# EPISTEMOLOGICAL NEGATIVISM AND SCIENTIFIC KNOWLEDGE

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By

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## Preface

Toward the end of the nineteenth century, Ernst Mach expressed his worries about obscurities and metaphysical elements in scientific knowledge, and consequently contributed to the development of a Viennese tradition. Later on, Vienna Circle further extended Mach's ideas and led to the development of the logical positivist movement. Among the main tenets of this movement is the view that scientific theories are to be reduced to an empirical base capable of conclusive verification. But scientific theories are usually based on unverified and occasionally unverifiable hypotheses and principles. Thus, once this is realized, positivism in spite of itself will contribute to the development of scepticism about scientific knowledge.

In reaction to the verificationism, however, Karl Popper developed his thesis of falsificationism or fallibilism; but this principle also leads to scepticism at least about certainty claims, with far reaching consequences. This chain of events leads to the development of Paul Feyerabend's epistemological anarchism: a rejection of all rules and methods in science in an attempt to reduce science to the level of irrationality and mythology.

Against the negativist conclusions of these

positions about science, the integrity of scientific knowledge is defended in this thesis. It is shown how scientific knowledge can be defended against scepticism of the type to which verificationism tends; this is done by examining one recent and rigorous sceptical position which undermines not only certainty and rationality of knowledge claims, but the very possibility of knowledge. By examining Popper's fallibilism, it is shown that science can also be defended against the negativist conclusions of fallibilism. Similarly, it is shown that Feyerabend's epistemological anarchism cannot either undermine scientific knowledge.

These negativist positions, though they have received strong criticisms in some quarters, yet have not been examined all together, from the standpoint of their impact on the integrity of scientific knowledge. This task is undertaken in this thesis; we thus arrive at a positive and correct evaluation of scientific knowledge in the context of contemporary negativist epistemological trends. It has been shown that in spite of all the negativist arguments of the above positions, we can obtain certainty, justification, and truth in science, and thus we can obtain knowledge. But my rejection of negativism in science does not entail, and should not be construed as an advocacy of a return to, positivism.

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Lastly, I would like to say that I myself am solely responsible for any mistakes that might remain in this thesis - either philosophical or typographical.

CONTENTS

# Introduction

The areas with which this thesis will be concerned are: scepticism, fallibilism and epistemological anarchism. Many issues can be raised in these areas. The problem which I shall present here, the problem of epistemological negativism in the context of science, has been in the recent past an important issue. By 'epistemological negativism in the context of science', I mean the tendency of the recent developments in the philosophy of science to undermine claims to scientific knowledge. Sometimes such undermining is related to the concern whether or not scientific knowledge is genuine knowledge, sometimes to the concern about the methodology of science. An attempt will be made in this thesis to show that the attempt of epistemological negativism to undermine scientific knowledge fails, and that we can obtain certainty and truth about empirical scientific statements, and consequently can obtain knowledge. Before proceeding further, I shall indicate the temporal limits within which I shall investigate the problem.

It is possible to trace critical attacks on scientific knowledge as far back as the history of science goes. But I shall limit the scope of my work to the later

nineteenth century onwards when a newly critical approach regarding scientific knowledge began to develop. In the following sections, I shall explain how the critical approach started, and how it led to the development of a chain of schools of thought - each representing a type of negativism. I shall not enter any discussion in these sections except as necessary to show the chain of their successive development and their negative character. And lastly, I shall state the plan by which I shall deal with these negative positions.

## §0.1: Philosophical Worries about Scientific Knowledge.

Toward the end of the nineteenth century, Ernst Mach expressed his concern about obscurities in scientific knowledge, and rejected such obscurities as metaphysical. Mach advocated a reductionist view, that all scientific knowledge must be reducible to empirical facts. Indeed, his Popular Scientific Lectures begins with a reference to the clarity of scientific thought. With a reference to Socrates' method of criticism, Mach says that, 'The history of science is full of examples of this constant change, development, and clarification of ideas'.[1] In fact, his writings reveal an utmost emphasis on the clarity and organization of thought, what he calls its 'economy'.

About this notion he says: '[the] tendency of obtaining a survey of a given province with the least expenditure of thought, and of representing all its facts by some one single mental process, may be justly termed an economical one'.[2] This goal cannot be attained unless our thoughts about facts are clear and well-organized. Such clarity of thought could, however, be best found in mathematics: 'The greatest perfection of mental economy is attained in that science which has reached the highest formal development,... in mathematics'.[3] But how can this perfection be achieved in the empirical scientific knowledge? An attempt to find Mach's answer to this question would lead us to his views about the nature of knowledge and the place of facts in scientific investigation. We will find here a reductionist view along with his view of the clarity and economy of knowledge.

According to Mach, knowledge must be reducible to empirical facts. This is clear when he says that, 'no knowledge worthy of the name can be gathered ... except by the most exquisite economy of thought and by the careful amassment of the economically ordered experience of thousands of co-workers'.[4] Emphasis on experience is presented more strongly at another place: 'In experience, therefore, is buried the ultimate well-spring of all knowledge of nature ...'.[5] We may further observe this

reductionist view of empirical scientific knowledge when he says: 'That science is inconceivable the principles of which would enable a person with no experience to construct the world of experience, without a knowledge of it'.[6] Or that, 'We know of only one source of immediate revelation of scientific facts - our senses'.[7] In his later work The Analysis of Sensations, Mach says: 'The adaptation of thoughts to facts, accordingly, is the aim of all scientific research. ... Our thoughts marshal the elements before us in groups copying the order of the sense-given facts'.[8] All these views clearly indicate the reductionist view about scientific knowledge.[9]

Mach's views about clarity and reductionism are further strengthened by his attempt to omit mysteries and metaphysical ideas from empirical sciences. The relation of science and mystery is expressed as follows: 'Yet she [i.e. science] who came to bring light into the world, can well dispense with the darkness of mystery, and with pompous show, which she needs neither for the justification of her aims nor for the adornment of her plain achievements'.[10]

Mach's concern about the nature and validity of scientific knowledge is solely epistemological in character. His position in this respect can be summarized as follows: scientific knowledge must be based on facts and experience,

and must be clear, organized and free from metaphysical elements.

Mach, who propounded his views in Vienna, became in the course of time the predecessor of, and the source of inspiration for, the similar views of the Vienna Circle. The members of the Vienna Circle, as A.J.Ayer notes, 'were developing a Viennese tradition which had flowered at the end of the nineteenth century in the work of such men as the physicists Ernst Mach ...'.[11] In Ayer's words: 'If we exclude contemporaries from the list, those who stand closest to the Vienna Circle in their general outlook are Hume and Mach'.[12] In fact, the verification criterion, which presents the central theme of the Vienna Circle and the logical positivist movement, is a crystalization of the above mentioned concerns of Mach's about scientific knowledge. I shall discuss this criterion in the following section and indicate how it leads to negativism.

## §0.2: Development of the New Trend: Verificationism.

Perhaps the most controversial contribution of the logical positivist movement is its meaning criterion: the verification principle. This principle gives the utmost importance to empirical observation and verification as the

criterion to determine the meaning of a scientific statement. One of the earliest statements of this principle is found in Moritz Schlick's "Positivism and Realism". Schlick writes:

> ... it is simply impossible to give the meaning of any statement except by describing the fact which must exist if the statement is to be true. If it does not exist then the statement is false. The meaning of a proposition consists, obviously, in this alone, that it expresses a definite state of affairs. And this state of affairs must be pointed out in order to give the meaning of the proposition.[13]

Schlick further says that, 'The statement of the conditions under which a proposition is true is the same as the statement of its meaning, and not something different'.[14] Schlick considers the claim that the 'meaning of a proposition is identical with its verification' as a 'principle',[15] and holds that the meaning of a proposition is exhaustively given by its verification. He says that it must be denied that 'a proposition can contain more than can be verified'.[16]

One main reason for putting forward the verification principle is to draw a sharp distinction between science and metaphysics, by claiming that the former is verifiable and the latter is not. Verificationists reject metaphysics as consisting of unverifiable, and therefore, meaningless pseudo-problems. Carnap presents this view in

the following way. He takes 'a' for any word and 'S(a)' for any elementary statement, and then he gives 'the sufficient and necessary condition for "a" being meaningful' as: 'The method of verification of "S(a)" is known'.[17] Since we do not know how to verify metaphysical statements empirically, they are meaningless, and are supposed to be separated from the empirical scientific statements.

Common to all early positivists was this requirement that the statements of empirical sciences be susceptible to verification. Thus 'Science is a system of statements ... controlled by experimental verification',[18] although the use of the weak (and vague) connective 'controlled' already indicates a retreat from the earliest ambitions of the movement. But the positivist insistence upon verification entails, as soon came to be seen, that a large area of scientific knowledge has to be rejected as meaningless and metaphysical. This is because there are many scientific statements which cannot be verified.

If verification is a necessary condition for the admission of a statement as (scientific) knowledge, then very little will still count as (scientific) knowledge. Thus resolutely following the early positivist programme results in a fairly widespread (though not, we would argue, complete) scepticism. In view of this limitation of the verification criterion, Carnap says that "Scientific

statements are not, in the strict sense, 'verified' by this process".[19] Similarly Neurath holds, as to the verifiability of protocol statements, that '<u>There is no way of taking conclusively established true protocol sentences as the starting point of the sciences</u>'.[20] This, of course, was not the intention of the positivists, and, faced with this dilemma, their usual response was to weaken their verificationist requirements. First, the demand for actual verification was replaced by weaker demands for verifiability. Schlick draws a distinction between actual verification and conceivable verification.[21] Similarly, Ayer makes a distinction between the strong and the weak senses of verification.[22] But eventually even these requirements were replaced by even weaker requirements of confirmation. Thus Carnap says, 'If by verification is meant a definitive and final establishment of truth, then no (synthetic) sentence is ever verifiable... . We can only confirm a sentence more and more. Therefore we shall speak of the problem of <u>confirmation</u> rather than of the problem of verification'.[23]

It was always the positivists' intention to preserve the fabric of scientific knowledge more or less intact, placing it, in fact, on a firmer foundation. In the trade-off between the preservation of science and the satisfaction of verificationist requirements certain epistemic

claims made on behalf of science were abandoned. For example, the claim that scientific statements could be known with certainty was generally abandoned, because only the strictest form of verification was thought to give certainty. But typically the trade-off resulted in a weakening of the verificationist requirements. With the strong verificationist requirements were lost the early positivist hopes for a conclusive refutation of scepticism.[24] It is the collapse of the positivists' fundamental programme in epistemology that led to the rise of the negativist positions discussed in this thesis, all of which share not just the view that verification provides an inadequate base for foundationalism, but that the foundationalist programme itself is in error.

## §0.3: Verificationism and Scepticism.

A consistent pursuance of the verification principle indeed leads to scepticism. This can be explained in the following way. We may note that on the basis of verification, we can never be certain about an empirical statement. We have seen above that if verification principle is accepted then a large part of scientific knowledge will turn out to be meaningless. If so, we cannot obtain certainty about a large part of scientific knowledge. Consequently,

this will lead us to scepticism. Such a sceptical remark, for example, is that, 'theoretically every objective fact is capable of some (partial) verification at any later date, and that no totality of such experience is absolutely and completely sufficient to put our knowledge of such particulars beyond all possibility of turning out to be in error'.[25]

This view of C.I.Lewis' belongs to the time when verificationism was in its early days of development. In later writings Lewis says of the possibility of further verification: 'The possibility that such further tests, if made, might have a negative result, cannot be altogether precluded; and this possibility marks the judgment as, at the time in question, not fully verified and less than absolutely certain'.[26] And thus we see that we are left in a sceptical situation in regard to certainty of a statement. In discussing Lewis' views, Norman Malcolm observes that, 'The Verification Argument is thought to prove, not simply that many assertions of this [empirical] sort are mistaken or unjustified, but that all such assertions are, in all cases, mistaken or unjustified. In short, it is thought to prove that it is not even possible that anyone should, in any circumstances, make such an assertion without the assertion being false or unjustified or improper or mistaken or incorrect'.[27]

In the second chapter of this thesis, we will again come across Malcolm's views and see that such a scepticism is unacceptable. But it is now clear that verificationism leads to scepticism, and this is a negative consequence. In the following section we will see a significant development which takes place as a response to verificationism.

## §0.4: The Popperian Turn and Fallibilism.

Perhaps the most forceful critique of the verification principle is given by Karl Popper, who not only has criticized the criterion but also has presented an alternative to it. But his alternative, which is his falsifiability principle and which leads to his fallibilism, embraces rather than resists the negativist consequences. As to verificationism Popper, like others, shows that the verification criterion leaves scientific theories as unverifiable and consequently treats them as metaphysical.[28]

Popper observes: 'positivists, in their anxiety to annihilate metaphysics, annihilate natural science along with it'.[29] On the other hand, Popper notes, "The positivist dislikes the idea that there should be meaningful problems outside the field of 'positive' empirical science".[30] This view is also unsatisfactory, and Popper calls it 'the positivist dogma of meaning'.[31] If meaning is considered

in this way, then "nothing is easier than to unmask a problem as 'meaningless' or 'pseudo'. All you have to do is to fix upon a conveniently narrow meaning for 'meaning', and you will soon be bound to say of any inconvenient question that you are unable to detect any meaning in it".[32] Moreover, what is paradoxical for the positivists is that if they "admit as meaningful none except problems in natural science, any debate about the concept of 'meaning' will also turn out to be meaningless".[33] Hence, the very positivist notion of meaning will render it difficult to settle the meaning of 'meaning' - leading positivism to a paradoxical situation.

On the other hand, <u>if we accept</u> the logical positivist notion of meaning even then their rejection of metaphysics will be unsatisfactory. If by 'meaningless' we mean " 'not belonging to empirical science', then the characterization of metaphysics as meaningless ... would be trivial; for metaphysics has usually been defined as non-empirical".[34]

Because of such difficulties in the verification criterion, Popper presents his falsifiability principle. Any empirical statement which is worthy of considerations must be falsifiable at least in principle. This principle implies that any empirical statement is possibly false, and thus is fallible.

Popper's fallibilism, however, is not free from negativism. In establishing his falsificationism or fallibilism, he comes to the negative conclusion that 'Science is not a system of certain, or well-established, statements', and that 'Our science is not knowledge'.[35] In the later phase of his fallibilism, i.e. in his Conjectures and Refutations, Popper admits that we can have knowledge. But even there he does not admit that we can obtain certainty about the empirical scientific statements. Popper's fallibilism thus leads to scepticism at least about certainty. We may further observe that Popper holds that: "I think that we shall have to get accustomed to the idea that we must not look upon science as a 'body of knowledge', but rather as a system of hypotheses; that is to say, as a system of guesses or anticipations which in principle cannot be justified...".[36] Popper's fallibilism, therefore, leads to scepticism also about justification. Moreover, Popper's fallibilism has some of the structural features of verificationism, for example, a fairly restricted method which does not fit all of science. It also seeks to square method with science by scaling down the claims of the latter.

However, the worst negative consequence of Popper's fallibilism is that in it are inherent the roots of a stronger epistemological negativism, viz., epistemological

anarchism. I shall explain this in the following section.

§0.5: The Development of Epistemological Anarchism.

The negativism of Popperian fallibilism has led Paul Feyerabend to react against it. Feyerabend emphasizes Popper's conclusion that science is not knowledge. A 'strict principle of falsification', Feyerabend holds, 'would wipe out science as we know it and would never have permitted it to start'.[37] One who consistently pursues Popper's falsificationism may indeed find it difficult to see how he can consider a view justifiable and certain.

Feyerabend is also against Popper's view[38] that scientific research starts with the formulation of a problem. As he notes,[39] this may not always be the case. One may indeed start an investigation in an area without formulating any problem, the problems may later develop in the course of research and inquiry. Thus if the view which Popper suggests would be the standard of the past scientific procedure then many scientific contributions possibly could not have been made.

Next, one of the aspects of Popper's falsificationism, as we will see in the course of our discussion, is that he criticizes the use of ad hoc hypotheses in science. But if the adoption of ad hoc hypotheses is

never permitted in science, then many scientific theories could never have been established and scientific advance would be seriously disrupted. This is another ground for which Feyerabend considers Popper's position unsatisfactory.

Feyerabend also rejects logical positivism, obviously on account of its overly strong meaning criterion. About logical positivism and Popper's position, Feyerabend says that they both "give an inadequate account of the past development of science and are liable to hinder science in the future. They give an inadequate account of science because science is much more 'sloppy' and'irrational' than its methodological image".[40] It is from these observations about the Popperian and the positivistic views that Feyerabend gradually arrives at his anarchism. Thus a step is taken toward anarchism when it is observed that, "what appears as 'sloppiness', 'chaos' or 'opportunism' when compared with such laws [of reason] has a most important function in the development of those very theories which we today regard as essential parts of our knowledge of nature".[41] From here he goes on to say that "Without 'chaos', no knowledge", and then to conclude that: '<u>even within</u> science reason cannot and should not be allowed to be comprehensive and that it must often be overruled, or eliminated, in favour of other agencies. There is not a single rule that remains valid under all circumstances and

not a single agency to which appeal can always be made'.[42] From such observations he claims (somewhat unwarrantedly, as we shall see) that no rule is valid, that no method is valid, and that there is no significant difference between science and irrationality or between science and mythology. Feyerabend goes further than this, and makes his most stunning claim, that anything goes in science. And he claims that anarchism is an excellent medicine for epistemology and for the philosophy of science.[43] Feyerabend defends this position throughout Against Method.

It is also possible to trace the relation between Feyerabend's anarchism and Popper's writing in a different way. The similarity between science and myth, and the presence of irrationality and prejudice in science, which Feyerabend advocates, are hinted at at the end of Popper's Scientific Discovery. Popper describes Bacon's inductive method as Bacon's 'myth of a scientific method that starts from observation and experiment and then proceeds to theories'.[44] On the other hand Popper also agrees with Bacon in describing our own contemporary science "as consisting of 'anticipations, rash and premature' and of 'prejudices' ".[45] What Popper notes about Bacon's method, Feyerabend says about all scientific method; and what Popper describes of our own contemporary science, is taken by Feyerabend to its extreme. One may thus reasonably wonder

whether Popper's 'Path of Science'[46] leads Feyerabend to his anarchism.

In the course of this thesis, however, we shall see that Feyerabend's conclusions are invalid; but, so far, it should be clear that his anarchism develops, to a great extent, as a reaction to the ideas of Popper and the positivists; and secondly, that in its anarchistic renunciation of all rules, methods and rationality, it is itself too negative a view. We will see that this negativism does not succeed in showing that it will do any good to science, that it will enhance progress in science. Moreover, as we will see, epistemological anarchism will lead to scepticism.

## §0.6: Some General Comments.

So far we have seen the chain of successive developments of the three major epistemological negativist positions. An attempt will be made in this thesis to show that these epistemological negativist positions fail to undermine scientific knowledge as they appear to do. In my attempt to do that I shall examine three negativist philosophers, each representing one type of negativism, and each being a major representative of his own type.

We have seen that verificationism is unsatisfactory

in the context of scientific knowledge, and that it leads to scepticism. The question that we have to answer, then, is whether scientific knowledge can withstand scepticism. I shall deal with this question, taking a prominent and recent case of rigorous scepticism. I shall examine Peter Unger's sceptical views to see whether we can obtain certainty and whether we can be rational in believing empirical statements. Similarly, I shall examine the negativist views of fallibilism, taking the position of one prominent and recent fallibilist philosopher, Karl Popper. Next, in the same way, I shall examine the negativist position of the epistemological anarchist, Paul Feyerabend. Before examining these three particular philosophers and their negative ideas, I shall, however, give formulations of the three negative positions: scepticism, fallibilism and epistemological anarchism.

## Notes: Introduction

[1]Mach, E., Popular Scientific Lectures, tr. T.J. McCormack (Chicago: Open Court, 1898), p.2, my italics.

[2]Ibid., pp. 194-95.

[3]Ibid., p. 195.

[4]Ibid., p. 198, my italics.

[5]Ibid., p. 181.

[6]Ibid., p. 227, my italics.

[7]Ibid., p. 237.

[8]Mach, E., The Analysis of Sensations, tr. C.M. Williams (New York: Dover Publications, 1950), p.316.

[9]Paul Carus says, in his acknowledgement of Mach's reductionism, that: 'As soon as Professor Mach loses the solid ground of concrete facts he feels the thin air of abstraction, and he has a deep seated prejudice against anything that is not tangible or sensible' ("Professor Mach and his Work", The Monist 21(1911), p.33).

[10]Mach, E., Popular Scientific Lectures, p. 189.

[11]Ayer, A.J. ed., Logical Positivism (Illinois: The Free Press, 1959), p.4. Cf. also Carnap, R., Hahn, H. and Neurath, O., Wissenschaftliche Weltauffassung: Der Weiner Kreis (Vienna: Wolf, 1929).

[12]Ayer, A.J., Logical Positivism, p. 4.

[13]Schlick, M., "Positivism and Realism", in A.J. Ayer ed., Logical Positivism, pp. 86-87.

[14]*Ibid.*, p. 87.

[15]*Cf.*, *ibid.*, p. 97.

[16]*Ibid.*, p. 90.

[17]Carnap, R., "The Elimination of Metaphysics through Logical Analysis of Language", in A.J.Ayer ed., *Logical Positivism*, pp.64-65. Carnap actually gives four formulations, the above quoted one being one of them; but he considers that 'ultimately [they] say the same thing' (*loc. cit.*).

[18]Carnap, R., *The Unity of Science* (Kegan Paul: 1934), p.42.

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

[20]Neurath, O., "Protocol Sentences", in A.J.Ayer ed., *Logical Positivism*, p. 201.

[21]Schlick, M., "Positivism and Realism", p. 88.

[22]Ayer, A.J., *Language, Truth and Logic* (New York: Dover Publications, 1952), p. 9.

[23]Carnap, R., "Testability and Meaning", in H.Feigl and M.Brodbeck ed., *Readings in the Philosophy of Science* (New York: Appleton-Century-Crofts, 1953), p.47.

[24]See for example, H. Reichenbach, ("Logistic Empiricism in Germany and the Present State of its Problems", *Journal of Philosophy* 33[1936],p.149) who regarded Carnap's position as 'after a fashion', "a modern fulfillment of Descartes' quest for an absolutely certain basis of science".

[25]Lewis, C.I., *Mind and the World-Order* (New York: Charles Scribner's Sons, 1929), pp.280-81, my italics.

[26]Lewis, C.I., *An Analysis of Knowledge and Valuation* (La Salle: Open Court, 1950), p. 180.

[27]Malcolm, N., "The Verification Argument", in M. Black ed., *Philosophical Analysis* (Ithaca: Cornell

University Press, 1950), pp.248-49. Reprinted in N. Malsolm, Knowledge and Certainty (Englewood Cliffs: Prentice-Hall, 1964). References are from M.Black, Philosophical Analysis.

[28]I have elaborated on this point in §3.1 and at the beginning of §3.2 (chapter 3).

[29]Popper, K., The Logic of Scientific Discovery (New York: Harper and Row, 1968), p. 36; henceforth referred to as Scientific Discovery.

[30]Ibid., p. 51.

[31]Ibid., pp. 37; also cf. pp. 40, 51.

[32]Ibid., p. 51.

[33]Ibid., p. 51.

[34]Ibid., p. 35. For further criticisms of Popper's, see §§3.1 and 3.2 below.

[35]Popper, K., Scientific Discovery, p. 278.

[36]Popper, K., "On the so-called 'Logic of Induction' and the 'Probability of Hypotheses', in Scientific Discovery, p. 317; first published in Erkenntnis, 1935.

[37]Feyerabend, P., Against Method (London: NLB, 1975), p. 176.

[38]Cf. Popper, K., Scientific Discovery, §3, esp. p.32.

[39]Feyerabend, P., Against Method, pp. 175-76.

[40]Ibid., p. 179.

[41]Ibid., p. 179.

[42]Ibid., pp. 179-80.

[43]Ibid., p. 17.

[44]Popper, K., Scientific Discovery, p. 279.

[45]Ibid., p. 278, Becon quoted.

[46]Popper, K., Scientific Discovery, §85, pp.276-81.

# Chapter 1

## Formulation of the Negative Positions

### §1.1: Scepticism.

The formulation of scepticism, like that of any epistemic position, has its problems because such positions usually mask many different sub-positions, and most formulations ignore the different reasons for which such a position may be held. For example, scepticism about the external world is quite different from ethical scepticism. In this section, I shall try to formulate scepticism without addressing myself to the usual sub-positions, though I shall confine myself to the context of scientific and empirical statements and knowledge claims.

Since scientific knowledge is based on observation, evidence and analysis, sceptical denial of empirical and scientific knowledge may be based on the denial of one or more of these three as an acceptable basis for knowledge claims. Their acceptability may be denied by claiming that they may be erroneous, or that they fall short of certainty, or fail to provide conclusive justification. One can be mistaken in one's observation or in one's evidence for a particular claim. One can also make mistakes in one's analysis

of the observed facts and gathered evidence. A person may wonder whether such a possibility will render all empirical claims doubtful, or possibly erroneous and inconclusively justified, though analytic statements may not fail in any of these three ways. For example, the statement that a bachelor is an unmarried man does not require observation, evidence and analysis the way empirical statements usually do. And there is no question of error in this instance of an analytic statement, for it is correct by definition. But not all analytic statements are as simple as this example. Many analytic statements are in fact based on complicated derivations and proofs; it is quite possible to make a mistake in such derivations. Consequently, many analytic statements may be subject to doubt. Moreover, many derivations or proofs of analytic statements may be based on some supposition; one can, therefore, raise a question whether or not these analytical derivations are conclusively justified. We may thus hold that the scepticism about scientific knowledge, whether empirical or analytical, is based on one of the three grounds, viz. (i) doubt; (ii) the possibility of error (either in the statements themselves or in their derivation); (iii) and the possibility that the justification provided for the statements may be inconclusive.

It may be noted here that all these three grounds

are closely interrelated. For example, the possibility of error may raise doubt whether or not a statement is true; on the other hand, if there is any doubt about a statement, then one may wonder whether something has gone wrong. Inconclusive justification is also related with the possibility of error and doubt in the same way. Each of these three sceptical grounds may now be elaborated. In doing this I shall give account of three prominent sceptical philosophers, viz. Sextus Empiricus, Descartes and Peter Unger.

All three sceptical grounds are already present in the ancient Pyrrhonian school. Pyrrhonian arguments which lead to sceptical suspension of judgment are called 'modes' or 'tropes'. There are a number of sets of modes; each mode refers to a particular situation in which a judgment is to be suspended.

In the first place, Pyrrhonism would render all statements about the external world doubtful on the ground that we can never make assertions about the reality which is behind our observations, that we can only make assertions about the appearances.[1] That it is not possible to make such assertions about the external world is defended by two sorts of arguments: by arguments to the effect that the empirical assertions are full of discrepancies, and secondly, by arguing that an attempt to defend empirical assertions would lead either to infinite regress, or to circularity or

to the adoption of an unproved hypothesis, thus rendering them inconclusive. The set of 'ten modes' are designed to give arguments of the first sort; the set of 'five modes' to give arguments of the second.

The ten modes, for the most part, give examples of the ways in which perceptions may be at variance with each other and with the real nature of the objects perceived. Some of them are still familiar in epistemology today. Thus Sextus Empiricus, for example, appeals to the perceptual and social relativity (the first and the second mode), to the effect of perspective on perception (the fifth mode), and to the possibility of conflicts in evidence presented by different senses (the third mode). Only the eighth mode offers a general argument, namely that since all things are related, the real nature of any one of them could only be known if its (infinitely many) relations to all the others were known.

Of the set of five modes the first and the third completely overlap with modes from the first set; the second, fourth and the fifth overlap partially. The second mode is based on infinite regress and is presented in the following way: 'The Mode based upon regress ad infinitum is that whereby we assert that the thing adduced as a proof of the matter proposed needs a further proof, and this again another, and so on ad infinitum, so that the consequence is

suspension...'.[2] The fourth and the fifth modes are the following.

> We have the Mode based on hypothesis when the Dogmatists, being forced to recede ad infinitum, take as their starting-point something which they do not establish by argument but claim to assume as granted simply and without demonstration. The Mode of circular reasoning is the form used when the proof itself which ought to establish the matter of inquiry requires confirmation derived from that matter; in this case, being unable to assume either in order to establish the other, we suspend judgment about both.[3]

The Pyrrhonian sceptics held that unless we can establish an empirical claim without infinite regress, circularity or the adoption of an unproved hypothesis in our arguments, then our defence of empirical statements cannot be considered as conclusive.

In consequence, Pyrrhonian sceptics suspend all such judgments. Pyrrhonism is, however, right in the observation that all chains of arguments either end or do not; if they do not end then either they go on for ever or else at some point they turn circular. If they do end then they end with some claim not itself argued for. But in §2.3 of the following chapter, I argue that it is possible to obtain absolute certainty about such statements in spite of these objections. I have shown there that arguments (which are similar to the above Pyrrhonian ones) against the certainty

of empirical statements cannot undermine such statements. My arguments in that section show that we can overcome the discrepancies among empirical statements, and further that a possible infinite regress of justification is not sufficient to reject a claim which involves such a regress. In the following chapters 3 and 4, I also suggest that ad hoc hypotheses can be accepted in our empirical scientific investigations. With reference to my claims in the following chapters, I would say that Pyrrhonian scepticism is based on unconvincing arguments. Before leaving the Pyrrhonian position, however, I shall refer to two more points.

First, it may be asked: can the Pyrrhonian sceptical claim to suspend all assertion survive suspension? Sextus Empiricus holds that:

> in regard to all the Sceptic expressions, we must grasp first the fact that we make no positive assertion respecting their absolute truth, since we say that they may possibly be confuted by themselves, seeing that they themselves are included in the things to which their doubt applies... .[4]

It is further held that, 'the Sceptic enunciates his formulae so that they are virtually cancelled by themselves'.[5] Pyrrhonian scepticism seems to be self-refuting, and, moreover, admitted as such by its advocates.

