76.

Chomsky, N. [1958], "A Review of B.F. Skinner's *Verbal Behavior*", reprinted in Fodor, J.A. and Katz, J.J., (eds.) [1964]. *The Structure of Language,* Englewood Cliffs, New Jersey: Prentice-Hall, Inc., pp.547-578.

Cole, S. [1970], "Professional Standing and the Reception of Scientific Discoveries", *American Journal of Sociology*, 76, pp.286-306.

Colodny, R., (ed.) [1962], *Frontiers of Science and Philosophy*, Pittsburgh: University of Pittsburgh Press.

Colodny, H. (ed.) [1965], *Beyond the Edge of Certainty*, Englewood Cliffs, New Jersey: Prentice-Hall, Inc.

Colodny, R., (ed.) [1970], *The Nature and Function of Scientific Theories*, Pittsburgh: University of Pittsburgh Press.

Copleston, F. [1946] *A History of Philosophy*, Vol.1, London: Burns and Oates Ltd.

Crano, W.D. and Brewer, M.B. [1973], *Principles of Research in Social Psychology* New York: McGraw-Hill Book Company.

Davies, J.T. [1965], *The Scientific Approach*, London: Academic Press.

Deese, J. [1972], *Psychology as Science and Art*, New York: Harcourt Brace Jovanovich, Inc.

Deutsch, M. and Krauss, R.M. [1965] *Theories in Social Psychology*, New York: Basic Books, Inc.

Dixon, K. [1973], *Sociological Theory Pretence and Possibility*, London: Routledge and Kegan Paul Ltd.

Einstein, A. [1935], *The World As I See It*, English translation of *Mein Weltbild*, [1934], by A. Harris.

Eiser, J.R. and Stroebe W., [1972], *Categorization and Social Judgement*, London: Academic Press.

Feigl, H and Brodbeck, M., (ed.) [1953] *Readings in the Philosophy of Science*, New York: Appleton-Century-Crofts.

Feigl, H. and Scriven, M., (eds.) [1956] *The Foundations of Science and the Concepts of Psychology and Psychoanalysis*, Minneapolis: University of Minnesota Press.

Feigl, H., Scriven, N. and Maxwell, G., (eds.) [1958], *Concepts, Theories and the Mind-Body Problem* Minneapolis: University of Minnesota Press.

Festinger, L.A [1954]., "A Theory of Social Comparison Processes", *Human Relations*, Volume 7, pp.ll7-l40.

Feyerabend, P.K. [1962], "Explanation, Reduction, and Empiricism", in Feigl, H. and Maxwell, G., (eds.) Minnesota *Studies in the Philosophy of Science*, III, Minneapolis: University of Minnesota Press, pp.28-97.

Feyerabend, P.K. [1964], "Realism and Instrumentalism", in Bunge, M. (ed.) [1964].

Feyerabend, P.K., [1965], "Problems of Empiricism, Part I", in Colodny, R. (ed.) [1965], pp.l45-260.

Feyerabend, P.K. [2967], "On the Improvement of the Sciences and the Arts", in Cohen, R.S. and Wartofsky, M., (eds.) [1967], *Boston Studies in the Philosophy of Science*, III, Dordrecht: Reidel, pp.387-415.

Feyerabend, P.K. [1970a], "Problems of Empiricism, Part II", in Colodny, R. (ed.) [1970], pp.275-354.

Feyerabend, P.K. [1970b], "Against Method", in Radner, H. and Winokur, S., (eds.) [1970], pp.l7-l30.

Feyerabend, P.K. [1970c], "Consolations for the Specialist", in Lakatos, I. and Musgrave, A., (eds.) [1970], pp.l97-230.

Feyerabend, P.K.[1970d], "In. Defense of Classical Physics", *Studies in History and Philosophy of Science*, Volume 1, pp.59-85.

Feyerabend, P.K., *Against Method*, 1975, (in press).

Frankl, V.E. [1969], "Reductionism and Nihilism", in Koestler, A. and Smythies, J.R., (eds.) [1969], pp.396-4l6.