Next, it may be asked that: if Pyrrhonian scepticism

may possibly be self-refuting, then why is it put forward? The main reason is that: 'as a consequence of this [scepticism] we end by ceasing to dogmatize'.[6] In other words, scepticism is considered here as the antithesis of dogmatism: unless we are sceptics, we will be dogmatic. But this also is not right. In the following §2.3, we will see that this alleged dichotomy of scepticism and dogmatism has survived even up to the present in discussions of scepticism; it is shown in that section that it is possible to hold a middle ground between scepticism and dogmatism. And this plea of Pyrrhonism, as we will see, also remains unconvincing.

However, whether or not Pyrrhonism has succeeded in establishing scepticism, arguments similar to the Pyrrhonian ones, have reappeared in the history of philosophy in varied forms. The claim that all our sense-impressions (and much else besides) are doubtful and possibly deceptive is also contended in Descartes' evil genius argument.

Descartes intends to find out what is true and certain and for this purpose he wants to reject everything that is doubtful. He says:

> inasmuch as reason already persuades me that I ought no less carefully to withhold my assent from matters which are not entirely certain and indubitable than from those which appear to me manifestly to be false, if I am able to find in each one some reason to doubt, this will suffice to justify my rejecting the whole.[7]

But what Descartes finds most doubtful are the senses. He says: 'it is sometimes proved to me that these senses are deceptive, and it is wiser not to trust entirely to anything by which we have once been deceived'.[8] According to this view, if a person claims that he is now awake and that he can see that he has two hands, even then this claim can be doubted, because, one can have such experiences in dreams, and one may fail to distinguish between the dream experience and a real experience. In this way, according to Descartes, we may make mistakes in any of our judgments regarding experience.

A further reason for consequent doubt about our empirical judgments is attributed to the possible deception of an evil genius. Possibly 'some evil genius not less powerful than deceitful, has employed his whole energies in deceiving me; I shall consider that the heavens, the earth, colours, figures, sounds, and all other external things are nought but the illusions and dreams of which this genius has availed himself'.[9] Cartesian scepticism, thus, like Pyrrhonian scepticism, rejects all empirical statements; every such statement is rendered doubtful by considering them as illusions created by an evil genius. In other words, if one claims to be sure about any such statement, one is in error.

The deception of the evil genius, as a reason for

scepticism, has also reappeared in recent sceptical arguments. One of the main sceptical arguments which Peter Unger formulates has superficial similarities to the deception of the evil genius (this argument is presented in detail in the following chapter). But Unger's argument is actually completely different from the Cartesian argument, and is presented as an argument from the closure of knowledge sets.

Both Descartes' and Unger's arguments are examined in detail in the following chapter of this thesis. These sceptical arguments are shown to be unsatisfactory. But whether or not any of the above discussed arguments have succeeded in establishing scepticism, they have at least shown what scepticism attempts to establish. Since we have undertaken to formulate scepticism in the context of empirical and scientific knowledge, scepticism, for our purpose, claims to undermine any claim to know an empirical and scientific statement either on the grounds that such statements are doubtful, or erroneous or inconclusively justified. We may thus state scepticism as the doctrine which denies the possibility of knowledge, or more weakly denies the existence of knowledge. With this view of scepticism, we may now proceed to formulate the next negative epistemological position: fallibilism.

§1.2: Fallibilism.

The central theme of fallibilism, as is obvious from the term itself, is the fallibility of human beliefs and knowledge. One may wonder why one should be interested in fallibilism, i.e. in the general theme about the mistakes in human knowledge. One reason is the desire to avoid mistakes. But in epistemology the fallibilist is typically more concerned to emphasize how universal such mistakes — or at least the possibility of such mistakes — are. And a main motive for doing this is to avoid the perils of dogmatism. In motivation, at least, fallibilism is close to scepticism. Some fallibilists in fact have turned out to be sceptics; and some of the currently available formulations of fallibilism do lead to scepticism. We will come across these points later in this chapter. This introductory account of fallibilism gives an idea of the intuitive motivation which may work behind the fallibilist's position. In my attempt to formulate fallibilism, I shall duly emphasize this aspect.

This intuitive motivation about fallibilism may be captured in the following statement: any of our beliefs may be false. The non-dogmatic aspect of fallibilism is stressed by affirmation that our beliefs may be false - a view which it shares with scepticism. On the other hand, what differentiates fallibilism from scepticism, at least in

the formulation we have primarily concerned with, is that fallibilism is not prepared to reject all knowledge. The chief difference between fallibilism and scepticism is that while scepticism denies all knowledge claims, fallibilism denies only certain knowledge claims.

However, if we take the above tentative statement of the fallibilist position at face value we are quickly involved in difficulties. For the convenience of critical analysis, I shall formalize this tentative statement of fallibilism in the following way.

(1) $(p)(\Diamond Bp \ \& \ \Diamond \sim p)$,

which reads: for any p, it is possible to believe p though p may be false.[10] This is Karl Popper's position; to make this clear, we may briefly consider Popper's case.

Popper presents his view as follows.

> the falsificationists or fallibilists - say, roughly speaking, that what ... can [at present] in principle be ... overthrown [by criticism] and yet resists all our critical efforts to do so may quite possibly be false, but is at any rate not unworthy of being seriously considered and perhaps even of being believed - though only tentatively.[11]

Popper formulates fallibilism in the light of his falsificationist position, and that is why his fallibilism holds for those statements which in principle can be overthrown by falsification. Since falsificationism is a theory

regarding the statements of the empirical sciences, one may wonder whether both metaphysical and mathematical statements are left out from Popper's fallibilism. One may, however, reply that Popper's fallibilist position should be applicable to mathematical and metaphysical statements as much as it is applicable to empirical statements. This is because Popper's falsificationist position, unlike the verification principle, is not a meaning criterion. According to this position metaphysical and mathematical statements can surely be mistaken. Moreover, both mathematical and metaphysical statements can be overthrown by criticism, i.e. by the fallibilist criterion which is stated in the above quoted passage. A closer examination of this problem, viz. the relation between Popper's fallibilism on the one hand, and methematical and metaphysical statements on the other, will not only clarify his position but also help us to evaluate it.

Let us consider the following statements from Popper. Popper says: "The problem of finding a criterion which would enable us to distinguish between the empirical sciences on the one hand, and mathematics and logic as well as 'metaphysical' systems on the other, I call the problem of demarcation".[12] Next we see that, 'the falsifiability of a system is to be taken as a criterion of demarcation'.[13] And lastly, that 'Falsifiability separates

two kinds of perfectly meaningful statements: the falsifiable and the non-falsifiable'.[14] These three statements together imply that metaphysical and mathematical statements are meaningful, but non-falsifiable, and demarcated from empirical statements which are falsifiable. Popper's criterion of falsifiability is thus a criterion which deals with the falsifiability of empirical statements. Since Popper formulates his fallibilism in terms of his falsificationism,[15] it is clear that his fallibilism fails to take account of the fallibility of mathematical and metaphysical statements, and his notion of falsifiability means something different than overthrowing by criticism. But I have noted above that both mathematical and metaphysical statements can be overthrown by criticism, and presumably, a complicated mathematical statement can be false.[16] Since I have said in the above that (1) is Popper's position, the limitation of Popper's view can be made clear by examining (1) in the following way.[17]

Under the apparent simplicity and the intuitive appeal of (1), however, are its latent limitations - in particular problems noted by Susan Haack.[18] From (1), we can get the following derivation. Instantiating some constant proposition $p_o$ for $p$ in (1) gives $\Diamond Bp_o \ \& \ \Diamond \sim p_o$, which is true just in case both conjuncts are true. But if $p_o$ is a necessary proposition then we have $\sim \Diamond \sim p_o$ and thus

fallibilism is false for necessary propositions. Yet the intuitive motivation for fallibilism was to emphasize the uncertainty of human knowledge, and there seems no reason why this uncertainty should not extend to our knowledge of necessary truths. In fact, (1) amounts to the claim that there is no necessary truth. If so, (1), which we have presented as a tentative statement of fallibilism, is false. And this generates a problem regarding the compatibility of necessity and fallibility. We have already seen this problem in Popper's position; Popper's fallibilism fails to take account of the fallibility of mathematical statements. In fact Haack's whole paper is devoted to the resolution of this problem (though Haack does not discuss Popper's position). It originates in the following way: on the one hand, necessary truths cannot be false, while, on the other, belief in them is clearly fallible. The fallibility of our belief in a necessary statement becomes apparent when we consider a complicated mathematical statement as an example. It is not uncommon that we make a mistake in the derivation of a mathematical statement, and consequently we may falsely believe that the derived statement is necessarily true. Since our first intuitive attempt to formulate fallibilism is vitiated by this problem, we must first discuss how this can be resolved.

We may consider here Haack's attempt to deal with

the problem. Haack presents her definition of fallibilism in the following way:

(F*) $(p)(\sim\Box(Bp \rightarrow p) \vee \Diamond B\sim p)$,

which reads: " 'for all p, either it is not the case that, necessarily, if we believe that p, then p, or else, it is possible that we should believe that ~p' ".[19] The fallibilist position (F*) is the negation of the thesis (D*), what Haack says[20] is the representation of dogmatism:

(D*) $(\exists p)(\Box(Bp \rightarrow p) \& \sim\Diamond B\sim p)$.[21]

It should be noted here that both in (F*) and (D*), '$\Diamond$' is taken as psychological possibility, while '$\Box$' is taken as logical necessity. '$\Diamond$' is considered here as psychological possibility, because both the logical and epistemic readings of '$\Diamond$' are unsatisfactory. If $\Diamond B\sim p$ in (F*) is read as 'it is logically possible to believe the falsity of any statement', then this is a too weak reading of '$\Diamond$'. On the other hand, if $\Diamond B\sim p$ is read by taking '$\Diamond$' as epistemic, then it becomes inappropriate. This is Haack's main reason[22] for reading '$\Diamond$' as neither logical nor epistemic. Haack does not explicitly argue in this way in connection with (F*) or (D*), but this argument is given in the preliminary discussion which leads to (F*). Haack says that, 'Epistemological possibility seems quite inappropriate, and logical possibility perhaps too weak...; psychological possibility seems most promising (NB

psychological possibility presumably entails logical possibility)'.[23]

Haack considers that one fundamental merit of her formulation (F*) is that it reconciles necessary statements with fallibilism. This may become clearer if we write (F*) in the following way:

(2) $(p)(\Box(Bp \rightarrow p) \rightarrow \Diamond B\sim p)$.

The antecedent of (2) asserts that there are statements which, necessarily, if we believe them then they are true. The consequent of (2) asserts that we can psychologically deny such logically necessary statements. Thus (2), unlike (1), does not entail that there are no necessary truths.

However, Haack's (2) has its own problems, pointed out by P.L.Mott. Mott utilizes the weak modal system T. We then get,[24]

(3) $\Box p \rightarrow \Box(Bp \rightarrow p)$.[25]

From (2) and (3), we get,

(4) $\Box p \rightarrow \Diamond B\sim p$.

Substituting '~p' for 'p' in (4) and after quantifying, we get,

(5) $(p)(\Box\sim p \rightarrow \Diamond Bp)$.

To dispense with the antecedent, we assume for *reductio* that there is a statement q which cannot possibly be believed. We then get,

(6) $\sim\Diamond Bq$.

By a theorem of T, we get,

(7) $\sim\Diamond(Bq \ \& \ q)$.

Since '$\sim\Diamond$' is equivalent to '$\Box\sim$', we can apply (5) and get:

(8) $\Diamond B(Bq \ \& \ q)$.

Since Haack uses '$\Diamond$' in the psychological sense, one may wonder how '$\sim\Diamond$' could be equivalent to '$\Box\sim$'. Consequently, one may not accept (8) in Mott's derivation. However, this difficulty in Mott's argument can be amended in the following way.

Haack says that psychological possibility entails logical possibility; i.e. $\Diamond^{\Psi}p \rightarrow \Diamond p$, where '$\Psi$' stands for 'psychological'. But $\Diamond^{\Psi}p \rightarrow \Diamond p$ is the same as $\Diamond^{\Psi}p \rightarrow \sim\Box\sim p$, from which we can get $\Box\sim p \rightarrow \sim\Diamond^{\Psi}p$ by contraposition. Applying $\Box\sim p \rightarrow \sim\Diamond^{\Psi}p$ to (5) we get,

(5') $\sim\Diamond^{\Psi}p \rightarrow \Diamond^{\Psi}Bp$.

Substituting $(Bq \ \& \ q)$ for '$p$' in (5'), we get,

(5") $\sim\Diamond^{\Psi}(Bq \ \& \ q) \rightarrow \Diamond^{\Psi}B(Bq \ \& \ q)$.

From (5") and (7), we can now derive,

$\Diamond^{\Psi}B(Bq \ \& \ q)$,

which is the same as (8). Amending Mott's derivation in this way, finally we assume that if it is possible to believe a conjunction then it is possible to believe the conjuncts. This gives the required contradiction:

(9) $\Diamond Bq$.

Thus we can derive from (F*) that,

(10) $(\underline{p})\Diamond Bp$.

From Haack's definition, Mott derives[26] a conclusion which not only renders the definition trivial,[27] but also is opposed by Haack herself. Regarding above (10), i.e. $(\underline{p})\Diamond Bp$, Haack says that: 'But the truth of this claim seems doubtful'.[28] We thus see that Haack's definition cannot be correct.

The triviality of Haack's position is at least partially due to the approach which is taken in her formulation. This is a negative approach: fallibilism has been presented here as an antithesis of dogmatism. (F*) is the negation of (D*). As Mott notes, 'Haack characterises fallibilism by what it denies'.[29]

To present fallibilism in this way is to misrepresent it; moreover, if the denial of dogmatism is the sole purpose, then scepticism can also do that. Our attempt to formulate fallibilism, without classifying it as scepticism, may indicate that the purpose of formulating fallibilism is not only more than the denial of dogmatism, but also other than what scepticism asserts. Moreover, there is some doubt[30] regarding the correctness of Haack's formulation of dogmatism.

A better approach to defining fallibilism is to start with the beliefs rather than the statements believed. Consider, first, the following.

(11) $(p)\sim JaBap$,

which reads: for any p, one is not justified in believing p. This formulation captures the idea of the fallibility of any of our beliefs. But a moment's reflection will make it clear that this formulation captures the idea too strongly. For to say that a is not justified about any belief is to render knowledge impossible. In fact by plainly denying justification of any belief, (11) leads to total scepticism. Since our aim is to define a form of fallibilism distinct from scepticism, (11) is unacceptable.

Since (11) does not admit any knowledge claims and as such is too strong, can it be made acceptable by weakening it? Instead of saying that for any p, a is not justified in believing p, we could say: for any p, a may not be justified in believing p. We would then get,

(12) $(p)\Diamond\sim JaBap$.

This formulation seems better for a number of reasons. First, that one may not know does not mean that it is impossible to know nor even that there is no knowledge. Thus scepticism has been restricted. But one could also ask: what exactly does 'may not' mean here? To put it in a different way, how should we interpret the operator '$\Diamond$'? Or, does it make any difference to interpret '$\Diamond$' in different ways? We shall investigate these questions in the following discussion.

In the first place, we shall not consider '$\Diamond$' to be psychological possibility. Since '$\Diamond$' is used in (12) for the possibility that justification is false, and since justification depends on evidence, psychological possibility would be inappropriate. Therefore, '$\Diamond$' is to be interpreted either as logical or as epistemic possibility. First let us consider '$\Diamond$' in the logical sense; (12) can now be written as:

(13) $(p)\Diamond_l \sim JaBap$.

What does it mean to say that it is logically possible that a is not justified in believing p? The logical possibility of a's being mistaken means that it is not logically inconsistent to hold that a is not justified in believing p. To put it simply, it is not self-contradictory for a to believe p. But this is too weak a version of fallibilism. All it denies is that there are any beliefs which have logically necessary justification. It simply states that it is contingent that one's belief is justified.

The only option now left is to consider the operator '$\Diamond$' in the epistemic sense. We would then get,

(14) $(p)\Diamond_e \sim JaBap$,

which reads: 'it is epistemically possible that a is not justified about p'. In other words, it is possible for a that his belief is unjustified. But does (14) meet other requirements we have set down for our formulation?

In the first place, (14) obviously is not dogmatic. Since according to (14) any of our beliefs could be epistemically unjustified, we cannot claim to be certain about any such belief. But does it mean that we can know nothing and that (14) collapses to scepticism? A little reflection will make it clear that (14) does not lead to scepticism. When we say that 'it is epistemically possible that a's belief in p is not justified', it does not mean that it is epistemically impossible for a's belief in p to be justified. Our position is $\Diamond_{\varepsilon}\sim JaBap$ (it is epistemically possible that a is not justified), not that $\sim\Diamond_{\varepsilon} JaBap$ (it is epistemically impossible for a to be justified). On the other hand, $\Diamond_{\varepsilon}\sim JaBap$ is equivalent to $\sim\Box_{\varepsilon} JaBap$. Therefore, $\Diamond_{\varepsilon}\sim JaBap$ is equivalent to the claim that a's justification for his belief is not epistemically necessary. But necessity of justification is not necessary for knowledge claims.

On the basis of the above discussion, we may hold that (14) gives us the required formulation of fallibilism. I shall sum up the three possible cases I have discussed, and characterize them as follows.

(11) Strong fallibilism: $(p)\sim JaBap$.

(11) collapses to scepticism as regards knowledge and certainty.

(13) Ultra-weak fallibilism: $(p)\Diamond_{\lambda}\sim JaBap$.

(13) does not lead either to dogmatism or to scepticism; knowledge, justification and

certainty - all are possible. But it is too weak and fails to capture the obvious theme of human fallibility and the negativist tendency which it is supposed to represent.

(14) Weak fallibilism: $(p)\Diamond_{\varepsilon}\sim JaBap$.

(14) does not lead to dogmatism; justification and knowledge are attainable. But it does not admit the possibility of (absolute) certainty; to this extent it is negative.

We may note that (14) does not involve the triviality which renders Haack's formulation unsatisfactory. It also does not commit the mistake which Haack is concerned to avoid, viz. the problem of the fallibility of necessary statements. Many complicated mathematical statements do depend on justification and according to (14), their justification can be false. Lastly, (14) is not obviously false, and it captures what fallibilism is all about, namely the fallibility of beliefs. With this formulation of fallibilism, we may now attempt to formulate our next negative position: epistemological anarchism.

## §1.3: Epistemological Anarchism.

Epistemological anarchism, being a very recent trend, has not yet been well developed as a concept. As we shall see, some ideas are borrowed from other fields, e.g. politics and art. On the other hand some other ideas used in explaining the view remain undeveloped and incomplete;

e.g. both 'anarchism' and 'progress' suffer in this regard. In this section I shall present a consolidated view of the different aspects of epistemological anarchism by examining what the epistemological anarchist says about this position.

The leading exponent of epistemological anarchism is Paul Feyerabend. We observe that Feyerabend's most developed epistemological work 'is written in the conviction that anarchism, while perhaps not the most attractive political philosophy, is certainly excellent medicine for epistemology, and for the philosophy of science'.[31] Feyerabend tries to defend his position in detail, but he does not explain what he means by 'anarchism'; and it seems that he may not even be sure about it. He says, "When choosing the term 'anarchism' for my enterprise I simply followed general usage", and then continues: 'However anarchism, as it has been practised in the past and as it is being practised today by an ever increasing number of people has features I am not prepared to support'.[32] It is thus clear that Feyerabend accepts the general usage of 'anarchism', though not the general anarchistic practice. The term itself, therefore, is not much help to us.

Epistemological anarchism is against all law and order in scientific and epistemological methodology; standards imposed by scientists and logicians upon

knowledge-creating and knowledge-changing activity are to be rejected; and individuals are to be permitted to develop freely, and unhampered by intellectual rules, duties and obligations.[33] This general rejection of law and order results in the rejection of all methods in science and epistemology. Consequently, an anarchistic '<u>Proliferation of theories</u>'[34] is advocated.

The rejection of all methodological constraints is aimed at obtaining complete freedom in scientific practice. This is clearly admitted in connection with epistemological anarchism when it is said that, "One should remember that the debate is about methodological rules only and that 'freedom' now means freedom <u>vis-à-vis</u> such rules", and that '... epistemological anarchism ... removes only the methodological constraints'.[35]

One main characteristic of epistemological anarchism is expressed by the slogan '<u>anything goes</u>'.[36] In fact Feyerabend attempts to establish this anarchistic claim, directly or indirectly, throughout the whole of <u>Against Method.</u> Feyerabend tries to derive it from his rejection of law and order. In fact, the claim that <u>anything goes</u> represents the main spirit of Feyerabend's epistemological anarchism.

The rejection of law and order, according to Feyerabend, must not endanger human life and happiness. And

there is also 'no need to fear that the diminished concern for law and order in science and society that characterizes an anarchism of this kind will lead to chaos'.[37] This view that epistemological anarchism must not harm us, and that it is instead necessary for human benefit, is presented later in a different way. In connection with defending anarchism in science and scientific knowledge, Feyerabend first points out what he considers to be the dangers of present day science:

> ... is it not possible that science as we know it today, or a 'search for the truth' in the style of traditional philosophy, will create a monster? Is it not possible that it will harm man, turn him into a miserable, unfriendly, self-rightous mechanism without charm and humour?[38]

It is also asked whether or not one's activity as an objective observer of nature will weaken one's strength as a human being.[39]

In regard to these questions, Feyerabend says that: 'I suspect the answer to all these questions must be affirmative and I believe that a reform of the sciences that makes them more anarchistic and more subjective ... is urgently needed'.[40] But whether or not anarchism can produce such positive results remains to be shown.

The concern for human benefit is also expressed in Feyerabend's discussion of the relation between

epistemological and political anarchism. It is held that, 'While the political or the religious anarchist wants to remove a certain form of life, the epistemological anarchist may want to defend it, for he has no everlasting loyalty to, and no everlasting aversion against, any institution or any ideology'.[41] Removing any form of life, and opposing any institution or ideology may involve violence, which Feyerabend opposes. In connection with his discussion on political and religious anarchism, Feyerabend observes that 'Violence, whether political or spiritual [i.e. religious], plays an important role in almost all forms of anarchism. Violence is necessary...'.[42] This, however, is an overstatement. Though it is not impossible to find in history instances of the association of anarchism and violence, yet to characterize anarchism in this way is to misrepresent it.[43] But by associating violence with political and religious anarchism, Feyerabend expresses his aim to keep epistemolgical anarchism separate from the political and religious types. But it is not clear that epistemological anarchism precludes violence; for if anything goes then ad baculum arguments go too.

Feyerabend's reservations about the term 'anarchism' lead him to wonder whether 'dadaism' would not be a better term for his position:

> It [anarchism] cares little for
> human lives and human happiness ...

> and it contains precisely the kind of Puritanical dedication and seriousness which I detest. ... It is for these reasons that I now prefer to use the term Dadaism. A Dadaist would not hurt a fly - let alone a human being.[44]

This view of Feyerabend's regarding dadaism, apart from showing the desire for peace, also indicates some aspects of epistemological anarchism other than those we have already encountered. For this reason I may present here a brief discussion of dadaism.

In the first place, Feyerabend's preference for dadaism indicates that epistemological anarchism is not a serious enterprise. This is reinforced by Feyerabend's observation that 'A Dadaist is utterly unimpressed by any serious enterprise... . A dadaist is convinced that a worthwhile life will arise only when we start taking things lightly...'.[45]

We may also consider the following views from Dadaists themselves:"A Dadaist is someone who loves life in all its uncountable forms,and who knows, and says that, 'Life is not here alone, but also there...".[46] And:

> Dada was the effective ... expression of a mighty surge of freedom in which all the values of human existence ... were brought into play, and every object, every thought, turned on its head, mocked and misplaced, as an experiment, in order to see what there was behind it, beneath it, against it, mixed up

> in it... . Dada was a state of mind feverishly exalted by the freedom virus, a unique mixture of insatiable curiosity, playfulness and pure contradiction.[47]

This passage clearly states the main aspects of dadaism. It is the spirit of playful curiosity and experimentation with human existence and values and pure contradiction that Feyerabend shows when he denounces seriousness.

The association of epistemological anarchism and dadaism reveals another significant aspect of Feyerabend's position. Though law and order are denounced by epistemological anarchism, yet no positive programme is presented for the advancement of science and knowledge. This is revealed when Feyerabend gives a quotation from Hans Richter - a dadaist. Richter says: 'Dada not only had no programme, it was against all programmes. Dada's only programme was to have no programme...'.[48]

However, since Feyerabend presents this utterly negative position, a question may be raised whether or not his position is nihilistic. In his main work, Against Method, Feyerabend does not address himself to the problem of nihilism. It is likely that, in the spirit of anarchism, Feyerabend may let any value go. It is indeed claimed, as we shall see shortly, that epistemological anarchism will lead to progress and development. Whether or not this claim is right, at least it shows an anti-nihilistic spirit.

Unlike nihilism, epistemological anarchism denies only the methodological requirements of knowledge, and not moral values; and unlike nihilism, epistemological anarchism accepts everything except methodological requirements.[49]

Epistemological anarchism is also considered as a view which will promote progress. It is said that: '<u>Science is an essentially anarchistic enterprise: theoretical</u> [i.e. epistemological] <u>anarchism is more humanitarian and more likely to encourage progress than its law-and-order</u> alternatives'.[50] It is further held that 'anarchism is not only <u>possible</u>, it is <u>necessary</u> both for the internal progress of science and for the development of our culture as a whole'.[51] But epistemological anarchism does not explain what it means by 'progress'.[52]

It may be noted here that Feyerabend's view indicates one assumption on which epistemological anarchism is based. The assumption is that 'man naturally contains within him all the attributes which make him capable of living in freedom and social concord'.[53] This is considered as an assumption which is accepted by all anarchists. Feyerabend's epistemological anarchism also seems to be based on this assumption.

With the above discussion of the main features of epistemological anarchism, I may now attempt to present a

consolidated view of it. First, I shall emphasize once more that this is an epistemological position; the anarchistic rejection of all law and order is only in the context of science and epistemology. But the general spirit of this position is present in other anarchistic positions too, though not all characteristics of other anarchistic positions are accepted by the epistemological version. For example, violence is not accepted by epistemological anarchism. But it may be said that one general characteristic of anarchism is 'its deliberate avoidence of rigidly systematic theory, and, above all, its stress on extreme freedom of choice and on the primacy of the individual judgment'.[54] This is completely accepted by the epistemological version of anarchism.

However, anarchism in the context of science and epistemology is defined by Kropotkin in the following way.

> Anarchism is an attempt to apply to the study of human institutions the generalizations gained by means of the natural-scientific inductive method; and an attempt to foresee the future steps of mankind on the road to liberty, equality, and fraternity, with a view to realizing the greatest sum of happiness for every unit of human society.[55]

This definition will not be acceptable to Feyerabend, the leading exponent of epistemological anarchism, because it accepts the methodology of natural sciences.

Epistemological anarchism has also been formulated in the following way: 'Its proponents advocate that there is a significant amount of irrationality in the development of and choice between scientific theories'.[56] Or that, 'Briefly characterized, the thesis is that there is no meaning contact or logical contact between theories and that there are no extratheoretical standards by which to judge a theory or to choose between competing theories'.[57] But this formulation does not convey the whole point of epistemological anarchism, and does not capture the anarchistic spirit of 'anything goes'.[58] Nersessian formulates epistemological anarchism by taking a partial account of Feyerabend's anarchism.

Epistemological anarchism, however, may be formulated in the following way: epistemological anarchism is a non-violent rejection of methodological requirements in science and epistemology, and on the other hand it is the claim that anything goes; it is based on the assumption that such a rejection can enhance human well-being, and lead to progress in science and knowledge.

## §1.4: The Three Negative Positions Distinguished.

So far we have considered the three positions as distinct from each other. But they are all negative, and

thus in spite of their distinctness, they are similar to one another to some degree. In this section I shall emphasize the extent of their similarity and also their differences.

I have suggested earlier in this chapter that fallibilism involves scepticism to some extent though it does not collapse into it. But this is not recognized by all exponents of fallibilism. John Kekes, for example, holds that:

> Fallibilism challenges the epistemological orthodoxy that regards knowledge as the attainable product of a reliable process of reasoning; it denies the authority, the reliability of reasoning, and consequently declares knowledge, regardless of its source, to be unattainable.[59]

Kekes' view asserts <u>a complete denial</u> of knowledge, and thus leaves no gap between scepticism and fallibilism. And if this were right, there would be no point in considering fallibilism as a distinct philosophical position.

A fallibilist's failure to keep his position separate from scepticism is due both to the inappropriate formulation of fallibilism and a confusion about scepticism. Apart from the proper formulation of fallibilism, a distinction has to be made between scepticism about knowledge and scepticism about certainty. It is the latter type of scepticism which fallibilism involves. But by admitting the

possibility of justification, and thus the possibility of knowledge, fallibilism keeps itself sufficiently distinct from scepticism. Whereas scepticism, generally speaking, denies that we can know anything, fallibilism admits the possibility that we can know many things.

But does the third negative position, epistemological anarchism, lapse into scepticism? Feyerabend would say that epistemological anarchism does not lapse. It is argued that, 'While the sceptic either regards every view as equally good, or as equally bad, or desists from making such judgments altogether, the epistemological anarchist has no compunction to defend the most trite, or the most outrageous statement'.[60] But in spite of this claim, epistemological anarchism does lapse into scepticism; this can be shown in the following way.

In the above passage, Feyerabend holds that a sceptic either regards every view as equally good, or as equally bad or desists from making such judgments. This statement regarding what a sceptic does is true if at least one of the three disjuncts is true. And thus, one can be a sceptic if he regards every view as true. By this standard of Feyerabend's, he himself is a sceptic, for he regards every view as equally good and that, <u>anything goes</u>. It may be replied that this claim, that epistemological anarchism implies scepticism, is not based on my definition of

scepticism. But it can be shown that epistemological anarchism lapses into scepticism even in my sense of scepticism.

Since, according to epistemological anarchism, there is no law and order in science and knowledge, one can believe, hold, claim and propound any view. This is what Feyerabend says: anything goes. Thus it is possible for one to make knowledge claims without any constraints, and thus to be a non-sceptic. But it seems unlikely that normally one will take such a positive course in the absence of law and order. On the contrary, one is more likely to take a negative and sceptical view. Since there is no law and order, since there is no method, one may easily find oneself unable to decide what is knowledge and what is not knowledge. Similarly, one may easily find oneself confused about what is certain and what is doubtful, what is true and what is not, and what is justified and what is not. And the inevitable consequence would be to take a sceptical view.[61]

How is epistemological anarchism related to fallibilism? I would say that they are just opposite; and a correct formulation of fallibilism can block epistemological anarchism. This is because, fallibilism can help us to construct an effective defence of justification and knowledge; and at the same time we can get an undogmatic freedom for which epistemological anarchism so strongly strives. In contrast to

epistemological anarchism, fallibilism will be able to distinguish between what goes and what does not. Fallibilism will thus block the anti-methodological aspects of epistemological anarchism. How fallibilism can do this will be clear in the course of this thesis. With these views, I shall proceed to examine the first negative position: scepticism as represented by Peter Unger's work.

## Notes: Chapter 1

[1]The Pyrrhonian arguments presented here have been taken from, Sextus Empiricus, Outlines of Pyrrhonism, tr. by R.G.Bury (London: William Heinemann, 1933), pp. 25-93 ("The Ten Modes") and pp. 95-101 ("The Five Modes").

[2]Ibid., p. 95; also cf. the fifth of "The Ten Modes", pp. 71-73.

[3]Ibid., p. 95; also cf. the fourth of "The Ten Modes", pp. 67-69.

[4]Ibid., p. 123.

[5]Ibid., p. 11.

[6]Ibid., p. 9.

[7]Descartes, R., Meditation on First Philosophy, in E.S.Haldane and G.R.T.Ross tr. The Philosophical Works of Descartes (Cambridge: at the University Press, 1970), vol. I, p. 145.

[8]Ibid., p. 145.

[9]Ibid., p. 148.

[10]This formulation coincides with Susan Haack's initial formulation: '(p)M(Bp & ~p)', (cf. her "Fallibilism and Necessity", in Synthese 41 (1979), p. 47). I shall use '□' for 'necessity' and '◇' for 'possibility' and, in the interest of clarity, I shall change quotations to follow my notations.

[11]Popper, K., Conjectures and Refutations, p. 228, my italics.

[12]Popper, K., Scientific Discovery, p. 34.

[13]Ibid., p. 40.

[14]Ibid., p. 40, note *3, my italics.

[15]See his Conjectures and Refutations, p. 228.

[16]Lakatos criticizes Popper for 'the mistake of reserving a privileged infallible status for mathematics' (I. Lakatos, Proofs and Refutations, ed. by J.Worral and E.Zahar [Cambridge: Cambridge University Press, 1976], p. 139n). For the application of Popper's falsifiability thesis on mathematical statements, cf. ibid., pp. 49, 139-40.

[17]Another feature of Popper's fallibilism is revealed by its relation to scepticism. In the context in which Popper presents the above quoted passage, it is said that: 'I may mention in passing a third group with whom I also disagree. They may be called the disappointed justificationists - the irrationalists and sceptics' (Conjectures and Refutations, p. 228). But though Popper, as a fallibilist, rejects scepticism, yet his position on the possibility of knowledge is not very firm. In the early phase of his fallibilism he holds that we do not, that we can only guess, and that we can never be sure about the empirical facts (Scientific Discovery, pp. 278, 280). In his later writings he overcomes this scepticism about knowledge but remains sceptic about certainty. His fallibilism, though anti-sceptical in intention, fails effectively to block scepticism.

[18]Haack, S. "Fallibilism and Necessity", cf. pp.47-48.

[19]Ibid., p. 52.

[20]One might expect dogmatism to be formulated universally, at least over some subset of propositions. From this standpoint, (D*) may not be considered as the proper formulation of dogmatism; this may be made clear in the following way. (D*) can be read as the claim that there are some statements which it is impossible to deny and such that believing them strictly implies their truth. But this is much weaker than dogmatism is usually assumed to be. A dogmatist does not dogmatically hold only some statements, rather all statements. Moreover, it actually appears that (D*) is true. For if 'I exist' is substituted for 'p'

in (D*), then (D*) turns to an arguable thesis. (Private communication from Dr. Nicholas Griffin).

[21]Haack, S., "Fallibilism and Necessity", p.52.

[22]Haack, however, does not explain what she means by the epistemic reading of '◇'. But if ◇B~p is read in the epistemic sense, i.e. if we claim that we can have epistemic ground to disbelieve anything then it would lead to complete scepticism. And this would be too strong, and thus inappropriate for the formulation of fallibilism.

[23]Haack, S., "Fallibilism and Necessity", p.51.

[24]The following argument (3)-(10) is taken, with slight variations and an amendment, from P.L.Mott, "Haack on Fallibilism", Analysis 40 (1980), p.178, my italics.

[25]We have been using '→' in the sense of material, but not strict, implication. It may be noted here that the use of '→' and '□' together in (3) makes it equivalent to p → (Bp → p).

[26]Mott, however, acknowledges that this derivation generates the 'paradox of the preface' (cf. op. cit., p. 178, note 4).

[27]Haack's definition will be trivial, because, if we can believe anything then any statement will confirm the step (2), which is equivalent to (F*).

[28]Haack, S., "Fallibilism and Necessity", p. 51.

[29]Mott, P.L., "Haack on Fallibilism", p. 179.

[30]See above note 20.

[31]Feyerabend, P., Against Method, p. 17. This work is an elaborated version of Feyerabend's "Against Method" in M.Radner and S.Winokur ed., Minnesota Studies in the Philosophy of Science (Minneapolis: University of Minnesota Press, 1970). I shall follow here Against Method, the later and the elaborated version.

[32]Ibid., p. 21n.

[33]Ibid., pp. 20-21.

[34]Ibid., p. 35.

[35]Ibid., p. 187n.

[36]Ibid., p. 28.

[37]Ibid., p. 21.

[38]Ibid., p. 175.

[39]Feyerabend raises this question by quoting Kierkegard, cf. ibid., p. 175.

[40]Ibid., p. 175.

[41]Ibid., p. 189.

[42]Ibid., p. 187.

[43]Woodcock, G., Anarchism (Pelican Book), pp.12-13.

[44]Feyerabend, P., Against Method, p. 21n.

[45]Ibid., p. 21n.

[46]This definition is given by Johannes Baader. Quoted from W. Haftmann, "Postscript", in H.Richter, Dada, Art and Anti-Art (London: Thames and Hudson, 1965),p.215.

[47]Ibid., p. 215.

[48]Richter, H., Dada, Art and Anti-Art, p. 34; cf., P.Feyerabend, Against Method, p. 33n.

[49]For the view that to consider anarchism as nihilism would be a misunderstanding, see G. Woodcock,

"Anarchism", in P.Edwards ed., The Encyclopedia of Philosophy (New York: Macmillan, 1972), vol. I, p. 112.

[50]Feyerabend, P., Against Method, p. 17.

[51]Ibid., p. 180.

[52]Ibid., p. 27.

[53]Woodcock, G., Anarchism, p. 19.

[54]Ibid., p. 15.

[55]Kropotkin, P., "Modern Science and Anarchism", in R.N.Baldwin ed., Kropotkin's Revolutionary Pamphlets (New York: Dover Publications, 1970), pp. 191-92.

[56]Nersessian, N.J., "The Roots of Epistemological 'Anarchy' ", Inquiry 22 (1979), p. 423.

[57]Ibid., p. 423.

[58]Nersessian's account is quite compatible with T. Kuhn's views which fall short of Feyerabend's anarchism. See T. Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1970).

[59]Kekes, J., "Fallibilism and Rationality", American Philosophical Quarterly 9 (1972), p. 301.

[60]Feyerabend, P., Against Method, p. 189.

[61]Cf. also J.G.McEvoy, "A 'Revolutionary' Philosophy of Science", Philosophy of Science 42 (1975), pp.50, 59; and E. Gellner, "Beyond Truth and Falsehood", British Journal for the Philosophy of Science 26 (1975), p. 335.

# Chapter 2

## Unger's Irrationalist Scepticism

In his book Ignorance, Peter Unger puts forward two major arguments in an attempt to establish scepticism. The first is a reformulation of the Cartesian-demon argument and is regarded by Unger as a classical form of sceptical argument. The second is his argument for universal ignorance. Along with these putatively rigorous arguments, he puts forward a number of pragmatic and persuasive appeals which I will not consider here. I shall first give a thorough examination of his two major arguments; secondly, I shall give an analysis of the notion of certainty, which will further weaken Unger's scepticism; and thirdly, I shall examine Unger's argument for irrationalism.

### §2.1: Unger and Cartesianism.

Unger presents the Cartesian argument against the possibility of knowledge of the external world. Taking as an arbitrary example the knowledge claim that rocks exist, Unger presents his argument first on pp. 7-8 of his book. The form of the argument can be resolved into the

following steps.

Premise: If one knows that there are rocks then one can know that there is no evil scientist (other than the knower) who is deceiving one into falsely believing that there are rocks.

Premise: But one can never know that some evil scientist is not so deceiving one.

Conclusion: Hence one does not know that there are rocks.[1]

The problem with this argument is that the second premise presupposes scepticism. In this premise Unger denies the possibility of a certain knowledge claim; and he uses this sceptical premise to establish his sceptical conclusion regarding an abitrary example of knowledge claims. And thus the argument commits the fallacy of _petitio principii_. Unger, however, does not elaborate on this argument, nor does he emphasize it. Rather what he emphasizes and elaborates in detail is a substantially different argument,[2] which is based on what he calls _the assumption of reasoning_. I shall first explain and examine this assumption.

According to the assumption of reasoning, a knower '_has and can apply at least a moderate amount of reasoning ability to what he knows so as to know other things which follow from it_'.[3] It says that if one knows _p_, and _p_ implies _q_, and if one is able to apply a moderate amount of reasoning ability, then one can know _q_. This can be formalized in the following way.

(1) [Kap & (p → q) & Ra] → ◇Kaq,

where 'Kap' is 'a knows that p', and 'Ra' is 'a has at least a moderate amount of reasoning ability', or simply 'a is rational'.[4]

In this formulation ~Deap may be substituted for q, where '~Deap' is 'a is not being deceived by an evil scientist e into falsely believing that p';[5] because p must imply, among other things, that one cannot be deceived into falsely believing that p. In other words, we get:

(2) p → ~Deap.

In the same way, substituting ~Deap for q in (1), we get:

(3) [Kap & (p → ~Deap) & Ra] → ◇Ka~Deap.

Unger also holds that:

(4) ~◇Ka~Deap.

Unger uses (4) as a premise in his argument for scepticism (see following step (14) ). Now we can get the following derivation.[6]

| | | |
|---|---|---|
| (5) | ~[Kap & (p → ~Deap) & Ra] | M.T.(3),(4) |
| (6) | ~Kap v ~[(p → ~Deap) & Ra] | DeM (5) |
| (7) | ~Kap v ~[Ra & (p → ~Deap)] | Com (6) |
| (8) | ~Kap v ~Ra v ~(p → ~Deap) | DeM (7) |
| (9) | ~(~Kap v ~Ra) → ~(p → ~Deap) | Imp. (8) |
| (10) | ~Kap v ~Ra | M.T.(9),(2) |
| (11) | Kap → ~Ra | Imp (10) |
| (12) | Ra → ~Kap | Trans (11). |

We get (11) and (12) from Unger's assumption of reasoning, and two other premises one of which is presented by Unger himself. As (11) and (12) show, Unger's assumption of reasoning leads to absurd conclusions, viz. (i) if one has knowledge then one is not rational and (ii) if one is rational then one does not know. It may be noticed here that (11) and (12) concern the relation between knowledge claims and rationality, and include no denial of knowledge claims. (11) and (12) each assert that knowledge and rationality are incompatible.

However, in what follows I shall state Unger's argument and evaluate it simultaneously. I shall also give a detailed examination of the second premise of the argument. The argument is the following.

First premise : if someone knows that p, then ... the person can or could know ... that there is no evil scientist ... deceiving him into falsely believing that p... .

Second premise : no one can or could know ... that there is no evil scientist ... deceiving him into falsely believing that p... .

The conclusion that follows by modus tollens from these two premises is: In respect of anything .., say, that p, no one ever knows that p.[7]

This argument may be formalized in the following way.

(13) $Kap \rightarrow \Diamond Ka\sim Deap$ - (first premise),

(14) $\sim\Diamond Ka\sim Deap$ - (second premise),

(15) $\sim Kap$ - (conclusion).

It may be noted here that if p, then it follows that one cannot be deceived by an evil scientist into falsely believing p; i.e. p → ~Deap. According to the assumption of reasoning, if a knows p and if a has a moderate amount of reasoning ability,then he can or could know that ~Deap. That is, on the assumption of reasoning we get, Kap → ◇Ka~Deap, which is (13), the first premise.

The derivation of (15) from (13) and (14) is valid; but there are several difficulties inherent in this argument. In the first place, the second premise of this reformulated argument is the same as that of his earlier argument (see above p.64) and is unacceptable for the same reason. One may attempt to make (14) acceptable by weakening it. The premise, ~◇Ka~Deap, is equivalent to □~Ka~Deap, and can be weakened by dropping the modal operator; we would then get:

(14') ~Ka~Deap.

Now if, in the above argument, (14) is replaced by (14'), the argument becomes invalid due to a modal fallacy.[8] Since the operator '◇' has been dropped from the second premise, it also has to be dropped from the first premise if validity is to be preserved. But the first premise is based on the assumption of reasoning, and hence the required change in the first premise cannot be done without prior change in the assumption of reasoning. It may be

observed that:

> The inclusion of the possibility operator in the consequent of (AR1) - though clearly sanctioned by Unger's statement that the rational knower "can or could know" that ~Deap - is obviously responsible for the error. What is wanted for the current sceptical argument is a closure principle on knowledge sets, and (AR1) does not give a genuine closure principle. Nonetheless, simply reformulating (AR1) by omitting the possibility operator is not much help ... [This reformulation] is plainly false: since it is not true that any finite rational knower will actually know all the consequences of his knowledge. While (AR1) was too weak to be useful, its revised version is too strong to be true.[9]

It is possible, however, to change the assumption in such a way that it would permit the operator '◇' to be dropped. Patrick Flynn proposes,for this purpose, what he calls the principle of rationality, a revised version of Unger's assumption:

> If there is a set of statements A, which 1) are known by some individual a, and 2) a knows that the statements of A lead in a proof by the strictest deductive inference to a certain conclusion p (ie.[sic] a can and does follow the proof without being interrupted or distracted), and 3) condition 1) and 2) occur in the same context; then a knows that p.[10]

This principle is formalized as: '$(x)(\varphi)\ (\psi)(Kx\psi\ \&\ Kx(\psi \vDash \varphi) . \rightarrow Kx\varphi)$ — where $Kx\psi$ and $Kx(\psi \vDash \varphi)$ occur in the same context'.[11] By instantiating a, p, q respectively for $x$, $\psi$ and $\varphi$, this principle can be stated as: if a (an individual)

knows a statement p and a knows that p logically implies a statement q, and actually carries out the derivation of q from p, then a knows q. The operator '◇' in the consequent is omitted by making the restriction that: a carries out the proof that q logically follows from p, and a's knowledge about p and a's knowledge about the proof that p implies q, occur in the same context. The restrictions make it possible to be sure about the consequent that a knows q; and the operator '◇' becomes unnecessary.

Following this formulation, Unger's argument can be reconstructed as:

(13') $Kap \rightarrow Ka{\sim}Deap$,

(14') $\sim Ka{\sim}Deap$,

(15) $\sim Kap$,

which is valid, and which gives Unger's intended conclusion.[12]

Our attempt to reconstruct Unger's argument in the above way does not help his case for scepticism. This can be shown by examining the second premise of this argument.

Even with the weakening of (14) to (14'), the second premise is still too strong. We would ordinarily claim to know that there was no evil scientist deceiving us into falsely believing that there were rocks. Hence the second premise is going to need some support if it is to be acceptable even in its weakened from. Unger attempts as much, but, as we shall see, his effort amounts merely to

redeployment of the old sceptical argument intended now to provide a necessary back-up to his main argument. The main argument becomes redundant since if the old argument on which it depends were any good, scepticism would be established without the help of the new argument.

Unger's second premise (14') can be criticized here by using Descartes' response to the evil-demon procedure. Descartes holds that it is not possible for one to doubt that one is doubting. In the same way one may argue that it is not possible to use electrodes to deceive someone into falsely believing that there are electrodes; and it will not be possible for an evil scientist to deceive someone into falsely believing that there is an evil scientist. Thus, the evil scientist supposition can do nothing to show that Unger's second premise, $\sim\underline{Ka}\sim Deap$, is true, at least in the case of propositions which pertain to the evil scientist supposition itself, e.g. where '$\underline{p}$' is '$(\exists x)(x=e)$'. Thus, Unger is not entitled to the full generalization of (14') that he wants.

We can thus present the structure of Unger's argument in the following way. We can use the evil scientist case (an exotic possibility) to rule out knowledge claims, by showing circumstances in which what is claimed to be known is false. But, of course, the evil scientist case cannot be used in this way to rule out knowledge claims

about the evil scientist; for, if the evil scientist deceives us then there is an evil scientist, and therefore it would be impossible to falsely believe that there is an evil scientist.

So, Unger's next move is to imagine a further exotic case which is capable of ruling out knowledge claims about the evil scientist. And thus we will get a whole range of exotic cases, each of which is capable to knocking out knowledge of any others.[13] An exhaustive range of such exotic cases, let us suppose, would be $c_1 \vee c_2 \vee c_3 \ldots c_n$ (one may suppose that there are only denumerably many such cases), where each such case entails the falsity of knowledge claims about all the others. Unger wants to deny that knowledge of anything is possible - thus he wants to deny that knowledge even of the exotic cases which allegedly undermine most other knowledge claims is even possible. Thus, to have shown that it remains possible to know about at least one exotic case is to block Unger's conclusion.

According to Unger, any of the exotic cases, for example, any $c_i$, will be capable of ruling out knowledge claims about any of the others - though no $c_i$ will be capable of ruling out knowledge claims about itself. Let us suppose that some $c_i$ holds, then $c_1 \vee c_2 \vee c_3 \ldots c_n$ is true - because no exotic case will rule out this situation.

Now, if one raises an objection against the second premise on the basis of the knowledge claim Ka~Deap, where 'p' asserts the exotic case that the evil scientist exists, then Unger will reply in the following way. Unger will hold that to have this knowledge claim, one must know that: 'he does not have, with respect to all external things, only randomly related experiences of such a nature that he falsely believes there to be such a scientist. But he can't know that'.[14] In other words, according to Unger, he cannot know whether or not his particular experiences are randomly related to external things; hence he does not know whether or not his experience is related to the evil scientist. This reply of Unger's is not acceptable, and this can be shown in the following way. We may ask: why is it not possible to know whether or not the experiences of a person are randomly related to external things? And we may ask: what sort of people is Unger talking about, who are not able to know whether or not their experiences are randomly related to the external things? From his discussion it is clear that the person under consideration is one whose 'brain is filled by nature from the first with drugs or chemicals which keep him continuously in error with regard to all sorts of external matters'.[15] Therefore, he will not know whether or not his experiences are randomly related to external things. Or there could be no problem

of drugs or of evil scientist, but 'all his experiences are only randomly related to any external things there may be'.[16] But it must be clear that this person is not the type of person we find in the assumption of reasoning. There the person has a moderate amount of reasoning ability, and the ability to apply that reason. Hence, the first premise of Unger's argument holds for moderately reasonable people. And the second premise holds for people either with a drug problem or with an inability to correctly connect their experiences with the experienced. Now we may inquire what sort of poeple the conclusion holds for. The argument now would be:

Premise : for moderately reasonable people, if someones knows p, then he knows q,

Premise : for people with problems of drugs or serious epistemic disability, one cannot know q;

Conclusion: therefore, no one knows p.

The conclusion does not follow from the premises; in fact nothing follows here logically. It may be said that the moderately reasonable people of the first premise are also either under the effect of drug or have serious epistemic disability, but they do not know this. But it would be inconsistent to think that moderately reasonable people can have a serious epistemic disability such that they always are in error.

Any further attempt to reconstruct the second premise and the argument as a whole also fails. If the argument is made valid by restricting the second premise over the range of reasonable people, then Unger must defend his second premise that moderately reasonable people cannot know whether or not their experiences are randomly related to external things. But in Unger's discussion there is no such defence.

From the above discussion it is evident that Unger's argument is not acceptable. This has been revealed from the attempt to see what sort of justification the second premise may have.

However, there is another sort of justification for the second premise, which says that we do not know that an evil scientist is not deceiving us. Unger holds that if we assert, contrary to the claim of the second premise, that we know that no evil scientist is deceiving us, then that would be dogmatic and irrational.[17] The reason it would be dogmatic is that it is always possible to be wrong in our knowledge claims. But the knowledge claim made in the face of the bare logical possibility (which is presumably the sense of 'possibility' Unger has in mind) of error need not be dogmatically made. On the other hand, Unger's argument from the possibility of error cannot yield denial of knowledge unless we have $\Diamond Map \rightarrow \sim p$, where 'Map'

reads '*a* is mistaken about *p*'. But this is a fallacy. Unger also holds that we would also be dogmatic if we claim to be *sure* and *certain* that no evil scientist is deceiving us. This latter view suggests that a knowledge involves certainty and certainty involves dogmatism. That is: *Kap* → *Cap* and *Cap* → □*p*, and by hypothetical syllogism, *Kap* → □*p*, where '*Cap*' reads '*a* is certain about *p*'. Both *Cap* → □*p* and *Kap* → □*p* are fallacies, and both make knowledge and certainty claims unduly strong. For there is no reason why a knowledge or certainty about a statement *p* should imply that *p* is necessarily true.

Unger presents these claims as 'intuitions [which] favour a sceptic's case'.[18] Along with these intuitions, Unger presents two hypotheses. First it is said that: 'an excessively severe attitude, or approach, or frame of mind, is entailed in one's being absolutely *certain* of something...'.[19] The second hypothesis is: 'If one *knows* that something is so, say, that *p*, then it follows that it is (perfectly) *all right* for one to be absolutely *certain* that *p*...'.[20] Like the intuitions, both these hypotheses are also wrong. This is clear from the following discussion on pages 101-04 and 84-86 respectively. But it may just be mentioned here that a certainty claim does not necessarily imply dogmatism, and that one can be certain without being dogmatic. Further discussion on this matter is given in

the following sections. It may be noticed that these intuitions and hypotheses are presented in defence of the second premise of Unger's argument. Since they are unsatisfactory, the premise remains unsupported.

In the above discussion we have seen Unger's claim that knowledge claims are always possibly mistaken. It may be argued that if we accept Unger's claim, even then the sceptical conclusion does not follow. For $\underline{Kap} \rightarrow \sim\underline{Map}$, and Unger's claim is that $\Diamond\underline{Map}$. An attempt to derive the sceptical conclusion $\sim\underline{Kap}$ will lead to a modal fallacy.[21]

Moreover, Unger presupposes that knowledge claims are always possibly mistaken. When Unger is supposed to show whether or not knowledge is possible, he actually presupposes that any knowledge claim could be mistaken. This circularity that Unger commits can be shown in a simple way without going to the details of Unger's argument. Before coming to the conclusion about whether or not anybody ever can know anything, Unger takes it for granted in the second premise of his argument that no one can ever know that there is no evil scientist deceiving him into falsely believing in something.[22]

There is one more issue which Unger considers in this context, namely whether knowledge about certain conditional statement is possible. It seems that his argument has done nothing to exclude such knowledge claims as: if

there is no evil scientist deceiving me about anything and my experiences are not randomly related to any external things there may be, then there are rocks.[23] To put this more generally, Unger has done nothing to show that the knowledge claims about the conditionals such as 'If I am not mistaken, p'are untenable. Unger thinks that if his classical sceptical argument is right, and if simple knowledge claims about external things like rocks are impossible, then knowledge of such exotic conditional cases is also impossible. He says that it is 'quite implausible that any of these exotic things *are* ever known if these simpler ones *can never* be'.[24] This reply of Unger's is also unacceptable. For while it may be difficult for us to know a simple case, it may be easier to know an 'exotic' one. For example, it may be difficult to know whether *p* is true; but we can easily claim to know that either *not-p* or *p* is true, which is equivalent to *p → p*. Both '*not-p or p*' and '*p → p*' can be exotic due to the exotic value of '*p*'. We can easily claim to know such an exotic conditional.

Unger further suggests that the two hypotheses, those quoted on page 75, together entail that to claim to know even the exotic cases would be dogmatic and irrational. Indeed it is suggested that sometimes it is not possible to be certain of such simple claims as to know that one is tired, or that one knows the capital of a state, and hence it would not be

possible to be certain about the 'exotic conditionals'.

It may be noticed here that these two hypotheses concern the simple cases, and Unger does not show how they are applicable to exotic cases. His transition from simple to exotic cases, it appears, is based on a misunderstanding regarding the relation between the simple case and the exotic conditional. The 'exotic conditionals' are weaker knowledge claims than the simple cases. And therefore it will be more difficult to be certain about simple cases than about 'exotic conditionals'. Unger's argument amounts to the claim that: since we cannot be certain about more difficult cases, it is also not possible to be certain about less difficult cases. This is absurd. However, in connection with the 'exotic conditionals', it is argued that:

> The attitude of certainty may be out of place ... in the matter of whether exotic conditionals are true: If I am not being deceived by a scientist and my experiences are not randomly related to external things, then there are rocks. Shouldn't certain experiences, like those already described, sometimes make one less certain of the truth of such propositions also? It certainly seems so. On our [above mentioned] hypotheses, then, one doesn't know them either.[25]

This argument cannot be convincing unless we are convinced about the hypotheses on which it is based. And we have seen above that the hypotheses are not satisfactory.

Unger's denial of the 'exotic conditional' can be examined in another way. The conditional may be formalized as:

(16) $\underline{Ka}(\sim \underline{Deap} \ \& \sim \underline{Re}^{\underline{a}}\underline{t}. \rightarrow .\underline{p})$, where '$\underline{Re}^{\underline{a}}\underline{t}$' reads '$\underline{a}$'s experience is randomly related to things'.[26]

According to most epistemic logics, $\underline{Ka}$ can be distributed across the conditional, and we would then get:

(17) $\underline{Ka}\sim\underline{Deap} \ \& \ \underline{Ka}\sim\underline{Re}^{\underline{a}}\underline{t}. \rightarrow .\underline{Kap}$.

This type of distribution can be made clearer by considering the following argument.[27]

(18) We know that if R is the rule for square root then 25 is the square root of 625.

(19) We know that R is the rule for square root.

(20) Therefore we know that 25 is the square root of 625.

To make the conclusion valid, one must distribute the epistemic operator 'know' across the conditional premise (18), and thus we must get that:

(18') If we know that R is the rule for square root then we know that 25 is the square root of 625.

We now get the valid argument (18')-(20). The model for this distribution is that for alethic modalitis:

(21) $\Box(\underline{p} \rightarrow \underline{q}) \rightarrow (\Box\underline{p} \rightarrow \Box\underline{q})$.

Let us now suppose that Unger is right in his

rejection of the above conditional knowledge (16). Since (17) follows from (16) by distribution of the epistemic operator, Unger is then also right in rejecting (17). Unger would then hold that:

(22) ~(Ka~Deap & Ka~$Re^a$t. → .Kap).

But (22) is true only when (Ka~Deap & Ka~$Re^a$t) is true and Kap is false. And this directly contradicts Unger's main position; he can never accept (Ka~Deap & Ka~$Re^a$t) as true, for that would admit two knowledge claims.(Ka~Deap & Ka~$Re^a$t) can be true only when both Ka~Deap and Ka~$Re^a$t are independently true. And this produces a contradiction in Unger's position. Moreover, to admit that Ka~Deap is true is to refute the second premise (14') of Unger's argument.

However, an objection may be raised here against my treatment of Unger's case. It may be said that the above criticism is based on the distribution if 'Ka' in (17), though such distribution is invalid. This can be shown in the following way.

It is argued that though (21) is a reasonable thesis of alethic modalities, yet it does not follow that it is a reasonable thesis for epistemic modalities - although most epistemic logics accept this thesis. In defence of this claim it is argued that: "in trying to prove a proposition 'q' a mathematician might find himself able to prove only that p → q, and many years later it may be that

he discovers a proof for '*p*' but nonetheless never comes to know that *q* since he never comes to connect his later proof with his earlier one".[28] And therefore, (17) does not follow from (16), and the criticism of Unger's position may not be acceptable. But otherwise and according to most epistemic logics, it may be said that Unger contradicts himself by admitting at least two knowledge claims.

To conclude this section we may note that Unger turns to a classical sceptical argument for the defence of the second premise of his new argument. But his defence of this premise has entirely failed, and this amounts to the failure of both the classical sceptical argument, as well as Unger's new argument for scepticism. However, we may now explain and examine Unger's next major argument for scepticism.