Freedman, J.L., Carlsmith, J.M. and Sears, D.O. [1970], *Social Psychology*, Englewood Cliffs, New Jersey: PrenticeHall, Inc.

Fromm, E. [1962], *Beyond the Chains of Illusion*, New York: Simon and Schuster.

Galileo, G. [1953], *Dialogue Concerning the Two Chief World Systems*, (translated by Stillman Drake); California: University of California Press.

Giorgi, A. [1970], *Psychology as a Human Science*, New York: Harper and Row.

Grosser, M. [1962], *The Discovery of Neptune*, Harvard University Press.

Guthrie, E.R. [1959], "The Status of Systematic Psychology", *American Psychologist*, 5, pp.97-l0l.

Halmos, P. (ed.) [1972], “Sociological Revue Monograph No.18”, *The Sociology of Science,* University of Keel.

Hampshire, S. [1965], *Thought and Action*, London: Chatto and Windus.

Hanson, N.R. [1958], *Patterns of Discovery*, London: Cambridge University Press.

Hanson, N.R. [1969], *Perception and Discovery*, San Francisco: Freeman, Cooper and Company

Hanson, N.R. [1972], *Observation and Explanation*, London: George Allen and Unwin Ltd.

Harre, H. and Secord, P.F. [1972], *The Explanation of Social Behaviour*, Oxford: Basil Blackwell.

Hayek, F.A. [1952], *The Sensory Order,* London: Routledge and Kegan Paul.

Hayek, F.A., "The Primacy of the Abstract", in Koestler, A. and Smythies, J.R., (eds.) [1969] pp.309-333.

Heisenberg, W. [1971], *Physics and Beyond, London*: Harper and Row, Publishers Inc.

Hempel, C.G. [1959, "The Empiricist Criterion of Meaning" - originally appearing in the *Revue Internationale de Philosphie*, 4, 1950; reprinted in Ayer, A.J. (ed.) [1959], pp.l08-132.

Hempel, C.G. [1958], "The Theoretician's Dilemma", in Feigl, H., Scriven, N. and Maxwell, G., (eds.) [1958], pp.37-96.

Hempel, C.G. [1962], "Explanation in Science and History", in Colodny, R. (ed.) [1962], pp.7-34.

Hendrick, C. and Jones, R.A. [1972], *The Nature of Theory and Research in Social Psychology,* New York: Academic Press.

Insko, C.A. and Schopler, J., [1972], *Experimental Social Psychology*, New York: Academic Press.

Hollander, E.P. [1967], *Principles and Methods of Social Psychology* , New York: Oxford University Press.

Israel, J. and Tajfel, H., (eds.) [1972], *The Context of Social Psychology*, New York: Academic Press.

Jahoda, G. [1970], *The Psychology of Superstition,* Harmondsworth, Middlesex: Pelican Books.

Kaplan, A. [1964], *The Conduct of Inquiry,* San Francisco: Chandler Publishing Company.

Katz, D. [1972], "Some Final Considerations about Experimentation in Social Psychology", in McClintock, C.G. (ed.) [1972], pp.549-562.

Kelly, G.A. [1963], *A Theory of Personality* New York: Norton.

Kelvin, P. [1970], *Bases of Social Behaviour*, London: Holt, Rinehart and Winston.

Kemeny, J.G. [1969, *A Philosopher Looks at Science*, Princeton, New Jersey: Van Nostrand.

Kiesler, C.A., Collins, B.E., and Miller, N., [1969], *Attitude Change*, New York: John Wiley and Sons Ltd.

Klineberg, 0. and Christie, R., (eds.) [1965], *Perspectives in Social Psychology,* New York: Holt, Rinehart and Winston.

Koestler, A. [1959], *The Sleepwalkers*, London: Hutchinson.

Koestler, A. and Smythies, J.R., (eds.) [1969] *Beyond Reductionism London*, Hutchinson and Company.

Kohler, W. [1969], *The Task of Gestalt Psychology*, New Jersey: Princeton University Press.