## §2.2: An Argument for Universal Ignorance.

It is mainly by this argument that Unger advocates his extreme view that we can never know anything, that we are completely ignorant. The argument is opened on pages 87ff of his book, a preliminary statement of it is given on pages 95ff, and it is further developed on the subsequent pages. The argument is the following.

First premise : 'If someone knows something to be so, then it is all right for the person to be absolutely certain that it is so'.

Second premise : 'It is never all right for anyone to be absolutely certain that anything is so'.

Conclusion : 'Nobody ever knows that anything is so'.[29]

This argument for "universal ignorance" is unaccptable; this can be made clear by showing that one of the premises of this argument is unsupported. I shall discuss the second premise and analyse how Unger "establishes" this premise by another unsound argument.

Before stating the second premise in the present form, Unger first states it in the following way: 'In the case of every human being, there is at most hardly anything of which he is certain'.[30] The difference between these two statements, which will be discussed later, is that in one case the premise is normative and in the other it is not. The non-normative version of the premise, moreover, is almost identical with the statement: 'in the case of each human being, there is at most hardly anything of which he really is certain'.[31] In fact, when I clear up the gap between the two (normative and non-normative) presentations,[32] it will be clear that Unger "establishes" the second premise of his argument for "universal ignorance" on pages 67-68 of

his book. Unger's attempt to establish it is the following:

> As a matter of logical necessity, if someone is certain of something then there never is anything of which he or anyone else is more certain ... . Thus, if it is logically possible that there is something of which any person might be more certain than he now is of a given thing, then he is not actually certain of that given thing.
>
> Owing to these observations ... I think, that hardly anyone, if anyone at all, is certain... .33

This argument can charitably be reconstructed as follows:

(1) Let us suppose, someone, namely a, is certain about something namely p,

(2) then he or anyone else cannot be more certain about anything else.

(3) But, it is logically possible that anyone, namely b, might be more certain about something else, q,

(4) therefore, a is not certain about p.

Thus Unger draws the conclusion that certainty claims by anyone about anything can be shown to be wrong.

But this argument is clearly unacceptable. What has gone wrong is that in the premises (1) and (2), the term 'certain' has been used by Unger in his own sense of absolute certainty. Hence, it is said in (2), that no one can be more certain about anything than a is about p. But in (3), the term 'certain' has not been used in Unger's own

sense; therefore, Unger can say that it is logically possible for someone to be more certain than a is about p. But as Unger says, if 'certainty' means 'absolute certainty', then there cannot be anything more certain. Hence, according to Unger's own standard, (3) is false. Alternately if we adopt the sense of certainty used in (3) then, by that standard, (2) is false. Either way Unger is deriving the conclusion by means of a false premise. Thus Unger's argument is unsound; and his claim that no one can ever be certain about anything, is unsupported.[34]

This criticism is appropriate to the constative version of the second premise of the argument for universal ignorance. Let us now try to fill in the gap between the normative and non-normative versions.

A prototype of the normative version of the first premise of this argument is first presented in connection with the Cartesian argument.[35] There it is presented as a hypothesis. In commencing the present argument, Unger explains why he intends to adopt the normative version; he thinks that by 'placing ourselves beyond whatever controversy normative matters may involve, [i.e. by not adopting the normative version] we have lost out on three things',[36] namely:

> First, we have not argued for any sceptical conclusion as a necessary truth. ... Second, as the normative requirements intuitively seem the most difficult to satisfy, we might

> expect to be able to increase the scope of our scepticism by using an argument which focuses on them. Hence, we might expect thus to be able to argue that no one knows anything to be so, not even that he himself exists or that one and one are two. And, we might also expect to argue, on normative grounds, that no being, not even a God, if there is one, knows even that he himself exists. And, third, beyond the necessity and the greater scope afforded by a normative argument, the intuitively felt difficulty of knowing's normative requirements bodes well for the compelling power of an argument with them in focus.[37]

These three things he hopes to gain by the transition to the normative version of the argument. This is why he attempts the transition; but the more important question is about whether and by which logical route he can do this.

On the normative version of the argument, Unger says:

> I think that their [i.e. of the premises in normative version] necessary truth derives, at least in part, from this [normative] characteristic. We may look at these propositions to help make the point: If someone promises to do something, then it is at least all right for him to do that thing, providing that no overriding (consideration or) considerations make(s) it not all right.[38]

It is now easy to see how Unger actually derives the normative version. On this schema, he can now hold, to put it grossly, that:

> if someone can never be certain then it is never all right to feel certain.

And he claims to have established that:

one can never be certain about anything.

Hence, by modus ponens he derives that:

it is never all right to feel certain.

Now, the second premise of this derivation i.e. that 'one can never be certain about anything', as we have seen above, has been supported with an unsound argument. Hence, again, the transition to the normative version of the premise is unsound.

The revealed difficulties of the second premise are sufficient to render the argument unsound. However, we may raise here another question; we may inquire what we may mean by 'certainty', and that may help to restrict Unger's rejection of absolute certainty. And so far we have only seen that Unger's arguments for the second premise (of the argument for universal ignorance) are unsound, and the premise remains unsupported. But we have not seen whether or not this premise is actually false. If we can show that absolute certainty can actually be obtained, then this premise will turn out to be false. From this standpoint, an attempt will be made in the following section to analyse the notion of certainty. And this may be considered as a turning point for some positive aspect of our discussion.

§2.3: On Certainty.

Two broad issues in the problem of certainty are of central importance to us: (i) we can discuss what the things are that we can legitimately claim to be certain or uncertain about; (ii) and we can discuss how we can claim to be certain or how we can deny such certainty claims. The first issue is, as will be clear from our discussion, relatively easier to settle, and hence we may discuss it first.

Let us take a few examples of statements about which we may claim to be certain or uncertain. These are the following.

(1) Two and two make four.

(2) A bachelor is an unmarried man.

(3) I have two hands.

These three statements represent two types of claims about which we may claim to be certain or uncertain. In my discussion I shall take these, and other such claims, as statements which can be believed by someone. Another characteristic of such claims is that some of them may be held atemporally, some omnitemporally and some for only a limited period. On the other hand there are other claims which can be held either omnitemporally (I may be sure that I always have had and always will have two hands) or temporally (I may be sure only that I have two hands).

Beliefs or statements like those represented above by examples (1)-(2) are characterized as analytical and thus atemporal. One cannot deny them without contradiction in terms. But there will not be any contradiction in denying that men have two hands. Such statements are based on contingent facts and our experience..

Of these two types, analytical and empirical, there is not much controversy about the certainty of analytical statements. A hint to the nature of certainty of such statements has been given just above while considering the example that two and two make four. The philosophers interested in the question of certainty have been mainly occupied with controversies regarding the certainty of empirical statements. I shall confine myself to discussing the certainty of empirical statements.[39]

How can one be certain about such statements? One can always claim to be certain about such statements on the basis of one's experience and evidence. Against such claims one can, however, raise objection on the basis of the evil-scientist argument.In view of the detailed criticism of this argument, that we have seen earlier in this chapter, we may set aside this argument as quite ineffective. But even if we can be free from the deception of the evil scientist, we are not free from another risk. It may be said that whether or not some other being is deceiving us,

our experience can be illusory, or there can be error in what we experience. It may further be said that many of our certainty claims may just be false due to some error in our evidence. And moreover experience and evidence may not be conclusive. In reply we may say that we can avoid an illusion or error by careful examination, and that the result of such examination can be conclusive enough to be satisfactory. Let us take an example.

I may claim that the colour of my rain coat is bottle-green. It is possible that I am a colour-blind person, and my experience of colour may not be correct. Or it may be the case that I have seen the colour bottle-green when it is actually blue. But I can take the coat to adequate light to examine it again, perhaps in day light, for artificial light may not be appropriate.[40] I can also show the coat to several other people and learn their views about its colour. I can even get my eyes examined by a qualified physician. I shall call this sort of examination of the colour of the rain coat an example of careful examination. If we find the coat bottle-green after this careful examination, can we then claim to be certain that its colour is bottle-green?

It may be said that it is logically possible that there can be error even in a careful examination. If so, we may again carefully examine our previous careful

examination. This second careful examination may again be said to be subject to the possibility of error. If so, we will require another careful examination; if objections go on like this, we may require a careful examination of the preceding careful examination ad infinitum. But if this infinite regress is considered a necessary condition for the certainty of careful examination and thereby for the certainty of the statement, then it would not be what we usually mean a careful examination to be. And if we reject the result of the above careful examination, because it is not possible to do an infinite number of examinations, then this rejection will be a misuse of the usual import of the phrase 'careful examination', of the term 'certainty' and of the statement 'my rain coat is bottle-green'. To put this in another way, the possibility of an infinite regress of careful examinations is not sufficient for rejecting the notion of careful examination, though this possibility may leave it open that our careful examination may have produced a false result. But this is only a logical possibility. Following Norman Malcolm,[41] we may say: to say that it is possible that our careful examination will turn out to be mistaken is to mean that it is not self-contradictory that a careful examination will be mistaken. Thus the possibility of an infinite regress of careful examinations and the possibility of its rejection do not

imply that we have any ground for its rejection - they only imply that it is not self-contradictory to accept certainty on the basis of careful examination and to consider that there may be grounds for its being mistaken.

I shall state here an argument given by O.K.Bouwsma against Descartes' evil demon. Bouwsma's argument not only weakens the case for the evil demon, but also has a similarity to my account of careful examination.

We know that if there is an evil demon, then he deceives all men, and they can never understand this. And since they cannot understand that they may be in illusion, they have a false belief about their false belief. First, they have the false belief about a thing which they perceive and think as real; secondly, they have the false belief that what they believe is true. They have an illusion about a perceptual object; considering the illusory belief as real, they are involved in a second illusion: they are in illusion about illusion. And thus people are in an infinite regress of illusions. On the other hand, the evil demon who creates all these illusions is not himself in illution. When people always have sense perception of illusions, their sense perception is of a different kind to the sense perception of the evil demon. Bouwsma suggests[42] that what the evil demon considers to be illusion according to his sense, cannot be illusion according to a different

but human sense. Bouwsma says: '[the evil genius] has certainly created his own illusions, though he has not himself been deceived. But neither has anyone else been deceived. For human beings do not use the word "illusion" by relation to a sense with which only the evil genius is blessed'.[43] The evil genius cannot create a universal illusion without having a special unique sense of 'illusion'. But unless the sceptic shows that the evil genius can create his illusion in the human sense, the sceptical argument remains unconvincing.

This argument is similar to my position regarding careful examination, viz. that it is a misuse of the usual notion of careful examination if it is held that careful examination must involve an infinite number of examinations in order to yield certainty; and hence the sceptic's notion of certainty is different from the usual one.

In this connection I may further note the following two points.[44] First, to demand an infinite number of careful examinations for a certainty claim is not to be extra-cautious, but to be absurd.

Secondly, the demand for the satisfaction of an infinite regress relies on the assumption that: one is certain only if one is certain that one is certain, and so on.[45] According to this assumption, it follows that,

(1) $\underline{Cap} \rightarrow \underline{CaCap}$,

and (2) $\underline{CaCap} \rightarrow \underline{CaCaCap}$,

and so on *ad infinitum*. And therefore we get that,

(3) $\underline{Cap} \rightarrow \underline{CaCa \ldots Cap}$.

On the other hand we must presumably hold that,

(4) $\underline{CaCa \ldots Cap} \rightarrow \underline{Cap}$.

But (3) and (4) yield, by conjunction,

(5) $\underline{Cap} \equiv \underline{CaCa \ldots Cap}$.

In other words, the added certainties or the infinite regress does not make any difference in the initial certainty. This can be proven as follows if we assign numerical values to degrees of certainty very much like degrees of probability. In probabilistic semantics we have,

(6) If $\underline{p} \rightarrow \underline{q}$ then $Pr(\underline{p}) \leq Pr(\underline{q})$; or $/\underline{p}/ \leq /\underline{q}/$ for short.

Presumably this result transfers to certainty. Since we must have,

(7) $\underline{CaCap} \rightarrow \underline{Cap}$,

we get,

(8) $/\underline{CaCap}/ \leq /\underline{Cap}/$.

If now we add (what Unger wants),

(U) $\underline{Cap} \rightarrow \underline{CaCap}$,

then we get,

(9) $/\underline{Cap}/ \leq /\underline{CaCap}/$.

(8) and (9) give,

(10) $/\underline{Cap}/ = /\underline{CaCap}/$,

and similarly for further iterations of certainty operators. In other words, with (U), being certain that we are certain gives no more certainty than just being certain.

It may be said that since the initial certainty is equivalent to the last certainty, the initial certainty cannot be obtained until we get the last one which we never get because the regress is endless. But this is not acceptable, for, as we have argued, it would be a misuse of the usual notion of certainty to make it dependent on the satisfaction of an infinite regress. Moreover, since the initial certainty is identical to the last one, then (even if one maintains that the last certainty is necessary condition for the initial one) it does not follow that we have to establish the last certainty in order to establish the initial one. We, however, owe the sceptic a supporting argument here. Since we claim that we can obtain an initial certainty without undertaking an infinite regress of careful examinations, we remain open to the possibility that we might have discovered a mistake in our initial certainty if we had pursued the infinite regress. We will see later in this section (see p. 104) that even granting this much to the sceptic, we can still obtain absolute certainty.

It should now be clear that the objections against the possibility of certainty, on the basis of the

traditionally raised limitation of experience and evidence, are not convincing. And thus it should be clear that it is possible to be certain about some of our empirical statements. This possibility of certainty may further be pursued here.

In the above discussion of the careful examination of the colour of the rain coat, we have seen that we can be certain about its colour. But someone who has not yet seen the coat may accept our view and may feel certain about the colour of the coat. But in fact this would be a certainty about the reported statement he has heard from us. If he is certain that we can be trusted, then he may accept with certainty that the colour of the rain coat is bottle-green. And there may be many circumstances where absolute certainty may be held without direct experience of the facts and events concerned. For example, it is not possible to experience the eighteenth day of May of 1872, and the fact that Bertrand Russell was born on that day. But we can be certain with complete absence of doubt, i.e. with absolute certainty, that Russell was born on that day.

But a person may have such a sceptical bent of mind that he may not accept any empirical statement with certainty unless he himself examines it. Moreover, he may not know us and the question of believing us may not arise. But such a person would admit that the results of careful

examination will be certain at least for those who have been involved with the examination. Similarly, one may be certain about one thing but may not be certain about something else. Though one may be certain about things like the colour of the rain coat, yet one may not be certain about the past, about other minds, etc. But in spite of these problems, we may say that, it is possible for some people to be certain about something and that something may be certain for some people but not certain for others. In the case of the statement of the colour of the rain coat, it is certain for us who have examined its colour, that it is bottle-green; but this may not be certain for others who have not examined the case.

We may also see that though we can be certain about an empirical statement at one time, yet we cannot claim that we will remain certain of the statement for all future time. Thus for example, after exposure of the rain coat to sun light and showers for a long time, its colour may get changed. If it is made of poor material, then its colour may get washed out. Hence, though we have carefully examined the colour of the rain coat and are certain of its colour, yet our certainty may not remain intact for ever. Hence it follows that certainty of empirical statements depends on context and on time, but it actually can be obtained by someone at some time, in the case of some empirical

statements.

A sceptic, at this point, may raise the question whether or not such certainty is absolute, and may hold that by 'certainty' we must mean only 'absolute certainty'. Unger, for example, holds that " 'certain' is an absolute term" and that "presence of certainty amounts to the complete absence of doubt".[46] If so, can we obtain certainty in the absolute sense? Those who have carefully examined the coat, can claim with complete absence of doubt (on the basis of their careful examination) that the colour of the rain coat, now, is bottle-green. In the same way we can also claim with complete absence of doubt that this piece of paper is white, that that umbrella is black, and so on. Thus we can be, and actually are, absolutely certain at present that the colour of the rain coat is bottle-green, and that this piece of paper is white. But another question may be raised here: is it possible for anyone to be more certain about any other statement than these two statements? The answer is, no. When one claim is absolutely certain (in Unger's sense), there cannot be any other claim which can be more certain. To put it in another way, when two claims are absolutely certain, there is complete absence of doubt about both, and nothing can be more doubtfree than that. There may be more than one such absolutely certain claim, but all these will be absolutely certain,

and no one will be more so than any other. This also shows why Unger is wrong and inconsistent if he claims that some statement more certain than absolutely certain statement is logically possible.

It is thus clear that if someone is absolutely certain about something, no one can ever be more certain about that thing or about anything else. If one is absolutely certain about p at present, no one can ever be more certain about p, or about anything else. This, however, does not suggest that absolute certainty is eternal certainty. In the above discussion we have made such certainty claims only in the context of a time. If at a later time doubt arises in the previously claimed certainty of something, say p, then there will be no certainty about p at this later time. Let us suppose that at this later time we are certain about q. Let us also suppose that this later time is $t_2$ and the previous time when p was certain is $t_1$. Previously p was certain at $t_1$, and q is now certain at $t_2$; and both cases are absolutely certain, and the question of which has the greater certainty does not arise here. In both cases, there is complete absence of doubt. But again the question of more certainty does not arise between p at $t_2$ and q at $t_2$. Because, at $t_2$, p is uncertain and q is certain. This shows that in the above explained sense, absolute certainty can be reversed but cannot be exceeded.

We should make here a distinction between two ways in which certainty claims may change. We have seen above that our certainty that the colour of the rain coat is now bottle-green, and that this certainty may change due to the change in the colour of the rain coat. This is one way in which certainty claims may change: due to changes in the things about which we are certain. There may be a different way in which certainty claims may change: due to changes in our evidence for our certainty claims. Epistemologically, this second way in which certainty may change is more significant. We may make mistakes in our careful examination, our evidence may fail to support our claim, or may change due to further examination, and our certainty claim itself may therefore get reversed. This shows that the temporal absolute certainty may change; and the admission of this fact will save us from the charge of dogmatism when we claim to be absolutely certain about a thing at a particular time. We shall further discuss dogmatism shortly hereafter.

In the above discussion we have seen that absolute certainty is the complete absence of doubt. On the other hand, the evidence on which such complete absence of doubt and the conviction are based, make the objective aspect of absolute certainty. Of course, one may have conviction and complete absence of doubt without any objective ground

but that will be dogmatic. In the subjective sense one can feel absolutely certain; in the objective sense one can justifiably claim to be absolutely certain. It should be noted here that the absolute certainty which I have discussed above in the sense of complete absence of doubt is based on careful examination and the evidence which we get from there. Thus the absolute certainty which we accept in the above has always a dual aspect: (i) careful examination and the obtained evidence, and (ii) complete absence of doubt and the corresponding conviction.

It should now be clear how absolute certainty cannot be more certain; this is in the negative sense of complete absence of doubt, the criterion which Unger adopts, that there cannot be more certainty than absolute certainty. But the objective aspect of absolute certainty (in the temporal and reversible sense), which consists in evidence, can increase.

Given the above account of absolute certainty, the worries of some philosophers,[47] including Unger, about more certainty of a claim at a later time is understandable. And we can now better understand Unger's argument (see above page 83) against absolute certainty. When he talks about the possibility of more certainty than absolute certainty, he might have had in mind the objective sense in terms of evidence. But he himself defines certainty in the

negative sense of complete absence of doubt. And now this is clearer how Unger uses (see above pp.83-84) two different senses of certainty.[48]

An objection to this view of absolute certainty, however, has been raised by some authors. It is said that we cannot accept a view as absolutely certain, because, such certainty claims will be dogmatic. Unger raises this objection as one ground for rejecting absolute certainty.[49] It has, therefore, to be shown that it is possible to hold a belief with absolute certainty without being dogmatic.

We have seen that we can be certain about an empirical statement regarding the colour of a rain coat, but only at a particular time. The colour of the coat may get changed, and though we are certain about it at present, we may not remain so in future. On the other hand, as we have admitted, the absolute certainty about the colour of the rain coat may get reversed (irrespective of whether or not the colour changes) due to some mistake in the evidence on which the certainty claim is based. Thus we see that certainty claims are reversible. When we are absolutely certain about an empirical statement, according to this account, we are still open to the possibility of its future rejection. We are so certain only due to the complete absence of doubt. We can remain absolutely certain until

counter-evidence is found. This absolute certainty or the complete absence of doubt is only a contextual and temporal notion. This notion is quite different from the absolute certainty which will be claimed as time-independent and irreversible and thereby independent of any evidence that may later be produced. One may make an absolutely certain claim that such and such is the case, and may further claim that this certainty will be independent of whatever evidence may occur later, and will be irreversible. It will be dogmatic to hold such a rigid claim, and we do not hold such a view.

This view that it is possible to obtain absolute certainty without being dogmatic, has been clearly established by Douglas Odegard. We may refer to his discussion of the dispute between Barry Stroud and Unger. About Unger's book, Stroud says that he is absolutely certain that he has read the book. He says: 'I simply couldn't be wrong about it; I have been reading it, thinking about it, and writing all over it for the past several weeks. And I am equally certain that nothing can refute the claim that I have read it'.[50] It is <u>true</u> that Stroud has <u>indeed</u> read Unger's book; but at the same time Stroud's attitude is severe, and it will lead him to dogmatism, unless he makes here a distinction. Stroud fails to make this distinction, and consequently he in fact strengthens Unger's view

that unless one is sceptic one must be dogmatic. The required distinction has been made by Odegard:

> To avoid being dogmatic, Stroud must restrict what he is certain of to his having read the book, i.e. to the truth of the belief that he has read it, and not extend it to the absence of good counter-evidence. Thus, if being certain that nothing will 'refute' the belief is being certain that no good evidence will count against the belief, this must be separated from being certain that the belief is true. Such a separation is in order, since being certain of the belief's truth is legitimate as long as there tenselessly <u>is</u> no good counter-evidence, whereas being certain of the absence of good counter-evidence is legitimate only if such counter-evidence is <u>impossible</u>.[51]

It is made clear here that we can be absolutely certain about a claim that such and such is the case, though we may not be absolutely certain that 'counter-evidence is <u>impossible</u>'; a certainty claim to the impossibility of counter-evidence may make a claim irreversible and dogmatic. Only this second type of absolute certainty will be dogmatic.

It thus follows that we can be absolutely certain without being either dogmatic or sceptical; we will not be sceptical because we can obtain absolute certainty. And thus there is a middle ground between dogmatism and scepticism. And therefore, Unger's position that unless we are sceptical we will be dogmatic, and the implied dichotomy of scepticism and dogmatism, are both false.

It may be noted here that by making the distinction between 'there is no good counter-evidence' and 'good counter-evidence is impossible', Odegard is providing the middle ground between scepticism and dogmatism, and at the same time granting the sceptic some recognition. For, now there is a third position which is closer to scepticism than its anti-thesis, viz. dogmatism. Since it is now admitted that counter-evidence is not impossible, a sceptic can depend on this point to support his claim to some extent. According to the first part of the distinction we can be absolutely certain about something, if 'there is no good counter-evidence'; but this absolute certainty may disappear and the sceptic has a reason to argue for his case, for according to the second part of the distinction, it is not the case that 'good counter-evidence is impossible'. In fact, in the vein of Odegard's view, we may say that we must grant this much to the sceptic. While on the other hand, in contrast to the sceptic's claim, we can be absolutely certain in the temporal sense, about many things; but we must save ourselves from dogmatism by agreeing that it is not impossible that this absolute certainty could be reversed. But until any counter-evidence is found, we can hold that we can remain absolutely certain.

We may thus hold that our discussion establishes that in some cases some people can actually be certain

about many things; and thus that the sceptic's claim that we can never be certain about anything, is wrong. My main purpose here has been to defend the last point and to restrict any unrestricted sceptical rejection of certainty claims.

## §2.4: Unger on Irrationality.

The next important task in the consideration of Unger's scepticism is to deal with his irrationalism. From his account of the classical form of scepticism, he passes on to his argument for universal ignorance; from this he passes to his view about human irrationality. Meanwhile he thinks that he has actually succeeded in establishing universal ignorance. He then holds that: 'my main objective in the present chapter, as well as in much of the rest of this work, is to examine what will follow in so far as ignorance does indeed prevail'.[52]

Since Unger's ignorance-thesis is unacceptable, all these undertakings could be set aside. But for completeness' sake we should give some attention to this discussion.

Unger argues, not only that we are unable to know anything, but also that we can never have any reason for any of our beliefs. Such denial of the possibility of rational belief was first advocated in connection with

Cartesian scepticism. It will not be inappropriate to look back at this earlier version of the defence of irrationality.

It is said that: 'When one despairs of ever knowing about certain things, then in so far as one believes things about those things, it is quite natural for one to aspire to be reasonable in one's beliefs about them'.[53] Is such reasonableness or rationality acceptable? Unger pursues a negative answer with the following argument.

> The simplest argument here is this: 'If I can't know anything concerning any external world there may be, then how can I have any reason at all for believing anything about any such world? It really seems that without this knowledge I can't. And, as we have already concluded, I can't know anything about any external world. Therefore, it seems, I can't really have any reason for believing any such thing either'.[54]

This is an argument of the modus ponens type. And again there is a problem with the second premise, namely, that I can't know anything. We have already seen that this view is not acceptable. However, whether or not this premise is true, the argument is unacceptable.The conditional premise here says that: If I can't know anything, then I can't have any reason for believing in anything. This claim is false. Let us consider two alternatives p, and ~p, and various ranges of evidence for or against either of them, viz. $\{e_1 \ldots e_n\}$, $\{e'_1 \ldots e'_n\}$, $\{e''_1 \ldots e''_n\}$, etc. Then we can,

in appropriate circumstances, say that it is more rational to believe p (or ~p) than ~p (or p) if a certain set of evidence turns up - without knowing either that p or that ~p, or knowing any of the evidence.

The latter argument for advocating irrationality is quite a different argument. This argument holds for a person S, and for any propositional value of 'p', where 'p' is about the external world. The argument is the following.

First premise : '... if there is a reason r for someone S to believe that p, then it is possible for S to know that r... .'

Second premise : '... it is never the case that it is possible for S to know that r... .'

Conclusion : '...[therefore] there is never any reason r for anyone S to believe that p... .'[55]

From this conclusion Unger presents the following instance for sceptical attention.

> 'For any propositional value of 'p' which concerns any external world there may be, no one ever is (at all) reasonable in believing that p'.[56]

Both the premises of this argument have difficulties. The second premise is a stronger version of the conclusion of the (unsound) argument for universal ignorance. The second premise is: '... it is never the case that it is possible for S to know that r'; and it is a stronger version of

'Nobody ever knows that anything is so', which is the conclusion of the argument for universal ignorance. In our analysis we have seen that it is wrong to claim that, 'Nobody ever knows that anything is so'; the import of our analysis was to suggest that this claim is unduly strong, and thus that a stronger version of this claim will be even less acceptable. From these observations, we can hold that the second premise of the present argument for irrationality again remains unsupported.

On the other hand, the first premise of the argument can be traced back, via. Unger's 'Principle of the Possibility of Identifying Knowledge', to his 'Basis Argument'; and the basis argument again contains an objectionable premise.

The first premise of the argument for irrationality is the result of application of the principle of identifying knowledge. Unger says that: 'The first premise is our principle itself, [i.e. the principle of the possibility of identifying knowledge] here, for convenience, confined to the topic of believing'.[57] I shall, therefore, first explain this principle; since this principle, as we will see, is based on Unger's basis argument, I shall then explain the latter. And then I shall return to the first premise of the argument for irrationality.

Unger's principle for identifying knowledge is: 'If

there is something r which is a reason for someone S to X, then it must be possible for S to know that r is a reason for him S, to X'.[58] This is a false principle, amounting to the claim that if I do not know a reason then I cannot hold it. But we do not have to know a reason for holding it. However, Unger says that: 'This principle entails, I suggest, that it must be possible for S to know that r (or, that j). This last condition is nothing new for us, as our Basis Argument requires S actually to know that r and, so, a fortiori, that it be possible for S to do so'.[59]

Unger suggests that what is entailed by the principle of identifying knowledge is required by the basis argument. And we will see that the third premise of the basis argument is a weaker version of this principle. Hence the satisfactoriness of the basis argument depends on the satisfactoriness of this principle. I shall now examine the basis argument.

The conclusion of the basis argument is deduced from three premises. The argument is the following.

First premise : 'If someone S is ... reasonable in something X, then there is something which is S's reason for X...'.

Second premise : 'If there is something which is S's reason for something X, then there is some propositional value of 'p' such that S's reason is that p...'.

Third premise : 'If S's reason (for something X) is that p, then S knows that p...'.

Conclusion: 'If someone S is ... reasonable in something X, then there is some propositional value of 'p' such that S knows that p'.[60]

It may be argued that the second premise of this argument is entirely otiose, since it merely amounts to the claim that reasons can be propositionally formulated which, in fact, is a precondition of being able to formulate the argument.[61]

The third premise of this argument is stronger than it should be. In favour of this premise Unger argues that, 'if Ralph's reason (for running to the store) is that the store will close in twenty minutes, then Ralph knows that it will close in twenty minutes'.[62] This may not be the case. If Ralph is asked why he is running to the store, he may give one of a number of answers: Ralph may say that he is running because he knows that the store will close in twenty minutes; or he may say that he believes that the store will close in twenty minutes; or he may say that he has been told as much by someone. Now, if Ralph acts on what he believes, even then he can be reasonable. This shows that Ralph does not have to be absolutely certain in holding a reason for something. And thus Unger is once more mistaken when he says, in defence of the third premise, that "It is inconsistent to say 'Ralph's reason was that Fred's hat was wet, but he wasn't absolutely certain that

it was' ".[63] To take another example, a scientist may act on what he believes or on what he hypothesises and not on what he knows; but he may be quite reasonable in doing this. Unger's claim that, to be reasonable one must know the propositional value of the reason, is unduly strong.