Kordig, C.R. [1971], *The Justification of Scientific Change*, Dordrecht, Holland: D. Reidel Publishing Company

Koyre, A. [1957], *From the Closed World to the Infinite Universe,* Baltimore: The Johns Hopkins Press.

Krasner, L. and Ullmann, L.P., [1973], *Behavior Influence and Personality*, New York: Holt, Rinehart and Winston.

Kuhn, T.S. [1957], *The Copernican Revolution*, New York: Random House.

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

Kuhn, T.S. [1970], "Logic of Discovery or Psychology of Research", in Lakatos, I. and Musgrave, A., (eds.) [1970], pp.1-23.

Lakatos, I. [1970], "Methodology of Scientific Research Programmes", in Lakatos, I., and Musgrave, A., (eds.) [1970], pp.9l-196.

Lakatos, I. [1974], "Popper on Demarcation and Induction", in Schilpp, P.A. (ed.) [1974], pp.24l-273.

Lakatos, I. and Musgrave, A., (eds.) [1970], *Criticism and the Growth of Knowledge*, London: Cambridge University Press.

Lambert, W.W. and Lambert, W.E., [1964], *Social Psychology*, Englewood Cliffs, New Jersey: Prentice-Hall, Inc.

Lemaine, G. [1974], "Social Differentiation and Social Originality", *European Journal of Social Psychology*, 4, pp.l7-52.

Lindgren, H.C. [1969], *An Introduction to Social Psychology*, New York: John Wiley and Sons, Inc.

Lindzey, G. and Aronson, E., (eds.) [1964], *The Handbook of Social Psychology*, Reading, Massachusetts: Addison-Wesley Publishing Company, 2nd Edition.

Lorenz, K. [1965], *Evolution and the Modification of Behaviour*, London: Methuen and Company Ltd.

Lorenz, K. [1966], *On Aggression*, London: Methuen and Company Ltd.

MacCorquodale, K. [1970], "On Chomsky's Review of Verbal Behavior", *Journal of the Experimental Analysis of Behavior*, 13, pp.83-89.

MacCorquodale, K. and Meehl, P.E., [1953], "Hypothetical Constructs and Intervening Variables", reprinted in Feigl, H. and Brodbeck, N. (eds.) *Readings in the Philosophy of Science*, New York: Appleton-Century-Crofts, pp.596-6l1.

Mannheim, K. [1952], *Essays on the Sociology of Knowledge*, London: Routledge and Kegan Paul Ltd.

Marx, N.H. (ed.) [1963], *Theories in Contemporary Psychology*, New York: Macmillan.

Marx, N.H. and Hillix, W.A., [1963], *Systems and Theories in Psychology*, New York: McGraw-Hill Book Company, Inc.

McClintock, C.G. (ed.) [1972], *Experimental Social Psychology*, New York: Holt Rinehart and Winston.

McGinnies, E. and Ferster, C.B., (eds.) [1971], *The Reinforcement of Social Behavior*, New York: Houghton Mifflin Company.

Medawar, P.B. [1969], *Induction and Intuition in Scientific Thought*, London: Methuen and Co. Ltd.

Melden, A.I. [1961], *Free Action*, London: Routledge and Kegan Paul.

Medawar, P.B. [1967], *The Art of the Soluble*, London: Methuen and Co. Ltd.

Merton, R.K., *Social Theory and Social Structure,* New York: Free Press, 3rd Edition.

Mischel, T. (ed.) [1969], *Human Action*, New York: Academic Press.

Mischel, W. [1968], *Personality and Assessment*, New York: John Wiley and Sons Inc.

Moscovici, S. [1972], "Society and Theory in Social Psychology", in Israel, J. and Tajfel, H. (eds.) [1972], pp.l7-68.

Nagel, E. [1961], *The Structure of Science*, London: Routledge and Kegan Paul.

Neurath, 0. [1932/1933], "Protocol Sentences", originally in *Erkenntnis*, III, 1932/1933; translated and reprinted in Ayer, A.J. (ed.) [1959], pp.l99-208.