It may now be clear that, since the third premise of the basis argument is unsatisfactory, both the basis argument and the principle of identifying reason are unsatisfactory. And the first premise of the argument for irrationality is the same as the principle of identifying reason. Hence the first premise of the argument for irrationality is also unsatisfactory, and the argument remains unconvincing.

On the other hand, Unger's thesis of ignorance and irrationality is not acceptable for another reason. Since Unger thinks that ignorance and irrationality prevail universally, presumably, Unger himself is not free from ignorance and irrationality. This possibility is strengthened by the observation that Unger does not want to exclude even God from this extreme scepticism.[64] And if so, what is the nature of his defence of irrationalism? In the first place, he cannot know anything, and therefore, he cannot even be reasonable in believing in his sceptical conclusion. Thus Unger holds his view without any reason; if so, Unger's position turns out to be similar to dogmatism,

where a claim can be held without any reason. If Unger's proclaimed ignorance and irrationality would not be universal, if some being, maybe God, could be free from such scepticism, then there could be some hope that at least Unger himself is above ignorance and irrationality. Unger does not keep open any such possibility even for himself.

Unger admits that if the evil scientist deceives everyone, then no one knows anything; but it is not denied that the evil scientist himself is not deceived and thus can perfectly know everything. The evil scientist is thus making a provision for his not being subject to the condition to which all others belong. When Unger claims that no one knows, and no one can reasonably believe anything, he is not making any such provision, even not for the legitimacy of his own views.[65] Consequently, when he holds any of his beliefs, he is not only irrational, but also dogmatic.

Unger claims that dogmatism is a severe attitude of mind such that after a certain claim is made, no further counter-evidence will count against that claim.[66] Unger's irrationalism denies the possibility of any reasoning, argument or counter-evidence; hence his irrationalism is so severe that no further counter-evidence will count against it. Thus Unger's irrationalism again turns out to be similar to dogmatism. Consequently, Unger's position is not acceptable.

## §2.5: Conclusion.

I may conclude this chapter with the observation that the case for scepticism, as presented by Unger, fails to make itself convincing. The reformulation of the classical form of sceptical argument is not any more effective than its original; the alleged universal ignorance has no real basis, and there is no reason to think that we are all irrational in what we believe. On the other hand, we can be absolutely certain about many things, without committing ourselves to dogmatism. With these results we may now pass on to the next chapter.

## Notes: Chapter 2

[1]Unger, P., Ignorance: A Case for Scepticism (Oxford: Clarendon Press, 1975), pp. 7-8. Much of the material of Ignorance has appeared before in different papers of Unger. Considering Ignorance as more recent, comprehensive and fully developed form of his scepticism, I shall confine my discussion on this work only. Page references to some of the papers will however be given in some of the following notes.

[2]The distinction between Unger's argument and the Cartesian one is not, however, always correctly recognized. Cf. for example, B. Williams, Descartes: The Project of Pure Enquiry (Sussex: The Harvester Press, 1978), p.56n. For detailed discussion of the distinction, see M. Smith, "Unger's Neo-Classical Scepticism", Mind 90 (1981), pp. 270-73.

[3]Unger, P., Ignorance, p. 15.

[4]Adopted from N. Griffin and M. Harton, "Sceptical Arguments", Philosophical Quarterly 31 (1981), p.28.

[5]Cf., ibid., p. 27.

[6]This derivation is based on a similar derivation constructed by Dr. N. Griffin (private communication). The abbreviation of the inference rules have been adopted from I.M.Copi, Symbolic Logic (New York: Macmillan,1968), ch. 3.

[7]Unger, P., Ignorance, pp. 20-21.

[8]For an account of different modal fallacies which are committed by sceptical arguments, see N.Griffin and M. Harton, "Sceptical Arguments".

[9]Ibid., p. 28. (AR1) stands for the formulation of the assumption of reasoning given above.

[10]Flynn, P., Agniology Revisited (M.A. Thesis, McMaster University, 1978), p. 16, my italics.

[11]Ibid., p.22, my italics; I have adopted '→' instead of Flynn's use of '⊃'. Also cf., R.A.Eberle, "A Logic of Believing, Knowing and Inferring", Synthese 26 (1974), pp. 356-82. Eberle's epistemic inference connective 'I' can be adopted to yield a more elegant form of the principle.

[12]It may be mentioned here that Peter Klein, in discussing different versions of the evil-demon argument, presents a general formulation of the structure of this argument (Certainty: A Refutation of Scepticism,(Minneapolis: University of Minnesota Press, 1981),p.82, my italics; for the reading of '$H_c$', see p. 25). This is the following.

$$Jsp \rightarrow Js{\sim}H_c$$

$$\sim Js{\sim}H_c$$

$$\therefore \quad \sim Jsp,$$

where '$s$' ranges over epistemic subjects, '$Js$' reads '$s$ is justified', '$p$' stands for any empirical proposition and '$H_c$' reads '$e$ & ~$p$ & there is some mechanism, $M$, which brings it about that $s$ believes (falsely) that $p$', and '$e$' is the evidence for $p$.

It is obvious that the structure of our reconstruction of Unger's argument i.e. that of the argument (13')-(14')-(15) and the structure of Klein's formulation are similar. We will see in the following discussion that the reconstructed version of Unger's argument, though valid, is unacceptable. Klein also comes to the similar conclusion about his general formulation of the evil-demon argument.

[13]Unger is aware of this difficulty in his argument; this was pointed out to him by Gilbert Harman. See Ignorance, p. 23n.

[14]Unger, P., Ignorance, p. 19.

[15]Ibid., p. 19.

[16]Ibid., p. 19.

[17]Ibid., p. 25.

[18]*Ibid.*, p. 26.

[19]*Ibid.*, p. 30.

[20]*Ibid.*, p. 33.

[21]See N. Griffin and M. Harton, "Sceptical Arguments", §2.

[22]Similarly, Klein also argues that the second premise of the sceptical argument begs the question. Klein holds that the sceptic would have to show that $\sim Js \sim H_c$ (my italics); but the sceptic cannot justify this without begging the question (*Certainty: A Refutation of Scepticism*, pp.86-87).

[23]Unger, P., *Ignorance*, p. 22, and also with a slight variation in wording on p. 35.

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

[25]*Ibid.*, pp. 35-36.

[26]*Cf.*, *ibid.*, p. 22.

[27]Adopted from N. Griffin, "An Invalid Epistemological Argument Against Double-Action Theories", *Analysis* 38 (1978), pp. 44-45.

[28]*Ibid.*, p. 45. Use of '→' instead of '⊃' is of mine.

[29]Unger, P., *Ignorance*, p. 95, and also in his "An Argument for Skepticism", *Philosophic Exchange* 1 (1974), p. 132.

[30]Unger, P., *Ignorance*, p. 87.

[31]*Ibid.*, p. 68.

[32]This gap has been pointed out to me by D.Hamlyn and L. Stevenson in private communication.

[33]Unger, P., Ignorance, p. 67. For this quotation with a few variations of wording see Unger, P., "A Defence of Scepticism", Philosophical Review 80(1971), p.212. Much of the present polemic against absolute certainty first appeared in this paper. For replies to Unger, see, J.Cargile, "In Reply to A Defence of Scepticism", Philosophical Review 81 (1972), pp.229-36, and G.Barnes, "Unger's Defence of Scepticism", Philosophical Studies 24 (1973), pp.119-24.

On the basis of the quoted argument, Unger says that: 'hardly anyone, if anyone at all, is certain that forty-five and fifty-six are one hundred and one' (Ignorance, pp.67-68). But since this argument is unsound (as we will see), his doubt about this analytic statement is unacceptable.

[34]In his argument that we cannot be absolutely certain about anything, Unger considers 'certainty' as an absolute term and compares it with 'flat' which he also considers as an absolute term. Unger contends that such terms, being absolute, are rarely, if ever, applicable (cf. Ignorance, pp. 48-49, 54-67). For replies to Unger, see J. Cargile, op. cit., pp. 230ff; and S. Andre, "Unger's Defence of Skepticism: New Wine in Old Bottles", Canadian Journal of Philosophy 12 (1982), pp. 453-465.

[35]Unger, P., Ignorance, p. 33.

[36]Ibid., p. 90.

[37]Ibid., pp. 90-91.

[38]Ibid., p. 93, my italics.

[39]In fact Unger is denying the certainty of both the empirical and analytical statements. Since my thesis is concerned with the empirical scientific knowledge, I shall suppress discussion about the certainty of analytical statements. I shall only note that Unger's argument for denying the certainty of analytical statements is unacceptable. See above note 33 and Unger's view referred to there.

I shall also note that there are philosophers who think that analytical statements, like those in mathematics, are empirical. For example, H.Putnam says that, 'mathematical knowledge resembles empirical knowledge - that is, that the criterion of truth in mathematics just as much as in

physics is success of our ideas in practice, and that mathematical knowledge is corrigible and not absolute' ("What is Mathematical Truth?" in his Philosophical Papers [Cambridge: Cambridge University Press, 1975], vol. 1, Mathematics, Matter and Method, p. 61). Also cf. H. Putnam, "Mathematics without Foundations" in his Philosophical Papers, vol. 1, pp.44 and esp. 45ff (first published in The Journal of Philosophy 64 [1967]). Also cf. H.Lehman, Introduction to the Philosophy of Mathematics (Totowa, New Jersy: Rowman and Littlefield, 1979), chs. 7 & 8.

If this empirical interpretation of the mathematical knowledge is correct then my present defence of certainty of empirical statements will apply to mathematics as well, and also to other putative analytical truths.

[40]In such a case error can be avoided by making a conditional certainty claim: I am certain that if this test was not a dud then the rain coat is bottle-green. We however need to get out of this conditional and to establish the consequent independently. That we can independently establish the certainty of the consequent will be clear in the following discussion.

[41]Malcolm, N., "The Verification Argument",pp.269ff.

[42]Bouwsma, O.K., Philosophical Essays (Lincoln: University of Nebraska Press, 1965), p. 97.

[43]Ibid., p. 97.

[44]These two points are due to Dr. N. Griffin (private communication).

[45]It may be noted here that not all sceptics nor all versions of the evil genius argument require this assumption.

[46]Unger, P., Ignorance, p. 63.

[47]Cf., for example, R. Firth, "The Anatomy of Certainty", Philosophical Review 76 (1967), p.11; and B. Russell, Human Knowledge, Its Scope and Limits (New York: Simon and Schuster, 1948), p. 396.

[48]The main theme of the above account of certainty is based on: R. Firth, "The Anatomy of Certainty", pp.3-27; N. Malcolm, "The Verification Argument", pp.244-298; D. Odegard, "Two Types of Scepticism", Philosophy 54 (1979), pp. 459-72; and E. Simpson, "An Analysis of Certainty", Canadian Journal of Philosophy 6 (1976), pp. 403-16.

[49]Unger, P., Ignorance, pp. 25ff, 33ff, 105ff.

[50]Stroud, B., "Review of Ignorance: A Case for Scepticism", Journal of Philosophy 74 (1977), p. 251.

[51]Odegard, D., "Two Types of Scepticism", p. 466.

[52]Unger, P., Ignorance, p. 152.

[53]Ibid., p. 36.

[54]Ibid., p. 37.

[55]Ibid., pp. 240-41. Unger's discussion about irrationality appeared in his "Two Types of Scepticism", Philosophical Studies 25 (1974), pp. 77-96, which forms much of the material of the present discussion.

[56]Unger, P., Ignorance, p. 242.

[57]Ibid., p. 240.

[58]Ibid., p. 232.

[59]Ibid., p. 232.

[60]Ibid., pp. 200-201, and also in his "Two Types of Scepticism", pp. 79-80.

[61]Private communication from Dr. N. Griffin.

[62]Unger, P., Ignorance, p. 200.

[63]Ibid., p. 209.

[64]*Ibid.*, p. 91.

[65]Unger, however, concedes both when he commences his discussion and when he concludes it, that his position may have 'genuine paradoxes' and 'plain contradictions' (*Ignorance*, p. 6, also *cf.* pp. 312-13). But at the same time he considers that such contradictions are due to the inadequacies of our language. Consequently, Unger suggests the need for possible reconstructions in our language (*ibid.*, pp. 6, 313ff) - needs which he himself leaves unsatisfied.

[66]*Ibid.*, pp. 105-06.

# Chapter 3

## Fallibilism à la Popper

An epistemological negativist, instead of being a sceptic, may adopt a different course of argument in his attempt to criticize scientific knowledge; this we have already seen. Karl Popper's position instantiates one such case; he claims that he is not a sceptic,[1] and he formulates a new negative trend: fallibilism. An attempt will be made in this chapter to examine Popper's fallibilism. An outline sketch of the development of Popper's fallibilism will be given at the outset. This development is due to Popper's dissatisfaction with the prevalent methods of scientific investigation; an account of this dissatisfaction constitutes the content of the following section.

### §3.1: Popper's Discontent with the Prevalent Methods of Science.

Popper's fallibilism develops from his search for the method of scientific discovery; and this search develops from his discontent with the already accepted

method: the method of verification. In the verification of a general proposition or theory, the theory is supposed to be reducible to particular statements, its consequences, which can be empirically verified. Presumably, any theory of importance will entail an infinite number of particular statements; and it will not be possible to verify all of them. Popper, therefore, holds that theories are '<u>never</u> empirically verifiable'.[2]

The method of verification was formulated as a criterion for distinguishing verifiable scientific theories and unverifiable metaphysical theories. Due to the high demands the verification criterion makes, many scientific theories will turn out unverifiable, and thus verificationism will fail in its attempt to keep scientific theories distinct from metaphysical theories.[3] Popper's rejection of the method of verification is thus acceptable.

However, it may be noted here that there is a certain tendency on Popper's part to treat the inductive method and the method of verification as inseparable. Such a tendency is obvious when Popper talks about "the positivistic concept of 'meaning' or 'sense' (or of verifiability, or of inductive confirmability...)".[4] To make this clear, I shall explain how Popper considers induction. Popper presents the method of induction in two ways, each of which he attacks. Consequently, as we shall see, we get two <u>slightly</u> different

treatments of induction.

The notion of induction is first presented in the following way: "It is usual to call an inference 'inductive' if it passes from singular statements (sometimes also called 'particular' statements), ... to universal statements, such as hypotheses and theories".[5] This version of induction is rejected because, 'it is far from obvious ... that we are justified in inferring universal statements from singular ones, no matter how numerous; ... no matter how many instances of white swans we may have observed, this does not justify the conclusion that all swans are white'.[6] In other words, instances alone never suffice to establish an inductive generalization since, no matter how many instances we have, we can never be sure that we have all of them.

The notion of induction is next presented in a slightly different way: 'The problem of induction may also be formulated as the question of how to establish the truth of universal statements which are based on experience...'.[7] This is further explained as follows.

> ... people who say of a universal statement that we know its truth from experience usually mean that the truth of this universal statement can somehow be reduced to the truth of singular ones, and that these singular ones are known by experience to be true; which amounts to saying that the universal statement is based on inductive inference.[8]

With these statements about the nature of induction,

Popper holds that to justify inductive inference 'we must first of all try to establish a principle of induction'.[9] But 'the principle of induction must be a universal statement in its turn'.[10] And hence we will have to justify the principle of induction; and thus to justify the principle of induction we will need another induction of a higher order. And this procedure will continue ad infinitum. About this principle of induction Popper says:

> To justify it, we should have to employ inductive inferences; and to justify these we should have to assume an inductive principle of a higher order; and so on. Thus the attempt to base the principle of induction on experience breaks down, since it must lead to an infinite regress.[11]

Clearly, we get according to Popper, two different types of inductive process. In one case it involves an indefinite number of instances of a case; in the other case, inductive inference will require a higher order of inference for its justification, and so on ad infinitum. The first case of induction is getting a universal statement from singular statements. The second case of induction amounts to confirming or validating the obtained universal statements. I shall call these two cases respectively inductive generalization and inductive confirmation or validation. In the first case some generalization is made; and in the second case an attempt is made to confirm or validate some general view, theory or

hypothesis.

Popper, as noted, tends to treat verification and induction as inseparable. But the two can be distinguished - indeed I should argue that Popper is wrong in rejecting induction, and right in rejecting verificationism - and he should not consider them inseparable. For, while it is true that if induction fails, then verification fails, the converse does not hold. Popper acknowledges that the failure of induction entails the failure of verificationism. He says that: "Now in my view there is no such thing as induction. Thus inference to theories, from singular statements which are 'verified by experience' (whatever that may mean), is logically inadmissible".[12] The failure of verification thus stems from that of induction. But the failure of induction does not necessarily follow from that of verification. For, verification may fail due to reasons other than the failure of induction; and moreover, induction may not involve verification at all. Thus the failure of verification does not necessarily imply the failure of induction. I shall, therefore, suggest that Popper's tendency to treat induction and verification as inseparable is unacceptable; and secondly, that though Popper is right in rejecting verificationism, yet it does not follow that he is right in rejecting induction - and indeed he is wrong.[13]

However, Popper's dissatisfaction with verificationism

serves as a strong ground for his attempt for an alternative which leads him to the development of his fallibilist position. I shall pursue this development in the following.

## §3.2: The Development of Popper's Fallibilism.

We have already seen that Popper rejects verificationism, because it fails to solve what he calls the problem of demarcation between science and metaphysics. This problem is defined in the following way: "... the problem of demarcation ... may be defined as the problem of finding a criterion by which we can distinguish between assertions ... which belong to the empirical sciences, and assertions which may be described as 'metaphysical' ".[14] Verificationism fails to solve this problem, because a large area of scientific knowledge is unverifiable; and thus a large area of scientific knowledge becomes metaphysical and meaningless. Moreover, one important aim of Popper's problem of demarcation is to make the demarcation criteria not criteria of meaning, but rather criteria for a demarcation within what is meaningful - a demarcation that separates science from the rest of meaningful discourse. Popper's criterion separates 'two kinds of perfectly meaningful statements... . It draws a line inside meaningful

language, not around it'.[15] Popper further says: 'I always dismissed the problem of meaninglessness as a pseudo-problem; and I was always opposed to the idea that it may be identified with the problem of demarcation'.[16] The verifiability principle is, according to Popper, unsatisfactory from this standpoint. A verificationist's attempt to draw a line between meaningful and meaningless statements renders a large part of empirical knowledge meaningless, and thus leaves no line of demarcation of the sort Popper requires.

Popper's own criterion of demarcation between empirical sciences and metaphysics is the following: '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'.[17] Popper calls this 'the criterion of falsifiability'. There are two requirements which must be met to see whether or not a system is falsifiable, and as such is an empirical system. One is the methodological requirement. There must be some method or methods of empirical test; without any such method the question of the possibility of falsifiability does not arise at all. The second requirement is the logical criterion which a system must have for its falsifiability and which is therefore a necessary precondition for the

first requirement. The latter can be explained in the following way.

If there is an empirical theory or a system, it must be possible to deduce singular statements from it. Some such singular statements can be deduced from the 'initial conditions [of the theory] that tell us what to substitute for the variables in the theory'.[18] But the deduced statements must include more than those which can be deduced from the initial conditions alone. Such deduced singular statements are considered as what Popper calls 'basic statements' - whereas the class of all basic statements can be divided into two sub-classes. To one sub-class belong those statements which are deduced from the theory and which do not contradict it. The statements of this class will be true according to the theory. Given a theory it will be possible to deduce the class of such statements. But to the other sub-class will belong those statements (not deduced from the theory) which will be false according to the theory. The statements of this class will be inconsistent with the theory. In other words, these statements are those which the theory rules out. Popper calls these statements the potential falsifiers of the theory. Popper calls a theory falsifiable if it meets two conditions. First, a theory should be able unambiguously to divide the class of basic statements into these two sub-classes; secondly,

the class of the potential falsifiers is not to be an empty class. These two conditions constitute what Popper calls the logical criterion of falsifiability of a system.[19]

By the criterion of falsifiability, thus explained, we can, according to Popper, make a demarcation between empirical sciences and metaphysics. We can also thus decide whether or not a particular theory or system is empirical in nature. But if we want to decide whether or not a theory is acceptable, within the group of demarcated empirical sciences, we will need another procedure. This is provided by the method of deductive testing or the method of corroboration.

Though a theory cannot be verified, it can be tested more and more by testing the consequences of the theory. The consequences are the basic or singular statements derived deductively from the theory. And we have seen that there is the class of potential falsifiers or the singular statements which are ruled out by the theory; and also there is the class of statements which are permitted by the theory. All these derived statements can be tested empirically. If the results of the tests are positive, i.e. if the results support the theory by falsifying the potential falsifiers, and by not falsifying the class of statements permitted by the theory, then the theory is said to

have passed the tests. This is called the method of deductive testing, because a theory is tested by testing the statements deduced from the theory. It may be noted here that the class of potential falsifiers can, and normally will, be an infinite class. Hence the deductive testing may go on indefinitely, and the success of a theory may only be temporary. In this process, "So long as a theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is '<u>corroborated</u>' ".[20] With this view in mind, Popper holds that, "THEORIES are not verifiable, but they can be 'corroborated' ",[21] and that: "we should try to assess how far it [i.e. a scientific theory] has been able to prove its fitness to survive by standing up to tests. In brief, we should try to assess how far it has been 'corroborated' ".[22]

From the above discussion in this section, it is clear how Popper arrives, starting from his dissatisfaction with verificationism and from the search for an alternative, at his view of falsificationism and corroboration. We have seen that any theory or system, to be a theory or system of empirical science, must be falsifiable. This means that any scientific view whatsoever is always open to the possibility of falsification, and may actually be false.

And again any scientific theory, though it might have been corroborated for any length of time, may fail a single test and thus, according to Popper, may turn out to be false. One essential aspect of Popper's view, therefore, is that it is possible for any accepted scientific view whatsoever to be false; equivalently, that all scientific belief is fallible. Thus we get the development of Popper's fallibilism. Popper declares himself a fallibilist.[23]

From the way Popper uses the terms 'falsificationist' and 'fallibilist', it appears that he considers falsificationists to be a subset of fallibilists.[24] Taking these terms in this way, he states his fallibilist thesis as follows:

> [T]he falsificationists or fallibilists - say, roughly speaking, that what cannot (at present) in principle be overthrown by criticism is (at present) unworthy of being seriously considered; while what can in principle be so overthrown and yet resists all our critical efforts to do so may quite possibly be false, but is at any rate not unworthy of being seriously considered and perhaps even of being believed - though only tentatively.[25]

Popper contrasts the group of fallibilists with two other groups. One group is that of verificationists or inductivists or justificationists. And the other group is that of, what Popper calls, the disappointed justificationists or irrationalists or sceptics. As a fallibilist, he rejects the proposals of both these groups. Hence, he does not

believe that the conclusive justification of knowledge claims is possible. Nor does he believe in scepticism or the total rejection of knowledge claims. Unlike the justificationists, he says that, 'We are not interested in establishing scientific theories as secure, or certain, or probable. Conscious of our fallibility we are only interested in criticizing them and testing them, in the hope of finding out where we are mistaken ...'.[26] And unlike the sceptics, he says, 'Indeed, it is only with respect to this aim, the discovery of truth, that we can say that though we are fallible, we hope to learn from our mistakes'.[27] Also to be noted is his view, '... the very idea of error - and of fallibility - involves the idea of an objective truth as the standard of which we may fall short'.[28]

This above account gives an explanation of Popper's fallibilist position. We may here take note of the following two points.

(i) Popper appears to suggest that conclusive justification of knowledge claims is not possible. He is not interested in establishing the certainty of such claims. On the other hand he also says that he is not a sceptic. We may raise here a question: to what extent is knowledge possible in Popper's fallibilism? Is he a negativist?

(ii) Secondly, according to fallibilism, any view is fallible, and may actually be false. And Popper concedes

to the view that we may fall short of the standard of truth. Now the question is: is the possibility of truth compatible with Popper's fallibilism?

Discussion of these questions will follow in the next two sections.

## §3.3: Fallibilism and Knowledge Claims.

One of the important consequences of Popper's fallibilism is the view which he arrives at regarding the possibility of knowledge claims. On the question whether knowledge is possible within his fallibilist framework, Popper's writings indicate both negative and positive answers. After showing that nothing can be verified, that all scientific views are fallible and falsifiable, that theories can only be tested and corroborated but can never be proved, he arrives at the negative conclusion regarding knowledge. It is held that 'Our science is not knowledge'; that science can never claim to have attained truth; that '<u>We do not know: we can only guess</u>'.[29] His positive answer regarding the possibility is implied in many of his <u>later</u> statements; for example: 'Though it [i.e. Popper's position] stresses our fallibility <u>it does not resign itself to scepticism, for it also stresses the fact that knowledge can grow</u>'.[30] Again he

says that, 'I assert that we know what a good scientific theory should be like... . And it is this (meta-scientific) knowledge which makes it possible to speak of progress in science'.[31] These statements clearly imply that Popper accepts the view that knowledge, and progress in knowledge, are possible. And this shows a contradiction with his earlier view. The inconsistent development of Popper's position can be resolved into three factors, contrasting the early Logic of Scientific Discovery and the later Conjectures and Refutations. The three factors are the following.

(i) The principle of fallibility remains the same in both phases (see above page 131, and note 24).

(ii) But his view of truth undergoes a significant shift in the second phase (see §3.4 below, esp. pages 150ff, 158f). Popper takes account of this shift and makes the necessary accommodation for it.

(iii) Popper's view of knowledge has also undergone a significant shift in the second phase, though, in this case, not an explicitly acknowledged one (see pages 133 above, and 148-49 below). Popper thus leaves a gap in his account.

But is Popper's denial of the possibility of

knowledge in the early phase of his fallibilism acceptable? To show it is not, I shall first show that the falsification principle, on which the denial is largely based, fails to render any theory of science actually and finally falsified.[32]

There are a number of grounds, some of which considered by Popper himself, on which the falsification principle may be claimed to have failed. The first one is that of conventionalism: 'According to ... the conventionalist point of view, laws of nature are not falsifiable by observation'.[33] This is because, "the conventionalist seeks in science 'a system of knowledge based upon ultimate grounds' ... it is possible [for conventionalism] to interpret any given scientific system as a system of implicit definitions".[34] With the help of such a system of definitions and by adopting ad hoc and auxiliary hypotheses, conventionalism could make it impossible to falsify a scientific theory. This threat of conventionalism is rejected by Popper because of the very nature of conventionalism. Popper's best defence against conventionalism seems to have been developed from the standpoint of what happens during scientific revolutions or during what he calls 'a time of crisis'.[35] During such times, conventionalism tries to protect old theories, and thus tries to set aside revolutionary new advances simply by upholding the

conventional view. Conventionalism also uses ad hoc hypotheses to reject the new ideas and to preserve its conventions as unchangeable. Thus the conventionalist search for the final, the ultimate, ground is incompatible with Popper's fallibilist view of science. It may be noticed here that Popper refutes the above objections against falsificationism by adopting a fallibilist view of science; a fallibilist view of science, on the other hand, is a consequence of falsificationism. One may wonder whether or not this is a circular defence. Moreover it remains to be seen whether Popper is accepting here a different form of conventionalism.

The non-falsifiability of a theory of science could be presented in another way. Popper says that:

> ... it is always possible to find some way 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.[36]

Popper holds that such (logical) possibilities might make the falsifiability principle dubious. He replies to this objection that the empirical method will be so characterized that it would exclude the above mentioned way of evading falsification.[37] Moreover, no attempt to evade

falsification will be appropriate here: the aim rather should be the opposite. It is thus said that, the 'aim [of this principle] is not to save the lives of untenable systems but ... to select the one which is by comparison the fittest ...'.[38] In other words, Popper will consider this objection an inappropriate one.[39]

Popper's view that the aim is not to save the lives of the untenable systems can in some cases be unsatisfactory. A system may be untenable because it is a newly presented system and requires to be developed, improved and perfected. Though on the other hand some long established system may be found untenable because some mistake has been discovered in it. If a system of the first type is rejected then it is deprived of the opportunity to become established as a system. Popper's view thus may destroy the chance for many systems to be developed and established. This point is made by Lakatos when he suggests that 'a breathing space'[40] should be allowed to a newly developed theory or system. Popper's view that untenable theories should not be saved is in this sense itself untenable.

Apart from these grounds, there may be another ground on which the falsifiability principle can be faulted. I will take an example, a theory T, and show how it is not falsifiable. As stated in the above §3.2, it must be possible to deduce from T two classes of singular or basic

statements. The class of potential falsifiers must be a non-empty class. The members of this class, I may suppose, are $h_i \ldots h_n$, where $i > 0$ and $n = \infty$. T will be false if a member of the class, $h_j$ (where $j \geq i$), a potential falsifier, turns out to be true. But this would be acceptable only if $h_j$ itself is falsifiable, for 'the falsifying hypothesis must be empirical, and so falsifiable'.[41] Hence to see that T is false, we have to see whether $h_j$ is falsifiable.

In this case $h_j$ is a basic statement. Any such basic statement must satisfy "a material requirement - a requirement concerning the event which, as the basic statement tells us, is occurring at the place K. This event must be an '*observable*' event ...",[42] by which he means 'an event involving position and movement of macroscopic physical bodies'.[43] Thus the basic statement $h_j$ can be traced to an observable event; this observable event will in turn produce another basic statement in our attempt to see whether $h_j$ is falsifiable - we may suppose $h_{j1}$. Our attempt to falsify T, *via* $h_j$, will next produce another basic statement $h_{j2}$, and so on *ad infinitum*. And this would raise the objection of an infinite regress, which Popper has raised against other positions. To stop this regress, however, Popper says: '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'; and that, 'if the test is to lead us anywhere, nothing remains but to stop at some point or other and say that we are satisfied, for the time being'.[44] This, however, leads Popper to the adoption of some form of convention.[45] Moreover, one may raise a question: at what point shall one stop and feel satisfied? Unless some guideline is given, some risk of arbitrariness in decision making will remain open. Another question: would it be dogmatic for Popper to hold such a view? Popper would reply:

> But this kind of dogmatism is innocuous since, should the need arise, these statements can easily be tested further. I admit that this too makes the chain of deduction in principle infinite. But this kind of 'infinite regress' is also innocuous... .[46]

From the above discussion it is clear that in an attempt to see whether or not a theory is falsifiable, we are led to conventionalism, and to dogmatism and infinite regress, though in supposedly innocuous forms. But all these enable us only to say that a theory T is falsifiable only in principle; actual falsification will remain unattainable; and that falsification is not final.