Pannenberg, W. [1971], *Basic Questions in Theology*, Volume II, London: S.C.M. Press.

Peters, R.S. [1958], *The Concept of Motivation*, London: Routledge and Kegan Paul

Phillips, D.L. [1973], *Abandoning Method,* San Francisco: Jossey-Bass Publishers.

Popper, K.R. [1934], *Logic der Forschung,* Vienna: Verlag von Julius Springer.

Popper, K.R. [1959], *The Logic of Scientific Discovery*, London: Hutchinson.

Popper, K.R. [1963], *Conjectures and Refutations*, London: Routledge and Kegan Paul.

Popper, K.R. [1972], *Objective Knowledge: An Evolutionary Approach,* London: Oxford University Press.

Popper, K.R. [1974a], "Falsifiability and Freedom" - a discussion with Sir John Eccles printed in Elders, F. (ed.) [1974], *Reflexive Water The Basic Concerns of Mankind* London: Sourvenir Press.

Popper, K.R. [1974b], "Intellectual Autobiography", in Schilpp, P.A. (ed.) [1974], pp.3-184.

Premack, D. [1970]., "A Functional Analysis of Language", *Journal of the Experimental Analysis of Behavior*, 14, pp.107-125.

Radner, N. and Winokur, S. [1970], *Analyses of Theories and Methods in Physics and Psychology*, Minneapolis: University of Minnesota Press.

Rescher, N. [1970], *Scientific Explanation*, New York: The Free Press.

Rosen, E. [1959], *Three Copernican Treatises*, New York: Dover Publications, Inc.

Rosenblatt, P.C. and Miller, N.,[1972], "Experimental Methods", in McClintock (ed.) [1972], pp2l-48.

Rosenblatt, P.C. and Miller, N., [1972], "Problems and Anxieties in Research Design and Analysis", in McClintock (ed.), [1972], pp.49-74.

Rosenthal, R. [1969], "Interpersonal Expectations: Effects of the Experimenter's Hypothesis", in Rosenthal, R. and Rosnow, R.L., (eds.) [1969], pp.l82-279.

Rosenthal, R. and Rosnow, R.L. (eds.) [1969], *Artifact in Behavioral Research*, New York: Academic Press.

Russell, B. [1924], "Logical Atomism", in R.C. Marsh (ed.) [1956], *Logic and Knowledge,* London: George Allen and Unwin. pp.321-344.

Ryle, G. [1954], *Dilemmas,* London: Cambridge University Press.

Sargent, W. [1973], *The Mind Possessed*, London: Heinemann.

Schick, K. [1971], "Operants", *Journal of the Experimental Analysis of Behavior*, 15, pp.4l3-423.

Schilpp, P.A., (ed.) [1974], *The Philosophy of Sir Karl Popper, Volumes I, and II*. La Salle, Illinois: The Open Court Publishing Company.

Schlick, M. [1930/1931], "The Turning Point in Philosophy", translated from the original which appeared in Volume I of *Erkenntnis*, [1930/1931], reprinted in Ayer, A.J. (ed.) [1959], pp.53-59.

Schlick, M. [1932/1933], "Positivism and Realism", a translation of the original which appeared in *Erkenntnis*, Volume 3, 1932/1933, and is reprinted in Ayer, A.J. (ed.), 1959, pp.82-107.

Schlick, M. [1934],"The Foundation of Knowledge", originally in *Erkenntnis* Volume 4, 1934; translated and reprinted in Ayer, A.J. (ed.) [1959], pp.209-227.

Schutz, A. and Luckmann, T., [1973], *The Structures of the Life-World*, Evanston: Northwestern University Press.

Schwayder, D.S. [1965], *The Stratification of Behaviour*, London: Routledge and Kegan Paul.

Scriven, M. [1956], "A Study of Radical Behaviorism" in Feigl, H. and Scriven, M., (eds.), [1956], pp.88-l30.