On the basis of the above discussion we may now claim that all scientific theories and basic statements which we may claim to know are falsifiable, though

falsification is not final. But do such claims, which are not finally falsified and which may therefore be true, qualify as knowledge claims? To answer this question in the affirmative, we have to see whether we can be justified in believing them.

From Popper's views, it would appear that we cannot fully justify such claims. He repeatedly asserts this view. He says, 'Now I hold that scientific theories are never fully justifiable or verifiable';[47] and that "Scientific theories can never be 'justified', or verified".[48] It may be noticed here that Popper is associating the impossibility of justification with that of verification.

It is apparent that Popper considers both verification and justification as the same. And Popper does not say anything exclusively about the impossibility of justification. Hence it may be considered that the justification of scientific views, for Popper, will be impossible for the same reasons for which verification would be impossible. That is, justification would be impossible because justification would mean conclusive and absolute justification. I shall call this sense of "justification" an extreme or absolute sense. But Popper considers science to be non-absolute and for this reason he considers that the absolute sense of justification does not arise in

the context of science. The non-absolute nature of science is referred to when he says: "The empirical basis of objective science has thus nothing 'absolute' about it".[49] It appears that he considers science in this way because scientific hypotheses fall short of justification in the absolute sense.

An analysis of Popper's view of justification, thus, leads us to the conclusions that:

(i) according to Popper, scientific hypotheses are not justifiable in the absolute sense of justification;

(ii) Popper's discussion seems to indicate that he admits the possibility of justification of scientific hypotheses in the non-absolute sense of justification.

This latter view of justification can actually be achieved in science. I shall show this in the following discussion.

In the last chapter we have seen that we can be absolutely certain that the colour of my rain coat is bottle-green. Will it not be inconsistent on my part to say that I am absolutely certain that the colour of my rain coat is bottle-green, but I am not justified in believing this? I think so. After detailed and careful examination, the nature of which has been outlined in the last chapter,

we may find good reason to believe in a scientific statement and no reason to doubt; in this way we can be absolutely certain about an empirical statement of science, and thus can be justified in believing it. But in the preceding chapter we came to the conclusion that our view of absolute certainty could be reversed; similarly, we will admit that our justification of empirical scientific statements will also be reversible, and thus will not be dogmatic.

It may be noticed here that such justification of a view can be obtained, _via_ absolute certainty, by _careful examination_ of the view. Hence, it may be said that, the method of _careful examination_ can be considered as the method of obtaining absolute certainty and justification of a claim. It may be interesting here to see what relation there is between the method of careful examination and Popper's method of corroboration and deductive testing.

It may be said that both the methods examine a claim as thoroughly as possible, though neither of them attempt to reduce a claim conclusively to experience; both accept a claim only after extensive examinations and tests, and only if the claim can withstand all the examinations and tests; both hold that such an acceptance of a claim can be upheld until a counter-example can be found; both of them face an infinite regress and both restrict it; neither

is dogmatic; and in both cases any acceptance of a claim always remains open to the possibility of reversal. It may be replied that Popper's method of deductive tests and corroboration has the aim of refuting a claim, whereas our method of careful examination has the aim of establishing it. But we should also note that Popper considers a claim to have 'proved its mettle' when it stands to thorough deductive tests. Thus, the method of deductive tests and the method of careful examination can come to the same conclusion, though the former has a different aim. On the other hand, when the deductive tests corroborate a claim, will Popper consider it justified in the non-absolute sense? Popper's position is not clear on this point.

On the other hand an attempt may be made here to show a difference between these two methods. It may be said that after corroboration and deductive tests, a view may be said to have proved its mettle; and a view which has passed careful examination in our sense is said to be absolutely certain. But Popper would not consider a view which has passed his deductive testing as absolutely certain. And thus, for Popper, the consequences of the methods for certainty claims would be quite different.

But if the method of corroboration is presented here as an acceptable method for obtaining justification,

a verificationist or an inductivist may raise some objections. It may be said that the method of corroboration is the result of Popper's discontent with verificationism and inductivism; hence, if the method of corroboration is considered acceptable, it should be shown how this is an improvement over induction or verification. I shall discuss this with reference to the discussion in §3.1 above.

It was suggested in that section that Popper is right in his criticism of verificationism. If the acceptability of a theory depends on its verification, then many theories will never be acceptable, for verification will not be possible in many cases. But the procedure of verification, to some extent, is similar to the procedure of deductive testing. In both cases the consequences of a theory are tested against some empirical basis. The difference between them is that whereas the verification principle does not explain how to restrict the infinite regress of verification procedures, Popper does restrict the infinite regress of deductive testing; and thus Popper overcomes the main difficulty of verificationism. In this sense, Popper's method of deductive testing may be considered as an improvement over verificationism. But the improvement remains incomplete, for, Popper does not provide us with any criterion to decide the point at which the infinite regress of deductive testing can be

stopped.[50]

Like verificationism, induction also attempts to establish a theory by taking account of particular cases. An induction depends on a great number of such cases. Deductive testing and corroboration also involve tests of a great number of particular cases; a theory is accepted if the tests do not contradict the theory. In induction also, a theory is accepted until a counter-example is found. But both corroboration and induction depend on enumeration of particular and singular cases. It may be said that induction tries to accumulate positive evidence in support of a theory; but deductive testing tries to accumulate all those cases which support the theory. But in both cases it is an accumulation of singular cases. It may again be argued that induction aims at arriving at a universal generalization. whereas deductive testing and corroboration do not have this aim. But after corroboration, what is held is held as a general and universal view, a theory. Lastly, it may be argued that induction does not contain any adequate principle which may be used to restrict an infinite regress and thereby dogmatism. But when the inductivist, after the necessary amount of ennumeration, draws the general conclusion - this act itself works like the Popperian decision, which is found in the innocuous infinite regress of deductive tests and corroboration;

and the inductivist's decision may be just as provisional as Popper's. And an inductivist is not so dogmatic that he would stick to a certain view when a counter example to his view is found.

It is also clear that on many issues, there are some basic aspects common to both induction and corroboration.[51] The main difference between the inductivist position and corroboration is that some of the latent and implied aspects of the inductivist position are more explicit and well-defined in corroboration. This offers further support for the claim in §3.1 that Popper cannot be right in considering induction and verification as inseparable; and why, even if he is right in rejecting verificationism, he cannot be right in rejecting induction. Popper's own position is, to a great extent, like the inductivist position.

According to some critics, Popper's method of deductive testing can involve induction in a different way. One may argue that for falsifying a scientific claim, only one refuting instance is not enough. Only one contrary case of a theory may be an isolated or even an unusual or accidental occurrence of a phenomenon - due entirely to experimental error. Hence, scientific claims should be considered false only when they are contradicted by many contrary cases or by at least one contrary case with many

repeatable instances which will falsify a theory. But if there are many contrary cases or many repeatable instances of a contrary case, then they will suggest a view of some generality of the form $(x)(Fx \rightarrow Gx)$.

Agassi recognizes this point, that refutation of a scientific claim cannot be achieved by only one contrary case. Agassi admits that such an argument 'looks reasonable enough to be problematic if not disturbing'.[52] He suggests that, in such a situation, 'generalization should be modified to exclude the [only] exception and the modified generalization retained'.[53] Agassi would object to such modification of a generalization (instead of its refutation) only if the generalization is repeatedly qualified in this manner.[54]

A Popperian, however, may reply that the purpose of the many contrary cases, through which a theory may be falsified, is not to establish a claim of the form $(x)(Fx \rightarrow Gx)$; rather the purpose is to obtain a sufficient degree of assurance that $(\exists x)(Fx \,\&\, Gx)$ - the theory being refuted having claimed that $(x)(Fx \rightarrow \sim Gx)$.

The method of corroboration would within Popper's fallibilist system provide us with a method of testing or examining a theory, and thus, like inductivism, to support a theory. A scientific view may now be accepted which, with the method of corroboration, we have seen to have proved

its mettle, and we can be justified in believing this, until a counter-example is found - though Popper fails to clearly admit the possibility of such justification. But can we not accept such a view - which we can be justified in believing - as a knowledge claim? That we can is indicated in the later, though not in the earlier, phase of Popper's fallibilism. I shall further explain the positive aspect of Popper's view on knowledge in an attempt to reveal the dominating feature of this aspect of his position.

In the second phase of Popper's fallibilism we observe Popper to say, about scientific theories, that: 'some of these theories of ours can clash with reality; and when they do, we know that there is a reality; that there is something to remind us of the fact that our ideas may be mistaken'.[55] Here we see that the possibility of knowledge is clearly admitted, and is made compatible with the possibility of fallibility.

We may note here that Popper might have used the term 'knowledge' in two different senses. In one sense, he says that knowledge cannot be compatible with fallibilism; for, if we are always fallible, how can we know? This sense is prevalent in his Scientific Discovery. On the other hand, Popper seems to have a different sense of knowledge in his Conjectures and Refutations, where he admits the possibility

of knowledge. It is said there: 'What we should do, I suggest, is to give up the idea of ultimate sources of knowledge, and admit that all knowledge is human; that it is mixed with our errors, our prejudices, our dreams, and our hopes...'.[56] We thus again see that though there is a shift in Popper's account of knowledge, yet there is no account of the shift itself; and this leaves behind an unexplained gap in Popper's account. Indeed, Popper's negativism about knowledge in early phase of his fallibilism is unacceptable.

We should also note that though Popper admits the possibility of knowledge in the second phase of his fallibilism, yet he does not admit that we can hold such knowledge with certainty. In pursuing Popper's own view about knowledge, we observe that a Popperian 'can never know for certain whether his findings are true'.[57] Thus though Popper, in the later phase of his fallibilism, has overcome his scepticism about knowledge, yet he has failed to do the same in respect of certainty. In fact this denial of certainty finally remains as the major negative aspect of Popper's fallibilism. This negative aspect has led Popper to deny that we can know the truth about anything. But though Popper has shown flexibility in admitting that we can have knowledge and that we can get nearer to truth (as we shall see shortly), yet he has not shown such flexibility about certainty. In the following

section we will see that the only reason behind Popper's failure to see that we can know truth even though we are fallible is his negativism about certainty. Indeed Odegard has considered Popper among the thoroughgoing sceptics about certainty.[58] With these observations, I shall now commence discussion on Popper's view of truth.

## §3.4: Fallibilism and Truth.

Popper's falsificationism is thoroughly discussed in his Scientific Discovery; but Popper says very little about truth there. About his attitude to the problem of truth at the time to writing Scientific Discovery, he says: 'I preferred to avoid the topic'; for the reason that: "it appeared to me safer and more economical to discuss the criterion of progress [for science] without getting too deeply involved in the highly controversial problem connected with the use of the word 'true' ".[59] In fact Popper's main discussion of the nature and definition of truth is found in his Conjectures and Refutations.

Popper accepts Tarski's correspondence theory of truth. Popper intends to take 'truth' as a synonym for 'correspondence with facts', and then to define the idea of 'correspondence with the facts'.[60] The latter is defined by Popper in the following way: the statement, or the

assertion, 'snow is white' corresponds to the facts, 'if, and only if, snow is, indeed, white'.[61]

Popper considers this view of truth as an objective view, and regards other theories, pragmatic, coherentist, and evidential, as subjective.[62] According to Popper, the coherence theory mistakes consistency for truth; the evidential theory mistakes 'known to be true' for truth; and the pragmatic theory mistakes usefulness for truth. According to the correspondence theory, Popper thinks, a statement may be true even if no one believes it, and a statement may be false even though a great number of people believe it.

This view is not considered, however, to have provided us with a criterion of truth. In this regard Popper holds that: 'we have no criterion of truth, but are nevertheless guided by the idea of truth as a regulative principle ... and that, though there are no general criteria by which we can recognize truth ... there are something like criteria of progress towards the truth'.[63] This assertion is explained by Popper by the following analogy.

Truth, or correspondence with fact, is compared with a mountain peak which remains almost always covered with clouds. A climber 'may not know when he gets there, because he may be unable to distinguish, in the clouds,

between the main summit and some subsidiary peak'.[64] In the same way one may not know whether or not one has found the truth even if one actually gets it. Nor, it seems, may we know how close we have got to it even though we may make some progress towards it.

But Popper's account does not say that it would be impossible to know whether truth could be found. Sometimes it is actually possible to know that one has reached the mountain peak. In the past some people did actually do so. To deny this would be absurd. And thus we may feel encouraged that we could also reach truth, and could have a criterion to recognize it - in spite of Popper's denial and in spite of our fallibility. I shall examine this possibility in the following.

Consider, again, the following case: is it true that the colour of my rain coat is bottle-green? According to Popper, the statement or the assertion 'the colour of my rain coat is bottle-green' is true, i.e. corresponds to facts if, and only if, my rain coat is, indeed, bottle-green. Is it possible for us, or for someone else, to determine whether the rain coat is, indeed, bottle-green? We have seen that it is possible with absolute certainty, as explained in the last chapter. We have seen that it may be difficult to obtain such certainty, and that such certainty may not be final and irreversible - but yet that we can

obtain it in some cases in the sense explained. Hence let us suppose, at least tentatively, that we have found the truth of the statement we are considering here. It would be a contradiction on our part to say that we can be absolutely certain that something is the case, and to deny the truth of the statement to that effect.

The view that the truth of a statement can be found if we can be certain that what the statement says is so, is implicit in Popper's own view. Popper's view that truth cannot be found is due to his view that truth is like a mountain peak and that 'it may be impossible for the climber ever to make sure that he has reached the summit [or the truth]'.[65] Popper's main concern, it seems, is due to his belief that we may never 'make sure' that what a statement asserts is so. In fact, he has been misled here by his scepticism about certainty. It seems that, if we can solve the problem of 'making sure' or making certain about fact, then the problem of finding truth may be solved.

An objection may, however, be raised here. It may be said, as Odegard observes, that 'Absolute certainty does not entail absolute truth, and finding something absolutely certain is therefore not to find it absolutely true'.[66] But this objection cannot undermine our claim that we can know the absolute truth, for reasons Odegard points out:

> We can solve the problem by enlisting the principle of implicit justification.
>
> If we are justified in believing p and also justified in believing "p entails q," we are justified in believing q.
>
> Given this principle, our being justifiably sure of a proposition guarantees that we are justified in being sure of the proposition's truth, since we are justified in being sure that p entails "p is true."[67]

On the basis of the principle of implicit justification, we can hold that since we can be absolutely certain that the colour of my rain coat is bottle-green, we can be justified in believing it. And because, if p, then p is true, we can also be justified in believing that our statement about the colour of the rain coat is true.

The principle of implicit justification thus provides us with what Odegard calls the 'access to truth'.[68] And the above objection that absolute certainty cannot provide us with absolute truth is an insubstantial one.[69]

It is, however, possible to raise an objection against the possibility of truth in a different way. It may be said: even though we can say that the statement 'the colour of the rain coat is bottle-green' is true, it may not actually be true. This is because, as also noted in the above discussion, the absolute certainty that such and such is the colour, is open to the possibility of change; while what is true, must be true for all time and

can never be reversed. Hence one may wonder whether any truth claim is acceptable while it is open to the possibility of reversal.

A truth claim may be reversed because truth is the correspondence with facts, and facts do change. And thus what can be true about a thing at one time, may not be true at a later time. But when the truth claim gets changed, the claim does not turn out to be false; the claim is replaced by a new claim. I shall say that it is true that the colour of my rain coat is at present bottle-green; if the colour is changed in future, for example, if it is dyed brown, then I shall say that it is true that the colour of the rain coat is now brown. But I shall not say that my previous claim has turned out to be false; rather I shall say that now we have a new truth claim, because the corresponding facts which we now have are different. Odegard presents this point in the following way.

> ... if we say "It was once true that John could eat hot peppers, but it's no longer true," we shall understand this to imply that his being able to eat peppers <u>then</u> was the case and his being able to eat peppers <u>now</u> is not the case. [But] His having the first temporalized ability is still the case now and his having the second ability was not the case even then.[70]

We thus see that facts change, and we make corresponding new truth claims; but a truth claim which corresponds to a

fact, at a given time, never changes with respect to that fact at that given time. To give another example, we may observe that, 'although my son has changed from short to tall, his having been short in 1968 has not changed'.[71] Such changes in our truth claims do not constitute any problem for us.

On the other hand, a truth claim may get reversed due to some error in our evidence or in our determination of the facts. It is always possible for us to err in the way we determine the correspondence between judgment and fact. And this possibility will leave us without a single truth claim which would be impossible to reverse. But this reversibility is a mere logical possibility. We may find a statement to be true by objective factual correspondence; mere logical possibility of an error in our finding cannot be sufficient for rejecting this. Many of the truths, which we claim to have found, may actually be false; and at the same time many of the truths, which we claim to have found, may actually be true. The reason why we cannot make ourselves free from the logical possibility of the reversibility of our proclaimed truth, is that: an attempt to determine a correspondence of a statement with the facts would involve an infinite regress of examinations of the facts. But we cannot conduct an infinite number of careful examinations in order to defend a truth claim - but

that does not mean that the truth claim cannot be accepted. I reaffirm: a mere reference to an infinite regress is not sufficient for rejection of a certainty claim. We can claim to have found truth at least in the case of many of what Popper calls basic statements.

On the basis of the above discussion it may be said that we do have a criterion for asserting the truth of a statement or its correspondence with facts. The criterion is the following: the truth of a statement can be tested by careful examination, and if we find after examination that there is strong evidence that such and such is the case, and if we find no counter-evidence, then we can say that the statement is true. This is, however, not to imply that it is simply enough to examine what a statement says, considering the statement in isolation. Presumably, a careful examination of a statement would involve many factors other than just what is asserted by the statement. What we require is that careful examination must be carried out as thoroughly as outlined in the last chapter.

Popper fails to recognize such a criterion for the correspondence of a statement with facts. This view about the unattainability or the impossibility of truth led Popper to make a distinction between unattainable absolute truth and the attainable proximity of truth. He acknowledges

that:

> ... whenever I used to write, or to say, something about science as getting nearer to the truth, or as a kind of approach to truth, I felt that I really ought to be writing 'Truth', with a capital 'T', in order to make quite clear that a vague and highly metaphysical notion was involved here, in contradistinction to Tarski's 'truth' which we can with a clear conscience write in the ordinary way with small letters.[72]

Later on Popper blurs this distinction between 'Truth' and 'truth'. He accepts the view that there really was 'no barrier here between what at first sight appeared to be Truth with a capital 'T' and truth in a Tarskian sense'.[73] And thus he considers Tarskian truth in the sense of absolute Truth. As a result he has maintained that we can never get Truth, but we can get nearer to Truth, or that we can get likeness or similarity to Truth, which he calls '<u>verisimilitude</u>'. He holds that verisimilitude can be obtained, and obtained in different degrees:

> Verisimilitude is so defined that maximum verisimilitude would be achieved only by a theory which is not only true, but completely comprehensively true: if it corresponds to <u>all</u> facts, as it were, and, of course, only to <u>real</u> facts. This is of course a much more remote and unattainable ideal than a mere correspondence with <u>some</u> facts... .[74]

The last sentence of this passage may be noticed in particular. Popper is rendering maximum verisimilitude as a

stronger notion than absolute truth. Moreover, it is not at all clear how a theory might correspond to all facts. And there seems to be no difference between what Popper calls the degrees of truth and what he usually means by verisimilitude in the non-maximal sense; and both of these are attainable. It does not seem, therefore, that, by introducing the notion of verisimilitude, Popper introduces any view significantly different from that of truth.

However, to accept the correspondence theory in spite of human fallibility, we have to answer two questions. First, is the correspondence theory of truth compatible with the notion of human fallibility? Secondly, is there any objection which can be effectively raised against the correspondence theory of truth as explained above? Answers to these questions will be attempted in the following discussion.

A little reflection may convince one that the correspondence theory of truth is compatible with human fallibility. It has been explicitly said above that truth claims are open to the possibility of revision. And therefore any truth claim which we may hold is fallible, though it does not follow that all such claims are actually false. In the case of truth claims, as in other cases, the possibility of falsity does not entail actual falsity. And thus it may be asserted that our fallibility and the

above version of the correspondence theory are quite compatible.

In my attempt to see if there is any effective objection against this theory, I shall consider one criticism which can be put forward against Tarski's theory of truth. This is because both Popper's position on truth and the view which I have adopted here are based on Tarski's theory.

One can raise the objection that this theory is a mere tautology, and does not give us anything more than an adequacy condition for a theory of truth and thus does not give the truth value of any statement. It only says that a statement is true if it can satisfy a certain condition. But can we know whether or not a statement satisfies this condition? What way or ways do we have for knowing that a statement has satisfied this condition? The correspondence theory, at least in its Tarskian origin and Popperian pursuance, is either incomplete or unsatisfactory in respect to these questions.

Popper discusses this aspect of Tarski's theory of truth.[75] Popper recognizes the distinction between the definition of truth and the criterion of truth, but also recognizes the great significance of Tarski's correspondence theory which provides the definition of the notion of truth. Popper's claim, as has been quoted in the

beginning of this section, that there is no criterion for determining the truth or the correspondence of a statement is due to his failure to provide Tarski's theory with a truth criterion. While speaking in connection with truth, he admits: 'there exists no general criterion of its applicability in specific cases'.[76] As Mary Hesse puts it, Tarski's theory of truth, in this respect, is 'at best incomplete'.[77]

These objections, however, are not applicable against the extended version of Tarski's theory that I have presented here. We may again consider the definition that the sentence 'snow is white' is true if and only if snow is indeed white. But we can never know the truth of the sentence unless we can independently know the truth of the consequent part of this definition. To know whether the sentence 'snow is white' is true, first we have to know independently whether snow is white. I have argued that we can do this by establishing the absolute certainty that snow is white. And we can do this by what I have described as the method of careful examination.

I shall, therefore, assert that we have a criterion by which we can actually determine the correspondence of statements with the respective facts. And hence Tarski's correspondence theory no longer remains uninformative; Tarski's truth definition, in the above discussion, has

been supplemented with a truth criterion. And thus Popper's negativism about truth - which is partially due to his failure to provide Tarski's theory with a truth criterion - is untenable as well.

## §3.5: Conclusion.

In this chapter we have undertaken a critical examination of Popper's fallibilism and his fallibilist views about truth, certainty and knowledge. We have found that the negativism of Popper's fallibilism is more far reaching than can be sanctioned. He seems unclear about the possibility of non-absolute justification; his early fallibilism is negative about knowledge. He denies that we can ever be certain about anything and that we can know truth. On the contrary we have seen that we can be certain about statements of the empirical sciences and that we can be justified in believing them; and furthermore, we can know the truth about them - and we have seen that all these are compatible with our fallibility.

## Notes: Chapter 3

[1]This is only partially true as we will see.

[2]Popper, K., Scientific Discovery, p. 40; also cf. pp. 252ff.

[3]Ibid., p. 40. It may be noticed here that a scientific theory may be unverifiable, not because we are unable to verify an infinite number of cases, but due to the fact that we may lack scientific means necessary for the verification.

[4]Popper, K., "The Demarcation Between Science and Metaphysics", in his Conjectures and Refutations, p. 253; first published in P.A.Schilpp ed., The Philosophy of Rudolf Carnap (La Salle: Open Court, 1964). References are from Conjectures and Refutations.

[5]Popper, K., Scientific Discovery, p. 27.

[6]Ibid., p. 27.

[7]Ibid., p. 28, my italics.

[8]Ibid., p. 28, my italics.

[9]Ibid., p. 28.

[10]Ibid., p. 29.

[11]Ibid., p. 29.

[12]Ibid., p. 40.

[13]See §3.3, pp. 144-46 below.

[14]Popper, K., "A Criterion of the Empirical Character

of Theoretical Systems", in his Scientific Discovery, p.313; first published in Erkenntnis 3 (1933). Also cf. Scientific Discovery, p. 34, and Conjectures and Refutations, pp. 253ff.

[15]Popper, K., Scientific Discovery, p. 40, n.*3.

[16]Popper, K., Conjectures and Refutations, p.258.

[17]Popper, K., Scientific Discovery, p. 41.

[18]Ibid., p. 85.

[19]Ibid., pp. 85-86.

[20]Ibid., p. 33.

[21]Ibid., p. 251.

[22]Ibid., p. 251.

[23]Cf. Popper, K., "Truth, Rationality, and the Growth of Scientific Knowledge", in his Conjectures and Refutations, p. 228. Reprinted as Truth, Rationality, and the Growth of Scientific Knowledge (Frankfurt am Main: Klostermann, 1979). References are from Conjectures and Refutations.

[24]Popper's use of the term 'falsificationism', via. its German original, can be traced as far back as 1934, when his Logik der Forschung was published. But his use of the term 'fallibilism' appears to go back to 1960 when he wrote the section of his Conjectures and Refutations which I have just quoted (cf. ibid., 215n). Popper's present use of these two terms indicates that, his views as a fallibilist from 1960 onwards should be compatible with his views as a falsificationist from 1934 onwards. This link between his falsificationism and fallibilism will thus be assumed.

[25]Popper, K., Conjectures and Refutations, p. 228.

[26]Ibid., p. 229.

[27]Ibid., p. 229.

[28]Ibid., p. 229.

[29]Popper, K., Scientific Discovery, p. 278.

[30]Popper, K., Conjectures and Refutations, p.vii, my italics.

[31]Ibid., p. 217.

[32]It may be noted here that if falsification were final, even then some knowledge could be possible, viz., that some theories are false.

[33]Popper, K., Scientific Discovery, p. 79.

[34]Ibid., p. 80.

[35]Ibid., p. 80.

[36]Ibid., p. 42.

[37]Ibid., p. 42.

[38]Ibid., p. 42.

[39]Critics recognize that Popper's falsifiability principle fails as the demarcation criterion and consequently science-metaphysics distinction collapses. See, for example, W.C.Kneale, "The Demarcation of Science", in P.A.Schilpp ed., The Philosophy of Karl Popper (La Salle: Open Court, 1974) vol. I, p. 206; also cf. C.G.Hempel, "Problems and Changes in the Empiricist Criterion of Meaning", in L.Linsky ed., Semantics and the Philosophy of Language (Urbana: University of Illinois Press, 1972), p. 170.

I shall not, however, elaborate on demarcation problem; my main point here is to examine whether or not the falsifiability principle is instrumental to corroboration or justification of knowledge claims.

[40]Lakatos, I., "History of Science and its Rational

Reconstructions", in R.C.Buck and R.S.Cohen ed., Boston Studies in the Philosophy of Science (Dordrecht: D.Reidel, 1971), vol. 8, p. 113.

[41]Popper, K., Scientific Discovery, p. 87.

[42]Ibid., p. 102.

[43]Ibid., p. 103.

[44]Ibid., p. 104.

[45]Lakatos recognizes that Popper himself adopts some form of conventionalism, though he is opposed to the traditional one (cf. I. Lakatos, "Popper on Demarcation and Induction", in P.A.Schilpp ed., The Philosophy of Karl Popper, vol. 1, pp. 242-43). In support of his claim, Lakatos quotes Popper's view that 'Methodological rules are here regarded as conventions' (cf. ibid., p. 263, n.13). We may further notice that Popper's demarcation criterion is a 'proposal for an agreement or convention' (Scientific Discovery, p. 37).

[46]Popper, K., Scientific Discovery, p. 105. One can at once raise a question here: why is not the infinite regress of induction innocuous? We will come to this point concerning the infinite regress in induction shortly hereafter.

[47]Ibid., p. 44.

[48]Ibid., p. 315.

[49]Ibid., p. 111.

[50]I do not consider that verificationism simply is opposed to Popper's position. Some traits of verificationism are to be found in Popper's position. This has been recognized by a number of critics. For instance, Ayer, a verificationist, gives the following quotation from Popper: 'I can examine my guess critically, and if it withstands severe criticism, then this fact may be taken as a good critical reason in favour of it' (Conjectures and Refutations, p. 234). Ayer then raises the question: 'And

how does this in the end differ from the idea of progress through verification which Popper believes that he rejects?' (A.J.Ayer, "Truth, Verification and Verisimilitude", in P.A.Schilpp ed., The Philosophy of Karl Popper, vol. 2, p. 691).

[51]Stove holds that Popper's position involves, what he calls, eliminative induction ("Review of The Logic of Scientific Discovery", Australasian Journal of Philosophy 38 (1960), pp. 181ff). Agassi, however, does not think that this is an appropriate criticism (J.Agassi, "The Role of Corroboration in Popper's Methodology", Australasian Journal of Philosophy 39 (1961), pp.84-85), and he does not see Popper's position as belonging to the inductivist tradition (ibid., p. 91). But it should be noticed here that though Popper does not belong to the inductivist tradition, yet his philosophy is not free from this trait. Agassi also acknowledges elsewhere that Popper concedes that his view contains a whiff of verificationism (J.Agassi, "Science in Flux: Footnotes to Popper", in J.Agassi, Science in Flux, R.S.Cohen and M.W.Wartofsky ed., Boston Studies in the Philosophy of Science [Dordrecht: D. Reidel, 1975], vol. 28, p. 27).

[52]Agassi, J., Science in Flux, R.S.Cohen and M.W.Wartofsky ed., Boston Studies in the Philosophy of Science (Dordrecht: D. Reidel, 1975), vol. 28, p.142.

[53]Ibid., p. 142.

[54]Loc. cit.