Shaw, N.E., and Costanzo, P.R., [1970], Theories of Social Psychology New York: McGraw-Hill Book Company.

Sheridan, C.L. [1971], *Fundamentals of Experimental Psychology*, New York: Holt Rinehart and Winston.

Sherif, M. [1935], "A Study of Some Social Factors in Perception", *Archives of Psychology*, 27.

Sherif, M. [1936], *The Psychology of Social Norms*, New York: Harper and Row.

Sherif, N. and Sherif, C.W. [1969], *Social Psychology*, London: Harper and Row, and Tokyo: John Weatherill.

Skinner, B.F. [1938], *The Behavior of Organisms*, New York: Appleton-Century-Crofts.

Skinner, B.F. [1953a], *Science and Human Behavior*, New York: The Macmillan Company.

Skinner, B.F. [1953b], "The operational analysis of psychological terms", reprinted in Feigl, H. and Brodbeck, M. (eds.), [1953], pp.585-595.

Skinner, B.F. [1956], "Critique of Psychoanalytic Concepts and Theories", in Feigl H. and Scriven, M., (eds.), [1956], pp.77-87.

Skinner, B.F [1957], *Verbal Behavior*, New York: Appleton-Century-Crofts.

Skinner, B.F. [1969], *Contingencies of Reinforcement*, New York: Appleton-Century-Crofts.

Skinner, B.F.[1972], *Beyond Freedom and Dignity,* London: Jonathan Cape Ltd.

Smart, J.J.C.[1968] , *Between Science and Philosophy*, New York: Random House.

Storer, N. [1966], *The Social System of Science*, New York: Holt, Rinehart and Winston.

Stotland, E. and Canon, L.K., [1972], *Social Psychology: A Cognitive Approach*, London: W.B. Saunders Company.

Stratter, S [1974], *Phenomenology and the Human Sciences*, Pittsburgh: Duquesne University Press.

Tajfel, H., [1964], "Social and Cultural Factors in Perception", in Lindzey, G. and Aronson, E., (eds.) *The Handbook of Social Psychology*, Reading, Massachusetts: Addison Wesley Publishing Company [1964], 2nd Edition, 3, pp.315-394.

Tajfel, H. [1972a], "Social Categorization", in Moscovici, S. (ed.) [1972], *Introduction a la Psychologie Sociale* Paris: Lorousse, pp.272-302.

Tajfel, H. [1972b], "Experiments in a Vacuum", in Israel, J. and Tajfel, H. (eds.) [1972], pp.69-l22.

Taylor, C. [1964], *The Explanation of Behaviour*, London: Routledge and Kegan Paul.

Taylor, R. [1966], *Action and Purpose,* New Jersey: Prentice-Hall.

Thibaut, J.W. and Kelley, H.H., [1959], *The Social Psychology of Groups* New York: John Wiley and Sons, Inc.

Turner, M.B. [1965], *Philosophy and the Science of Behavior,* New York: Appleton-Century-Crofts.

Turner, M.B. [1971], *Realism and the Explanation of Behavior*, New York: Appleton-Century-Crofts.

Waismann, F. [1930], "Logische Analyse des Wahrscheinlichkeitsbegriffs", Erkenntnis, 1, 1930.

Walker, E.L. [1970], *Psychology as a Natural and Social Science*, Belmont, California: Brooks/Cole Publishing Company.

Wartofsky, M.W. [1968], *Conceptual Foundations of Scientific Thought*, New York: Macmillan.

Whitley, RD. [1972], "Black Boxism and the Sociology of Science", in Halmos, P. (ed.), pp.61-92.

Will, C.M. [1974], "Gravitation Theory", *Scientific American,* 231, pp.25-33.

Wittgenstein, L. [1922], *Tractatus Logico-Philosophicus*, London: Kegan Paul.

Wolman, B.B. [1960], *Contemporary Theories and Systems in Psychology*, New York: Harper Brothers.

Wrightsman, L.S. [1972], *Social Psychology in the Seventies*, Monterey, California: Brooks/Cole Publishing Company.