[55]Popper, K., "Three Views Concerning Human Knowledge", in Conjectures and Refutations, p. 117, my italics; first published in H.D.Lewis ed., Contemporary British Philosophy, 3rd series (London: George Allen and Unwin, 1956). References are from Conjectures and Refutations.

[56]Popper, K., "On the Sources of Knowledge and Ignorance", in Conjectures and Refutations, p. 29; first published in the Proceedings of the British Academy 40 (1960). References are from Conjectures and Refutaions.

[57]Popper, K., Conjectures and Refutations, pp. 114-15, my italics.

[58]Odegard, D., "Two Types of Scepticism", pp.462-63.

[59]Popper, K., Conjectures and Refutations, p. 223.

[60]Ibid., p. 224.

[61]Ibid., p. 224. Cf., A. Tarski, "The Semantic Conception of Truth", Philosophy and Phenomenological Research 4 (1944), pp. 343ff; reprinted in L.Linsky ed., Semantics and the Philosophy of Language, ch. 2.

[62]Cf.Popper,K.,Conjectures and Refutations,pp.224-25.

[63]Ibid., p. 226.

[64]Ibid., p. 226.

[65]Ibid., p. 226, my italics.

[66]Odegard, D., Knowledge and Scepticism (Totowa, New Jersy: Rowman and Littlefield, 1982), p. 153.

[67]Ibid., p. 154. For objections to this principle, see K. Lehrer, Knowledge (Oxford: at the Clarendon Press, 1974), p. 227; E. Wolgast, Paradoxes of Knowledge (Ithaca: Cornell University Press, 1977), ch. 1; and R. Richman, "Justified True Belief as Knowledge", Canadian Journal of Philosophy 4 (1975), pp. 435-39. For replies to these objections, see D. Odegard, Knowledge and Scepticism, pp. 154-55.

[68]Odegard, D., Knowledge and Scepticism, ch. 13, pp. 152-58.

[69]We may also note that Odegard's principle can be formalized as:

$$JaBap \ \& \ JaBa(p \rightarrow q). \rightarrow .JaBaq,$$

where 'JaBap' reads 'a is justified in believing that p'. This formulation and what we have presented in the preceding chapter as the reformulation of the assumption of reasoning have much in common. This again enhances the consolidation

of our position against sceptics, and against the denial of truth.

[70]Odegard, D., Knowledge and Scepticism, p. 40.

[71]Ibid., p. 42.

[72]Popper, K., Conjectures and Refutations, p. 231.

[73]Ibid., p. 232.

[74]Ibid., p. 234.

[75]Popper, K., Objective Knowledge (Oxford: Clarendon Press, 1972), p. 320.

[76]Ibid., p. 320.

[77]Hesse, M., "Truth and the Growth of Scientific Knowledge", in her The Revolutions and Reconstructions in the Philosophy of Science (Sussex: The Harvester Press, 1980), p. 142.

# Chapter 4

## Feyerabend's Epistemological Anarchism

In earlier chapters we have seen that scepticism and fallibilism cannot undermine scientific knowledge. We have seen that we have a method which enables us to obtain absolute certainty, and which in turn enables us to obtain truth and knowledge. In this chapter I shall argue that epistemological anarchism, as presented by Feyerabend, is unacceptable. But first I shall make clear the limits within which I shall examine Feyerabend's position.

Feyerabend's works can be divided into two phases: a non-anarchistic phase up to 1970 and an anarchistic phase thereafter. Roughly speaking, Feyerabend's epistemological anarchism emerges with his "Against Method" in 1970, which is later extended into an elaborate work of the same title. His works before "Against Method" are mainly criticisms of the traditional empiricism. These criticisms may be considered to give 'an index of the contemporary crisis of empiricism'.[1] I do not intend to examine this phase of Feyerabend's work. I shall examine only the later phase where Feyerabend

derives his anarchistic conclusions regarding epistemology from his observations regarding the methods of empiricism and science. I shall examine whether his anarchistic conclusions logically follow from his observations, and whether the observations themselves are accurate. I shall confine my discussion mainly to Against Method, which is the main anarchistic work, though some reference will be made to some of Feyerabend's other works as well. First I shall briefly examine the basic anarchistic thesis that anything goes.

## §4.1: The Thesis that Anything Goes.

Feyerabend states his thesis that anything goes at the very beginning of his book Against Method. His main position regarding this thesis can be stated in the following propositions: (i) that science is an ever-changing and flexible enterprise and is humanitarian in attitude;[2] (ii) that the methods proposed for science by philosophers of science are rigid; (iii) such methods inhibit progress in science; (iv) and therefore the principle to be accepted is the one which is not fixed, and can take account of all circumstances, and as such does not inhibit progress. It is held that: 'the idea of a fixed method, or of a fixed theory of rationality, rests on too naive a view of man

and his social surroundings. ... there is only one principle that can be defended under all circumstances and in all stages of human development. It is the principle: anything goes'.[3] As suggested here, the rigidity of fixed methods is due to a naive understanding of man and his surroundings; and it is implied that Feyerabend's thesis is due to a better understanding thereof.

Feyerabend's views on this point are given in terms of his view of human history. Feyerabend conceives history as full of accidents, conjectures and curious juxtapositions of events, which are dominated by complexity and unpredictability.[4] The history of science, a part of the history of society,[5] shares its character:

> (1) The history of science contains ideas, interpretations of facts, problems created by conflicting interpretations, mistakes and so on.
>
> (2) On closer analysis we even find that science knows no 'bare facts' at all but that the 'facts' that enter our knowledge are already viewed in a certain way and are, therefore, essentially ideational.
>
> (3) This being the case, the history of science will be as complex, chaotic, full of mistakes, and as entertaining as the ideas it contains, and these ideas in turn will be as complex, chaotic, full of mistakes, and as entertaining as are the minds of those who invented them.[6]

We may examine these steps in the following way. In the first place, the statement (1) is not of much help for

Feyerabend's position. Even if the history of science is full of ideas, problems, conflicting interpretations and mistakes, it does not necessarily follow that anything goes. Moreover, the history of science may well be as colourful as Feyerabend claims; but this by no means exhausts the content of science, and ignores altogether its respect for data, consistency, rigorous criticism and open-mindedness. To this extent the above step (1), Feyerabend's account of the history of science, remains inadequate.

Secondly, the premise (2) is unduly strong. By saying that facts, which enter scientific knowledge, are essentially ideational, Feyerabend means that all facts are viewed and consequently determined by individual minds in a certain way. Feyerabend holds that all facts are essentially determined in this way and that this is inevitable. One may ask why science knows no bare facts at all. If science considers the fact that snow is white, will not that be a consideration of a bare fact? And does not science deal with such matters?[7] But even if facts are already viewed in a certain way, will they be essentially ideational? Such a claim would be unduly strong, and may amount to a subjective idealist position. Even Gonzalo Munévar, who is a radical epistemologist in the Feyerabend spectrum and is largely sympathetic to Feyerabend, would

hold that here 'Feyerabend goes too far'.[8] Munévar considers four traditional positions with which he does not agree, one of them being subjectivism: 'Reality is in the mind'.[9] Feyerabend's above claim seems to fall in this category. It is true that facts are already viewed in a certain way; Munévar would agree with Feyerabend in this claim.[10] But it does not follow that facts are <u>essentially</u> ideational. This reminds one of another comment of Munévar's, that though the picture Feyerabend presents of science is largely correct yet it is not the case that 'the conclusions he draws from that picture are correct'.[11]

Next, in (3), Feyerabend has <u>assumed</u> that scientific minds <u>in general</u> are chaotic and full of mistakes. This is a strong assumption, and has not yet been supported by facts or arguments. Moreover, its consequences, <u>if any</u>, for epistemology seem rather obscure.

Since the history of science is, according to Feyerabend, so complex, a question is raised whether 'the naive and simple-minded rules which methodologists take as their guide'[12] are adequate. In contrast to Feyerabend's concern, we may observe that sometimes apparently simple-minded rules may be adequate in dealing with the complex phenomena, to take an example, the problem of moving bodies. In treating this complex problem, Galileo used simplifying assumptions and special cases and his interpretation has

been quite successful. One implied suggestion of Feyerabend's is that science is usually understood in a naive and simple-minded way. But it has yet to be shown that Feyerabend's understanding of the history of science is accurate and complete. And it is not clear how far his account is from being naive.

However, from his claim that the history of science is very complex, Feyerabend draws further conclusions, which in turn reinforce his thesis that anything goes.[13] It is thus argued that in the history of science, scientific methods have been proposed on the basis of a too simple understanding of the history, and as such have been made rigid and unsatisfactory. Feyerabend argues that, since the acquisition of scientific knowledge depends upon the personal and individual aspects of the investigator, the question of whether methodological description of the acquisition of scientific knowledge should be confined to strict rules should be answered with 'a firm and resounding NO'.[14] The answer is claimed to be warranted, first of all, because rigid and strict rules will obstruct the exploration of the world, which is largely an unknown entity; and secondly because, such rigid and strict rules obstruct individual development and humanitarian attitudes.

What Feyerabend suggests here is to replace

strict rules by freedom, and by 'freedom' Feyerabend understands anarchism, a complete absence of law and order. Feyerabend also believes that such anarchism will not lead to chaos: for, 'The human nervous system is too well organized for that'.[15] But, since the human nervous system is strong enough to withstand chaos, it seems that anarchism will not be very liberating. For Feyerabend claims, 'Even in undetermined and ambiguous situations, uniformity of action is soon achieved and adhered to tenaciously'.[16] If Feyerabend is right in these observations, then anarchism will be completely unnatural and unsuitable for human beings. Feyerabend is in fact proposing an impossible methodology. On the other hand, if social conformity is the natural human condition resulting from the structure of the nervous system, then the only good defence against such conformity would be rational criticism.

But what implications can Feyerabend derive from the observations about strict rules? First, does 'a resounding NO' to rigid and strict rules suggest that <u>anything goes</u>? Feyerabend's rejection of rigid and strict rules suggests that the only alternative to rigid and strict rules is no rules at all. Feyerabend himself suggests that the 'attempt to increase liberty, to lead a full and rewarding life, and the corresponding attempt

to discover the secrets of nature and of man entails, therefore, the rejection of all universal standards and of all rigid traditions'.[17] But the rejection of strict and rigid rules is not the same as the rejection of all rules as such or of the very notion of rule itself. And hence, anything cannot go yet. Moreover, if science is as full of mistakes and idiosyncracies as Feyerabend suggests, even then it is necessary to describe scientific history in terms of firm methodological precepts around which the idiosyncracies could be understood.

These views constitute the fundamental ideas of Feyerabend's whole position and can be divided into two groups: (i) Feyerabend's observations regarding history (particularly history of science); the rigidity of methods and rules supposedly discovered there; the nature of facts as we know them (whether or not they are bare) and the nature of their interpretations, etc. and (ii) his derived anarchistic conclusion: anything goes. Feyerabend considers in detail each of the observations as a means to establish his anarchism. In the following sections of this chapter I shall examine these detailed accounts. Throughout all the following sections I shall emphasize that the anarchistic conclusion that anything goes does not follow.

§4.2: Proceeding Counterinductively.

Feyerabend designs his thesis of counterinduction to undermine inductive results and thus to undermine all inductively established theories and facts. If this can be done then the thesis that anything goes will get a strong defence. From this standpoint Feyerabend presents his thesis of counterinduction first at the beginning of the second chapter of Against Method - just immediately after stating the anything-goes thesis.

The thesis of counterinduction may be presented as follows: it 'advises us to introduce and elaborate hypotheses which are inconsistent with well-established theories and/or well-established facts. It advises us to proceed counterinductively'.[18] It may be noted here that there is a difference between introducing a hypothesis and adopting it.[19] We may introduce a hypothesis which is inconsistent with a well-established theory; but introducing such a hypothesis is not necessarily to undermine the already accepted theory but may be only to sharpen it, compare it with alternative points of view or to subject it to critical scrutiny. Such uses of alternative hypotheses do not constitute counterinduction; yet they are the uses to which such hypotheses are typically put in science. This point, that introducing new hypotheses or alternatives may not necessarily be a counterinduction

but instead may reinforce an induction can be defended by Feyerabend's own observations. Feyerabend would agree that theory constructions proceed by a survey of alternative positions. Feyerabend says that:

> A scientist who wishes to maximize the empirical content of the views he holds and who wants to understand them as clearly as he possibly can must ... introduce other views... . He must compare ideas with other ideas rather than with 'experience' and he must try to improve rather than discard the views that have failed in the competition.[20]

This passage clearly indicates that by introducing alternative views, a scientist can strengthen well-established views and theories.

Next we may observe that if Feyerabend is to defend counterinduction then he must refute what he calls 'the consistency condition', namely, 'the demand that new hypotheses must be consistent with such [i.e. well-established] theories'.[21] Feyerabend links the consistency condition with traditionalism and attacks the latter. He contends that traditionalism preserves and defends old theories, only because they are old. But given the condition of counterinduction, that introduction of a new theory counts as counterinduction only if the old theory is well-established, it is difficult to see how Feyerabend can claim that the rejection of counterinduction amounts

to a defence of the old theory because it is old, rather than a defence of a well-established theory because it is well-established. Feyerabend, however, presents this brief note on the consistency condition as the 'first shreds of support for counterinduction'.[22] We will see detailed evaluation of the consistency condition in the following section.

As a means of showing how well-established theories can be overthrown by already rejected theories, Feyerabend holds that: 'The examples of Copernicus, the atomic theory, Voodoo, Chinese medicine show that even the most advanced and the apparently most secure theory is not safe, that it can be modified or entirely overthrown with the help of views which the conceit of ignorance has already put into the dustbin of history'.[23] But this is not an adequate defence for the thesis of counterinduction. Can we come to the conclusion that we should proceed counterinductively from the observation of failure of a number of inductive generalizations or theories? Let us suppose that we can; in this case, are we not establishing a rule? Or, are we not establishing a claim of some generality like that of a principle? I think that the answers to these questions are in the affirmative. And Feyerabend is contradicting his own position, for, as an anarchist his position is not supposed to lead to any such claims.

It may be replied that Feyerabend is not trying to establish a general view in this respect; but instead he is merely saying that some inductions fail and that they are contradicted by apparently unacceptable ideas. In other words, one may say that Feyerabend is not saying that no inductive arguments are acceptable, but only that some counterinductive ones are.

But the above quotation shows that Feyerabend is projecting his view against inductive generalizations; he is not claiming that some counterinductions are valid. For to claim merely that some counterinductions are valid does not support the thesis that anything goes. The thesis of counterinduction is aimed at establishing this anarchistic conclusion. And this cannot be achieved as long as induction remains a satisfactory working procedure in some cases - for, anarchism then cannot go. Hence, Feyerabend's main thesis will not get any support if counterinduction is taken to be the limited thesis that some counterinductive arguments are valid. It is, therefore, more reasonable to consider the thesis of counterinduction as a claim of some generality as regards the inadequacy of induction. This interpretation is supported by Feyerabend's own statements, for example: 'Counterinduction is ... always reasonable and it has always a chance of success'.[24] It is also said that: 'all methodologies,

even the most obvious ones, have their limits'.[25] These views which are presented in the context of the thesis of counterinduction indicate that his claim is intended as a general one.

One may, however, again reply that what Feyerabend is claiming is that counterinduction has always a chance of success, but not that counterinduction always actually succeeds. If so, a counterinductive attempt to overthrow an inductive generalization may not succeed. Consequently, what follows is that sometimes counterinduction will be successful, and sometimes inductive generalizations will remain valid, and we cannot be sure which one will succeed in which situation. If we accept this reply then anarchism does not necessarily follow. On the other hand, it cannot be denied that Feyerabend's claim is of the following nature. Let us suppose that 'Ci' stands for counterinduction. Feyerabend would then claim that: for all x, if x is Ci, then it is possible that x will succeed. This general nature of his principle is further clear when Feyerabend derives a general claim, viz., his claim regarding the rejection of all rules. When Feyerabend suggests we reject all rules, his view can be presented as,

(R) ignore all rules.

But is (R) a rule? If so, then it involves the paradox of self-refutation. But if (R) is not a rule then we can

follow rules and his anarchistic thesis does not follow.

The self-refuting aspect of Feyerabend's position is also evident in his anti-methodological campaign. Regarding Feyerabend's motto 'against method' and his attempt to overthrow all methodological rules, Meynell says: 'The most fundamental objection to Feyerabend's position seems to be this: that his attack on "Methods" supposed to be operative in science is based on historical arguments, which themselves involve the application of "methods" very like those which he is attacking'.[26] Or it may be asked: 'Does he neglect evidence, and rely on rhetorical powers of persuasion instead? Not a bit ... at least the <u>form</u> of his argument is of a kind that would appeal to the most stringent of methodologists'.[27] Feyerabend thus again contradicts himself.

Regarding the self-refuting character of the thesis of counterinduction Feyerabend must face an inescapeable consequence. Either Feyerabend's notion of counterinduction is general, in which case it refutes itself, or it is not, in which case it does not imply that anything goes. In fact, the most Feyerabend can say is that anything <u>might</u> go, not that all things actually do. This difference between 'anything goes' and 'anything <u>might</u> go' is extremely important as we have seen in the discussion of scepticism. Feyerabend's argument seems to have employed a modal fallacy: whereas

he could quite reasonably say that anything might go, he in fact makes a much stronger claim - anything goes.[28]

We may note here a further point. In the above we have seen Feyerabend's view, in connection with his defence of counterinduction, that all methods have their limits. In fact, it is true that there is no universal and unerring method, and the limitation of all methodologies is provable.[29] But if we concede this to Feyerabend, it does not follow that there cannot be any acceptable method, and it does not follow that we are forced to lapse into anarchism. Of course, if science is as lawless as Feyerabend claims, then counterinduction will suggest the introduction of the hypothesis that science should have a strict and rigid methodology. And, indeed, such a methodology does seem to have been useful for progress in the history and philosophy of science. Moreover, progress is possible within a strict and rigid methodology.

The next phase of Feyerabend's defence of counterinduction also involves similar difficulties. It is intended to show that any inductive generalization falls short of factual adequacy, that no theory can take account of all facts in its domain. In support of this claim a few examples of theories are pointed out: Copernicus' theory of the motion of the earth, which was in conflict with the plain facts of Copernicus' and Galileo's time; Newton's

theory of gravitation in which there are discrepancies; and Newton's theory of colours, which Newton had to support with auxiliary and ad hoc hypotheses.

Feyerabend, however, is right in his observation that the inductive generalizations which support the above mentioned theories fell short of factual adequacy. From this observation a claim is made to discredit all inductive generalizations. Feyerabend says: 'wherever we look, whenever we have a little patience and select our evidence in an unprejudiced manner, we find that theories [we may notice that Feyerabend is not talking about some theories] fail adequately to reproduce certain quantitative results, and that they are qualitatively incompetent to a surprising degree'.[30] Thus again he comes to a general claim of the nature of a principle; this contradicts his anarchism instead of supporting it.[31]

Feyerabend, however, contends here that scientific theories are to be accepted even though they fall short of factual adequacy; accepting such theories in spite of rigid and strict methodological rules would be counter-inductive. But this, as Carthoys and Suchting say, would 'be incompatible only with a methodology which made any inconsistency between theory and observation a sufficient condition for eliminating that theory. [But] It is at least very doubtful whether any theory of method (any

significant one at least) has contained such stringent standards'.[32] From this standpoint it may be said that Feyerabend's thesis of counterinduction is based on an unduly strong characterization of the prevalent methods of science. What in fact Feyerabend needs[33] is an argument which, though not presupposition-free, has minimal presuppositions and which will undermine any proposed methodological principle. Feyerabend might do this through a disjunctive elimination - but the onus then is proving that his initial disjunction is exhaustive.

There is, however, a different but more comprehensive approach to the defence of the thesis of counterinduction. This is an attempt to overthrow the underlying methodological demand of inductive generalization. Feyerabend holds that all methodological elements are contaminated by imperfections. It is contended that all observations, experiments, statements and results are influenced by our mental operations, by our understanding, ideologies and education - within which the methodological procedures are carried out. This is explained by a detailed discussion of how Galileo 'defused an important counter-argument against the idea of the motion of the earth'.[34] Galileo discusses and rejects the following argument which is presented against the Copernican view of the motion of the earth. If the earth moves on its

axis, and if a rock is dropped from the top of a tower then the rock should not fall at the bottom of the tower; it should fall away from the tower at the distance which the earth moves during the fall of the rock. But the rock falls at the bottom of the tower. Therefore, apparently, the earth does not move.

Galileo's reply is based on the distinction between sense-experience and the real facts which underlie such experience. Such a distinction is comprehended by the rational interpretation of sense-experience. The rational intervention takes place in the form of influences on sense-experience as exerted by linguistic expression, comparison, learning, imagination, ideologies, etc. Feyerabend says that sense-experience can be distinguished from these mental operations, though it is difficult to separate them.[35] We can distinguish our sense-experience from our mental operations, but it will be difficult to separate them from each other. 'Considering the origin and the effect of such operations, I shall call them natural interpretations'.[36]

From the fact that, when a rock is dropped from the top of a tower, it falls at the bottom, it appears that the earth does not move. Feyerabend also refers to another observation of Galileo's: a person walking along a street at night may observe that he is being followed

by the moon.[37] But Galileo's interpretations, that the earth does move and that the moon does not follow a person, are possible because, " 'reason intervenes'; [each of] the statement[s] suggested by the impression is examined, and one considers other statements in its place. The nature of the impression is not changed a bit by this activity".[38] Such interpretations of Galileo's are called by Feyerabend natural interpretations. Within Feyerabend's framework, we can call them rational - because they would not be possible 'if reason did not intervene'.[39] 'Galileo identifies the natural interpretations which are inconsistent with Copernicus and replaces them by others'.[40] These 'other' natural interpretations are called by Feyerabend the 'new natural interpretations' and are predominantly characterized by the intervention of reason.[41]

Natural interpretations, according to Feyerabend, may be responsible for mistakes in scientific theories, and for contradiction between our sense-experience and scientific theories. Though Feyerabend does not explicitly make natural interpretations responsible for this, yet this view is clear from the way he connects natural interpretations with appearance and with scientific theories. We already have an idea of the limitations of natural interpretation: it involves the intervention of mental operations, and consequently produce a difference between

the real facts and our sense-experience of them. In fact Feyerabend connects natural interpretation with appearance in the following way: 'Confirming the reality or revealing the fallacy of appearances means, however, examining the validity of those natural interpretations which are so intimately connected with the appearances... .'.[42] On the other hand an analysis of Galileo's defence of Copernicus' theory reveals that natural interpretation is closely related with scientific theory. It is due to natural interpretation that the pre-Copernican view regarding the lack of motion of the earth is right for the pre-Copernicans, and wrong for the Copernicans; and it is the discovery of a new (alternative) natural interpretation that gives a defence to the Copernican theory. The role of natural interpretation is clear from the following comment: 'it should be clear that a person who faces a perceptual field without a single natural interpretation at his disposal would be <u>completely disoriented</u>, he could not even <u>start</u> the business of science'.[43] In other words, the very business of science is considered to be intimately dependent on natural interpretation.

This view, that natural interpretation may be responsible for mistakes in scientific theories or contradictions between such theories and observation, is also clear from Feyerabend's discussion of Galileo's defence

of Copernicus' theory. Copernicus' view about the motion of the earth is true, though it contradicts supposedly plain and obvious facts. But now the question of what the real facts are becomes important. This is because, Galileo may be seen to have uncovered real facts by the reinterpretation of apparent facts. Whether this is so or not, we can put forward the following argument.[44]

If Galileo's procedure is to distinguish apparent facts from the real ones, then there is no contradiction between Copernicus' theory and the facts. So, we do not have a clear case of counterinduction, for counterinduction urges the adoption of those theories in conflict with the facts. On the other hand if we have a real case of counter-induction, then there is a clash between Copernicus' theory and the facts, and Galileo's procedure is not to uncover the facts from deceptive appearances clothed in natural interpretations. But in this case, Feyerabend's account of what Galileo is up to seems wrong, and that what Galileo was really up to,on this view, was inventing a sort of mythology by which the facts can be reinterpreted. What this shows, really, is that Feyerabend has defined counterinduction too strongly. One may think that Feyerabend has done so for a shock appeal and in the hope of establishing 'anything goes' by its means.

Feyerabend, however, tries to keep the examples of

counterinduction by giving it a somewhat more restrained characterization than the initial one. He does this by examining the difficulty of Copernicanism which is attributed to natural interpretation. It is said that the difficulty may be 'due to the presence of natural interpretations which are in need of improvement. The first task, [it is suggested] then, is to discover and to isolate these unexamined obstacles to progress'.[45] Feyerabend holds that Galileo does this by replacing one natural interpretation by an at least partly unnatural one; by this is meant that Galileo uses 'propaganda', 'psychological tricks' and his 'fertile imagination' in his scientific procedure.[46] In his attempt to explain how natural interpretation could be discovered, Feyerabend pursues the above discussed Copernican problem, and gives the following argument.

> We can now turn the argument around and use it as a detecting device that helps us to discover the natural interpretations which exclude the motion of the earth. Turning the argument around, we first assert the motion of the earth and then inquire what changes will remove the contradiction. Such an inquiry may take considerable time, and there is a good sense in which it is not finished even today. The contradiction, therefore, may stay with us for decades or even centuries. Still, it must be upheld until we have finished our examination or else the examination, the attempt to discover the antediluvian components of our knowledge, cannot even start. ... Ideological ingredients of our knowledge and, more especially, of our observations,

> are discovered with the help of theories which are refuted by them. They are discovered counterinductively.[47]

I shall divide this process into the following steps.

(1) Let us consider a well-established fact F which is taken to entail that the earth does not move, i.e. the theory T1.

(2) Let us suppose that Copernicus' theory of the motion of the earth is T2.

(3) We will assert T2 and then inquire what changes will remove the contradiction between T2 and F.

(4) We will thus discover what Feyerabend calls the antediluvian components of our knowledge.

(5) This is a counterinductive discovery.

While this is a more reasonable account of counterinduction, it offers very little support for Feyerabend's anarchism as will be clear in the following. I shall make the following notes on this account.

First, by the 'antediluvian components' of our knowledge, Feyerabend means those elements which influence our knowledge, viz., our ideologies, past education, way of perception, or to put it in a single phrase, our natural interpretations. But it may be noticed here that though Feyerabend gives this explanation by taking Galileo's case as a paradigm, yet Galileo actually did not work to find out the natural interpretation of F, but rather only to make T2 defensible.

Secondly, it may be noticed in connection with the

third step that when the changes are made, what is achieved is a change in the interpretation of facts. The change in the interpretation of a fact does not change the fact - it changes the way we understand it. The facts are the hard data. It may be asked though whether there really are any hard data. But, as Feyerabend says, what we change is the natural interpretation and not the facts; and then to that extent, facts are hard. On the other hand, all data may not be equally cognisible to us, and everything that we call data will not be equally hard. But some will be harder than others. And this is an anti-anarchist view.

Thirdly, in contrast to the well-established fact F, we have put forward T2. We then inquire what will remove the contradiction between F and T2. It may be asked: why have we put forward just T2, and not anything else? Did anything compel us to take T2 and not anything else? If yes, then Feyerabend's main thesis that anything goes does not go.[48] Feyerabend, however, will reply that anything might be used instead of T2, in fact anything, even 'the ramblings of madmen'.[49] But this is not an acceptable view. For this would be self-defeating for Feyerabend's position: there would be no point in his discussion, and he could not present Galileo's theory as an instance of scientific progress. What Feyerabend might

suggest here is that there are no absolutely fixed parameters in science. But even if we accept this suggestion, it does not follow that anything, including the ramblings of madmen, goes.

Moreover, when Feyerabend is trying to see what goes and what does not, at least among natural interpretations, is it not a bit absurd to say in the same context that anything goes - even the ramblings of madmen? But of course it might be said that we can be inspired even by the ramblings of a madman. But this only shows that inspiration may be non-rational. Thus there is a sense in which the ramblings of madmen may advance science - by inspiring us - and not directly. Moreover, there is a difference between the actual inspiration and the theory which it inspires. Feyerabend's view suggests that <u>anything</u> might inspire a theory and <u>in that sense</u> might go; but it is not the case that <u>any theory itself</u> goes, because theories inspired by lunatic ramblings are subject to test as any others.

Next, we deal with Feyerabend's claim that the contradiction between a well-established theory and a new theory may continue for centuries. This means that we may not be able to refute an old theory, and to establish a new theory. Feyerabend presents this claim in support of his thesis of counterinduction. But Feyerabend does not

as yet show why we may not be able to resolve the contradictions, and thus leaves his position incomplete and to this extent unconvincing. And indeed the Galileo case seems to be an excellent example of such a resolution. To show that anything goes, we would have to show that such contradictions could never be resolved. But it seems equally clear that they sometimes are resolved.

It may be objected that it is not in this way, i.e. by appealing to our failure to resolve contradictions, that Feyerabend presents his principle. But we may reply that the above discussed thesis of counterinduction has been designed by Feyerabend precisely to defend his thesis that anything goes. This claim is presented at the end of the first chapter of Against Method (p.28). And in the second chapter it is argued that this principle can be examined in concrete detail by proceeding counterinductively. It is thus clear that the principle that anything goes is defended by proceeding counterinductively; and we have just seen that the principle of counterinduction is put forward by suggesting a certain degree of difficulty in resolving contradictions between old and new theories. But this move of Feyerabend, as I have said in the preceding paragraph, remains incomplete and unconvincing.

In concluding this section we may hod that

Feyerabend's claim of counterinduction is a thesis which contradicts itself.[50] Moreover, his attempt to show, by explaining Galileo's defence of Copernicanism, as to how counterinduction takes place also remains unconvincing. Consequently, as it should follow, Feyerabend's thesis of counterinduction proves itself of little use as a defence of his anarchism.

§4.3: The Consistency Condition and Scientific Theories.

Feyerabend's critique of the consistency condition is a part of his defence of the thesis of counterinduction. The anarchist's purpose is to undermine the consistency condition; if this can be done, then, he hopes, anarchism is further strenthened. I shall evaluate Feyerabend's position in this regard in the following.

According to the consistency condition for scientific theories (as Feyerabend presents it) any new hypothesis, to be acceptable, must be consistent with the already accepted theory.[51] The consistency condition demands that any proposed alternative to a theory must be rejected if this alternative is inconsistent with the already accepted well-established theory. It is possible that the alternative theory is supported by factual evidence; but the consistency condition will undermine such evidence.

And moreover, in the case of two equally adequate theories, the older theory is to be accepted.

No one has ever used the consistency condition in this form, and Feyerabend certainly does not cite anyone. Feyerabend is really attacking a strawman here. On the other hand if this form of the consistency condition is rejected, it does not follow that we are led to anarchism. According to Feyerabend, a new theory should be accepted even if it is inconsistent with the older theories. It does not follow that in this situation any new theory is acceptable. Moreover, Feyerabend has not yet shown that any position can be considered actually to be a theory. Thus, though the demand of this form of the consistency condition is waived, yet it does not help Feyerabend's case.

Feyerabend next observes that if any inconsistency is found in an already accepted well-established theory then the defenders of the consistency condition will attempt to make it consistent with the facts. According to Feyerabend: 'The only real improvement, so the defenders of the consistency condition will continue, derives from the addition of new facts. Such new facts will either support the current theories, or they will force us to modify them by indicating precisely where they go wrong'.[52]

In the first place, however, we can say that there

may not be anything wrong in pursuing the consistency condition in this form. This may be made clear in the following way. Why should an attempt to preserve a theory, correcting precisely where it goes wrong, be objectionable *per se*? Let us take the example of the fact *F* which suggests *T1*, the theory that the earth does not move. The fact *F*, viz., that a rock dropped from the top of a tower falls at its bottom, is a true fact. It is a part of what we have called the hard data. On the other hand it is also a fact that the earth moves, and the theory which we get in this regard is *T2*. The fact *F* suggests *T1*, and contradicts *T2*. Those who take the fact *F* seriously may decide to take account of other facts and to correct their interpretation of *F* with the help of these new facts. This should be considered as a flexible and undogmatic procedure. And Feyerabend is in favour of flexibility in science. It should be noted here that the defenders of an old theory, by following this procedure, may in fact arrive at a new theory. Feyerabend does not consider this possibility, and therefore, his criticism of the defenders of the consistency condition seems unsatisfactory.

On the other hand, let us ask: is it a fact that the earth moves? If not, how could *T2* be true? If it is a fact that the earth moves, and if it is also a fact that a rock dropped from the top of a tower falls at its bottom,

then surely they seem to conflict, and therefore there seems to be a difference in the nature of these two facts. Both are true, but apparently one contradicts another. This reminds us of the earlier point that all data may not be equally hard. Given this situation, it is not unlikely that a scientist may rigidly stick to hard data, and may refuse to recognize an alternative theory. The hard data may appear to be more forceful and convincing to a scientist who may deny anything which is inconsistent with the hard data. In the controversy regarding the motion of the earth, the hard data seem to have been against Galileo (though Galileo resolved the contradiction by reinterpretation of the data). It is in such a case that Feyerabend can criticize the defenders of the consistency condition. But Feyerabend does not make all these points clear, and misses the point that sometimes the defenders of the consistency condition may be right in adding new facts and in modifying their theory. In fact, Feyerabend gets his point from the ambiguity of his use of the word 'fact'.

It is only regarding those defenders of the consistency condition who refuse to recognize any alternative new theory, that Feyerabend may say: 'the condition may finally create a situation where a certain point of view petrifies into dogma by being, in the name of experience,

completely removed from any conceivable criticism'.[53] Feyerabend's position is right in rejecting this. It is not at all right that any theory must be dogmatically preserved for any length of time in the face of unexplained counterevidence. Any such rigid claim must be rejected on the basis of the findings of the preceding chapters of this thesis. We have seen that the denial of the possibility of counterevidence leads to dogmatism. And wherever we find counterevidence to a certain claim, the claim may be reversed. But what is wrong in Feyerabend's position is that he has over-reacted against dogmatism. In order to restrict dogmatism it is by no means necessary that <u>any</u> scientific theory should be acceptable. On the other hand, if anything goes, then a dogma can also go. Feyerabend's over-reaction to the consistency condition thus leads him to a paradoxical situation.

Lastly, we may raise the question: if this form of the consistency condition is rejected, does it follow that anything goes? If we reject it, as Feyerabend suggests we should, then new theories are to be accepted in spite of a certain degree of factual inadequacy rather than preserve the old ones. It is in this sense that sometimes theories can be established if they are allowed a 'breathing space' (see below pp. 220-21). But, as I have said earlier in this section, again, it does not follow that

anything can go in the name of a new theory nor that any new theory is as good as any other. However, I shall now examine the next phase of Feyerabend's anarchism.

## §4.4: The Alleged Similarity Between Science and Witchcraft.

From Feyerabend's views on the consistency condition about how theories are preserved, a sharp turn is made to claim that scientific theories are almost indistinguishable from the myth of witchcraft. Immediately after explaining the above view on the consistency condition, and on the preservation of theories, it is claimed that:

> At this point an 'empirical' theory of the kind described ... becomes almost indistinguishable from a second-rate myth. In order to realize this, we need only consider a myth such as the myth of witchcraft and of demonic possession that was developed by Roman Catholic theologians and that dominated 15th-, 16th- and 17th-century thought on the European continent.[54]

This claim that empirical theories are almost indistinguishable from the myth of witchcraft is vague, inaccurate and misleading. The vagueness is partially due to the vagueness of the phrase 'almost indistinguishable'. What is meant by 'almost indistinguishable'? In what sense and to what extent are empirical theories and witchcraft supposed to be the same? I shall give an analysis of these

points regarding Feyerabend's position on science and witchcraft. For brevity, I shall take the phrase 'almost indistinguishable' in a weaker sense of similarity. In fact the difference between 'almost indistinguishable' and 'similar' may be crucial for Feyerabend's case. But if Feyerabend fails to establish the weaker claim that science and witchcraft are similar, then we can reject his stronger claim that they are 'almost indistinguishable'.

However, the main reason why empirical scientific theories are considered similar to myths is that they are, as Feyerabend argues, developed and preserved in similar ways. It is claimed that a myth 'is enforced by fear, prejudice and ignorance as well as by a jealous and cruel priesthood'; that it is supported by 'auxiliary hypotheses designed to cover special cases' and thus achieves observational support; and that it is made able to explain 'any conceivable event - conceivable, that is, for those who have accepted it'.[55] Similarly, it is argued, theories also penetrate the whole life of community, and preserve the status quo of intellectual life; and consequently, the result obtained is an 'absolute conformism'.[56]

Feyerabend seems to involve here a crude fallacy.[57] Two claims or views or standpoints may be enforced in the similar way, but it does not follow that they are similar. This can be made clear with an example. Let us consider

the claim that one form of entertainment is more popular than another, and the claim that nuclear power is safe. They are not similar claims, though both may be propagated or enforced by propaganda. But it does not follow that these two claims are similar, let alone almost indistinguishable. Thus we see that two claims enforced in the same way may not be similar; and that, even if they are similar, they may not be almost indistinguishable.

In the above I have quoted Feyerabend's view that a myth can explain any event conceivable to those who accept it. This is perhaps not true; for, it is not clear that witchcraft can explain everything, e.g. not the pronouncements of the Pope. The same, of course, is also true of science. For example, Galileo's theory of the motion of the earth cannot explain the origin and the development of the universe. But, nonetheless, scientific theories do, whereas witchcraft does not, have well-defined areas of application. That witchcraft lacks clear boundaries to its area of applicability does not, of course, imply that it has no such boundaries. Scientific theories, by contrast, seem to have fairly clearly defined areas of application, even where these areas are very large.

It is, however, true that in the 15th-, 16th- and 17th-century Europe, people at large conformed to the prevalent myths of that time. Conformism at that time, as the

context and background of witchcraft suggest, was based on mere faith, a dogmatic and uncritical consideration of the myths. And this indicates the epistemological framework within which conformity to the myth of witchcraft took place. But are also scientific theories conformist in the same way? In the first place, whereas people at large are involved in conformity to the myth,[58] only the scientific community is involved in conformity to the scientific theories; conformity of people in general to science is conformity to applied science or techonology, and not to particular scientific theories. But it is clear from the above discussion that the distinction between the conforming communities is overlooked by Feyerabend.[59]

Next, from the epistemological standpoint, conformity of the community to the myth may pertain to mere blind faith; but the conformity of the scientific community does not necessarily have to be based on faith, nor is there evidence that they all are so based. When a community conforms to the myth of witchcraft, it is more likely that the conformity is based on fear and faith in some supernatural power, belief in some authority and surely not on critical analysis. On the other hand, when the scientific community conforms to a scientific theory, it is predominantly based on critical analysis, observation, experiments, etc. - and usually not on fear or supernatural

power. There is a great difference between these two types of conformity.

But even if it is conceded that conformity to science is based on mere faith and uncritical consideration, will this be dogmatic? Conformity may be based on mere faith and uncritical consideration of scientific theories, but it need not be dogmatic, for it may just be a passive epistemological state; and this is not dogmatic for dogmatism involves an active state of commitment to a view together with a failure to consider the evidence. When this passive state of mere faith and uncritical consideration is overcome, conformity may either be a dogmatic conviction, or a critical consideration. If on the other hand scientific conformity takes place on the basis of an ad hoc hypothesis then that does not mean that the conformity is uncritical; for an ad hoc hypothesis need not be based on uncritical consideration. I thus suggest that the conformity of the scientific community to a scientific theory may be different in different instances, but not necessarily dogmatic or uncritical.

The importance of the epistemic context in discovering the difference between science and witchcraft can be elaborated as follows. We may make a distinction between (a) the conformity to witchcraft and, (b) the consensus about a scientific theory. Whereas the former

is not based on epistemic and rational grounds, the latter, which is often associated with science, is based on epistemic and rational grounds. Feyerabend may, however, claim that there is no distinction between rational grounds on the one hand, and irrational grounds on the other. But this claim cannot be used to support his anarchism, for Feyerabend's anarchism is supposed to establish this claim.

On the basis of the above discussion I would hold that scientific conformity may not be the same as conformity in witchcraft. I would further say that this difference is due to the difference of the epistemology on which is based the conformity. Thus mythical conformism will be different from scientific conformism both in regard to their form and in regard to their content. What is common between myths and scientific theories, to state it in Feyerabend's words, is only that 'they tend to preserve the status quo of intellectual life'.[60] But this is only a partial and superficial similarity. This is not enough to support the claim that science and myth are 'almost indistinguishable'.

The sort of similarity that is required by Feyerabend's claim should involve not only similarity of social and historical setting, but also similarity of content, methodology and structure. The myth of witchcraft and Galileo's theory about the motion of the earth are

different from the methodological standpoint. The dominant feature of the methodological aspect of witchcraft is the cynical manipulation of evidence, strictly speaking pseudo-evidence.[61] For example, if someone confessed under torture to hearing a witch then he was a witch; if he did not then he was also a witch because the devil must be helping him overcome the pain. Such evidence was used in witchcraft without any pretence at corroboration. In contrast, Galileo's theory is the product of experiments, observation, critical analysis, etc. (see below pp.212-215). But contrast this with the case for witchcraft:

> All the evidence makes it clear that the new mythology owes its system entirely to the inquisitors themselves. Just as anti-semites build up, out of disconnected tidbits of scandal, their systematic mythology of ritual murder, poisoned wells and the world-wide conspiracy of the Elders of Zion, so the Hammerers of Witches built up their systematic mythology of Satan's kingdom and Satan's accomplices out of the mental rubbish of peasant credulity and feminine hysteria; and the one mythology, like the other, once launched, acquired a momentum of its own.[62]

Feyerabend refers to the author of this passage in support of his claim of the science-witchcraft similarity.[63] But if the author of this passage is right then Feyerabend is wrong in his claim.

The difference between myths and scientific theories may also be raised from the standpoint of the

closure rules.[64] All myths seem to be closed under the authority of the source book (not necessarily written) for the myth. But scientific theories are not closed in this way. For example, all the claims which Einstein made about the theory of relativity are not necessarily a part of that theory; for Einstein could have made mistakes in his claims. But anything that is claimed by the source book is part of the myth. And this shows one important way in which myths are different from scientific theories.

I have tried to show in this section that the alleged similarity between science and witchcraft is much weaker than Feyerabend supposes. Not only are they different in regard to intellectual procedures, content and subject-matter, but also in their political and social roles, as well as in their internal political and social structures. Isolated examples may be cited (though Feyerabend cites none) in which witch-hunters act like scientists or scientists like witch-hunters. But such isolated cases prove nothing about the similarities of witchcraft and science per se. So, far from concluding that witchcraft and science are almost indistinguishable, there is little reason to suppose that they are even similar in form and function.

§4.5: Science and Rationality.

Feyerabend next attempts to strengthen his case for anarchism by arguing that science involves irrationality as much as it involves rationality. The point is that the presence of irrationality in science sanctions us to have recourse to anything in our scientific practice - even prejudices and personal idiosyncrasies.

Feyerabend asserts in this connection that Galileo's scientific practice involves propaganda, trickery, i.e. irrational factors; an attempt is made to show that the presence of irrational factors constitutes one aspect of science as we know it. Feyerabend sums up his view of science as follows: "[S]cience is a complex and heterogeneous historical process which contains vague and incoherent anticipations of future ideologies side by side with highly sophisticated theoretical systems and ancient and petrified forms of thought... . Many of the conflicts and contradictions which occur in science are due to this heterogeneity of the material, to this 'unevenness' of the historical development".[65] In support of the claim regarding the heterogeneous nature of science, Feyerabend explains how Copernicus' and Galileo's views developed in contrast with the earlier (Aristotelian) views. I shall present here a summary of Feyerabend's account.

Feyerabend observes that Aristotle's views on

motion, observation and perception make a coherent system, a form of naive realism: 'Astronomy, physics, psychology, epistemology - all these disciplines collaborate in the Aristotelian philosophy to create a system that is coherent, rational and in agreement with the results of observation'.[66] On the other hand, Galileo's pursuance of Copernicus' position is different. Unlike Aristotle's naive realism, Galileo depends on 'crude lenses' in an imperfect telescope for his scientific observations. And the results which Galileo gets are completely different from those suggested by the Aristotelian system. Regarding the tension between the coherent and well-developed Aristotelian system, and the newly developing Copernican-Galilean position, Feyerabend says: 'what is needed for a test of Copernicus [and Galileo] is an entirely new world view containing a new view of man and of his capacities of knowing'.[67] Regarding these new views it is further claimed that:

> the new view is quite arbitrarily separated from those data that supported its predecessor and is made more 'metaphysical': a new period in the history of science commences with a backward movement that returns us to an earlier stage where theories were more vague and had smaller empirical content.[68]

Two points are emphasized here: first, history is heterogeneous and uneven; coherent and well-established views

are contradicted or replaced by new views which are not fully developed. Secondly, the new scientific views required a new view about the world, man and knowledge. A question is raised regarding how the newly developed hypotheses can be established against the conflicting but 'well-developed', 'sophisticated' and 'successful' systems. The answer that is put forward is that: 'allegiance to the new ideas will have to be brought about ... by irrational means such as propaganda, emotion, ad hoc hypotheses, and appeal to prejudices of all kinds'.[69]

Feyerabend comes to this conclusion by discussing how Galileo introduces a new view, and how he uses propaganda, ad hoc hypotheses, etc. Galileo establishes new views in science and thus advances science by means of what Feyerabend considers the 'essence of Galileo's trickery'.[70] Galileo's case is made here a paradigm case. But what Feyerabend has not shown is why the methodological procedure of Galileo has to be taken as the general methodological procedure of scientific practice.[71] Moreover, such attempts to make Galileo's case a general scientific practice will not be compatible with Feyerabend's own thesis of counterinduction, where he rejects such generalizations.

Feyerabend takes Galileo's case as a paradigm case. But is Feyerabend's account of Galileo right? If not, then

Feyerabend's whole account becomes void. In support of his claim that Galileo's scientific procedure involves irrational elements, Feyerabend depends to a great extent on the historians of science. For example, in support of his claim, Feyerabend quotes the following passage from Stillman Drake.

> Galileo as a physicist treated inertial motions as rectilinear. Nevertheless, Galileo as a propagandist, when writing the Dialogue, stated that rectilinear motion cannot be perpetual, though circular motion may be. In the same book he ascribed some special properties almost metaphysically to circles and circular motions.[72]

About this passage, Feyerabend comments that 'All this, of course, fits in quite marvellously with the ideology of the present essay [Against Method]'.[73] Feyerabend, however, does not mention that the above passage gives only one aspect of Drake's account of Galileo's scientific procedure. Drake admits that Galileo uses propaganda, but also makes clear that this is not the main or primary characteristic of Galileo's scientific procedure. Drake holds that the passages in which Galileo uses propaganda, 'should be construed in the light of the purpose for which that book [Dialogue] was written. It was not written to teach physics or astronomy, but to weaken resistence to the Copernican theory'.[74] Galileo thus neither establishes nor argues for his theory through propaganda; he merely

propagates it by propaganda. And there is some difference between establishing, and arguing for a theory on the one hand, and propagating it on the other. One may establish a theory by experiments and proofs, and write arguments for it, but may never disclose or propagate it. Similarly, there is a marked difference between scientific research works and popular scientific works; whereas argumentation may not be fully rigorous in the latter, it may well be so in the former. And the fact that popular scientific works are not fully rigorous does not undermine scientific research works. Moreover, it is not uncommon or illicit that in seeking to persuade someone of an uncongenial position, a scientist may use considerations which he does not accept, but which the person he is seeking to persuade does. All these points suggest that there is a significant difference between rational justification of a theory and its propagation, and that Feyerabend's attempt to denigrate the former by pointing out the use of tricks and supposed dishonesty in the latter is a failure.

Drake duly emphasizes and points out the rational aspect of Galileo's scientific procedure. Thus Drake acknowledges that Galileo 'praised mathematical demonstrations as the only source of certainty'.[75] More important is that 'Galileo's rejection of authority as a substitute

for direct inquiry or observation had its counterpart in his respect for sensory evidence and his willingness to abide by the verdict of observation or experiment'.[76] We may further observe that, 'Galileo's physics was founded on his own actual measurements, which, through ingenuity and precision, led him to his law of falling bodies'.[77]

Feyerabend in fact accepts as the basis of his views only one part of Drake's account of Galileo. In turn, he has characterized all of Galileo's scientific procedure by this partial truth about it. And indeed, Feyerabend has based his claims on the less substantial part of Galileo's procedure, by suppressing the rational counterpart. Feyerabend's position is thus unsound and unacceptable.

On the other hand, even if we assume that Feyerabend's account of Galileo's procedure is right, it does not follow that any scientific theory will be acceptable. Let us assume that Feyerabend is right in his account of Galileo, and that irrational elements like tricks and propaganda were necessary for getting the Copernican theory accepted. But after the theory was accepted, it was held not on account of propaganda or tricks, but because of its value as a rational explanation of mechanical phenomena. This shows that propaganda and tricks provided only a 'breathing space' for this theory when it was first established. A

'breathing space' helped this theory to get accepted for the first time, but ever since it has gone on its own. And thus we may say that the irrational elements occupy only an auxiliary position in propagating a scientific position, but these elements cannot on their own make a scientific theory go.

A further point may be noted here. According to Feyerabend's account, the exponents of the old and well-established theories will reject new theories. Feyerabend suggests that irrational means are therefore necessary to establish the new theories. But the irrational means will also be unacceptable to the exponents of the old theory. Given this situation, a new theory is unlikely to be successful unless their exponents adopt rational means to weaken the resistence of the old theory. This observation further weakens the alleged importance of irrationality in science.

However, if we concede that science is like Galileo's scientific practice, and that Galileo's scientific practice involves emotion, propaganda, ad hoc hypotheses, etc., even then we may ask: why should the means such as emotion, propaganda, ad hoc hypotheses have to be necessarily irrational? Cannot the use of propaganda or emotion be compatible with rationality? On the other hand, if propaganda and emotion are irrational, and if science uses them

even then it does not follow that such uses will necessarily be irrational. What Feyerabend has done here is to treat the rationality or irrationality of science as the same as the rationality or irrationality of scientific propaganda. His purpose in doing so may be that if irrationality is admissible then any view can be accepted. It is in the similar vague manner that Feyerabend also confuses science and scientific chauvinism.[78] Science and scientific chauvinism should not be considered as the same; similarly the rationality or irrationality of science should not be considered as the same as that of scientific chauvinism.

We may hold that the sense of irrationality, in which Aristotle or Galileo are said to have been irrational, is the scientific sense and is acceptable. But does any theory go in this sense? At least the Aristotelian theory of the lack of motion of the earth does not go, when Galileo's theory does. What is true is that a view which is now irrational may become rational. But it is not true that any irrational view will become rational. And, therefore, somethings are not acceptable.

Lastly, we may observe that a distinction should be made here between the history of science on the one hand, and the philosophy and methodology of science on the other. The conflicts, contradictions and heterogeneity of science which Feyerabend alludes to are the parts of

the history of science. But they do not directly constitute the philosophy and methodology of science. Philosophy of science would study and analyse the structure and the nature of science. That the heterogeneous and the conflicting aspects of the history of science have no direct impact on the theoretical aspect of science is admitted by Feyerabend himself. He refers to, and accepts, a view that the unevenness of historical development has 'no immediate theoretical significance'[79] for science. This observation of Feyerabend, however, does not promote the irrationality of science, it rather conflicts with Feyerabend's own position.

## §4.6: Against Method.

Feyerabend's polemic against method is divided into two parts: (i) he presents concrete proposals against rigid methods of science, and (ii) he criticizes Imre Lakatos' methodological proposals. It is in this latter polemic that Feyerabend criticizes methodology and rationality together. I shall deal with the two issues in order.

We have already seen Feyerabend's dissatisfaction with induction. One may also cite as a further example (though Feyerabend does not cite it) that the method of verification cannot be satisfied. One may,however, reply

to this view by saying that the method of falsification, unlike other methods, does not require positive factual support for a theory, nor does it ignore the irrational aspects of scientific development. Therefore, one may wonder whether falsificationism could provide us with a satisfactory method. But we have seen in the preceding chapter that the method of falsification has also its difficulties. Feyerabend also rejects this method: 'The right method must not contain any rules that make us choose between theories on the basis of falsification. Rather, its rules must enable us to choose between theories which we have already tested and which are falsified'.[80]

Falsificationism presents a criterion for choosing a theory which is falsifiable though has not yet been actually falsified and as such is acceptable at least for the time being. But when Feyerabend suggests we 'choose between theories which we have already tested and which are falsified', he simply claims that a falsified theory cannot be rejected. And since we have agreed that falsification may not be final, we may make a wrong choice even if we follow Feyerabend's suggestion to choose from 'falsified' theories, unless we have some other criterion of choice. The need for a criterion in order to choose from competing falsified theories gets support from the

following observation of Feyerabend.

> ... the material which a scientist actually has at his disposal, his laws, his experimental results, his mathematical techniques, his epistemological prejudices, his attitude towards the absurd consequences of the theories which he accepts, is indeterminate in many ways, ambiguous, and never fully separated from the historical background. This material is always contaminated by principles which he does not know and which, if known, would be extremely hard to test.[81]

But if we are provided with any criterion, then that will contradict the principle that anything goes, because the criterion will not let those theories go which will fail to satisfy the criterion.

On the other hand it may be true that all methodological materials are contaminated; but this is the given which no one can alter. The point is not to alter this situation; nor is the point to hold a negativist attitude and to overthrow anarchistically the whole schema, just because what is given is contaminated. The point is: given contamination, how can we distinguish between what goes from what does not in that situation? Naturally, we need a criterion; and the fact that all materials of science are contaminated does not imply that it is impossible to have a methodological rule to choose from these materials.

Feyerabend does not address himself to this point at all, because he thinks that he has refuted the

presupposition which this question is based on, viz., that it is possible to establish a criterion to distinguish between what goes and what does not. But as we have seen, so far, Feyerabend has failed to establish that anything goes, or to establish that a methodological rule is impossible. And thus, so far he has failed to refute the presupposition on which the above question is based. On the other hand, his failure and his negativism are based on the assumption that we can never get a satisfactory methodological theory because we can never overcome the contamination that is involved in the methodological material. The point is: cannot we minimize the contamination even though we cannot be absolutely free from it?

Next, it is argued that Lakatos' views are not satisfactory, that they cannot avoid irrationality and consequently that anything goes. I shall briefly present those of Lakatos' views which are the focus of Feyerabend's discussion.

Lakatos' views may appear similar to Feyerabend's, for Lakatos also is willing to allow some breathing space to an inconsistent hypothesis or theory. This can be done in two ways: a new inconsistent theory awaiting its full development may be allowed to go; and an old theory in which inconsistency has been found may be allowed to go. This is because when a new theory is developed, pending

its full development, it may fail to explain many things which it is supposed to explain. It may be inconsistent with the fully developed theory. But if this new theory is given a chance to develop fully, then it may overcome its limitations and turn out to be satisfactory. This is because the inconsistency could be corrected; and our judgment that we consider something as an inconsistency may in fact itself be incorrect. From this standpoint, according to Lakatos, an inconsistent theory may be adopted. On the other hand, though this is not Lakatos' view, an old theory could also be given a breathing space to recover. Feyerabend will not approve such a view, for holding which he has already criticized the defenders of the consistency condition. However, Lakatos' view of giving a breathing space concerns the new theories only. For example, Lakatos approves of the fact that 'a breathing space [was given] both for the infinitesimal calculus and for naive set theory when they were bedevilled by logical paradoxes'.[82] It is also held that "All methodologies ... can be ... 'falsified' ".[83]

Lakatos holds that all methodologies can be falsified because, according to the falsifiability criterion, anything which is worth considering can be overthrown in principle. But it is not claimed that they all are actually false. Thus, according to this view, what can

be falsified can also be deductively tested more and more and thus can be improved until actually falsified. It is the issue of whether all methodologies are false or merely falsifiable that separates Feyerabend from Lakatos and enables Lakatos to go on to advocate improving methodologies.

It is this last point of Lakatos', that methodologies can be improved, that Feyerabend criticizes. Regarding Lakatos' views Feyerabend says that:

> if it is unwise to reject faulty theories the moment they are born because they might grow and improve, then it is also unwise to reject research programmes on a downward trend because they might recover and might attain unforeseen splendour... . Hence, one cannot rationally criticize a scientist who sticks to a degenerating programme and there is no rational way of showing that his actions are unreasonable.[84]

Feyerabend thus criticizes Lakatos' view not to give a breathing space to degenerating research programmes. But Feyerabend has missed[85] the point that, since a research programme will consist in many theories, the need to give a breathing space to a new theory does not imply the need to give the same to a new research programme. In the very beginning, perhaps, a research programme may consist in one theory only and thus may require a breathing space. But it does not, therefore, follow that an old and degenerating research programme should be given a breathing

space as well.

However, the above quoted passage indicates not only Feyerabend's anti-methodological and irrationalist view, but also a conflict in Feyerabend's own position. It is clearly claimed here that one cannot be criticized for sticking to a degenerating programme, where presumably well-established programmes degenerate. And that such degenerating programmes could be well-established is indicated by their comparison (in this passage) with new-born theories. But is not this defence of degenerating well-established programmes similar to maintaining the status quo which Feyerabend has been all along criticizing? Feyerabend is now arguing for what he himself has severely criticized. It is also said that, 'scientists must develop methods which permit them to retain their theories in the face of plain and unambiguously refuting facts'.[86] Is this not an inconsistency? Well, an anarchist does not have to worry about consistency; but that does not show either that anarchism is acceptable.

Moreover, the statement that 'one cannot rationally criticize a scientist who sticks to a degenerating programme and there is no rational way of showing that his actions are unreasonable' implies that a scientist can stick to a degenerating research programme as long as he wants. And, therefore, one cannot be rationally criticized

as long as one wants to act in a particular way.[87] This would seem to be a plea for the eternal entrenchment of theories. Even if we agree with Feyerabend that no theory may finally and irrevocably be rejected, he still owes us an account of what constitutes a problem for a theory. For without some such an account it seems very little can be made of scientific progress which would seem otherwise to be merely a happy accident.

Lakatos' position regarding this problem is that one 'may rationally stick tc a degenerating programme until it is overtaken by a rival and even after'.[88] Decision-making will certainly play some role in such rejection of degenerating programmes. Lakatos' position does not contain any definite guide about how to make such a decision. Lakatos is against giving any 'firm heuristic advice about what to do'.[89] From this lack of rule and guidance as to what to do, Feyerabend derives the conclusion that: "Reason as defined by Lakatos does not directly guide the actions of the scientist. Given this reason and nothing else, 'anything goes' ".[90]

But does the absence of rules and direct guidance necessarily imply that anything goes? In the first place, the absence of rules does not necessarily imply complete freedom (though complete freedom implies absence of rules). Let us consider an imaginary situation that a scientist

is working on a scientific model, and that he is neither constrained nor guided by any rule or law whatsoever. He is simply working and investigating certain facts. Let us also suppose that he is not even aware of any rule or procedure. But can he do anything he wants? Or, can anything go in the name of his experiment? Not at all; simply because the very nature of the material on which he is working will determine his work in some particular ways and will deter him from doing some particular things. This is implied by Feyerabend's own view, though not without self-contradiction, viz., that to see 'whether a certain feature is necessary for science is to carry out a functional study of this feature'.[91] Thus even if there is no rule or law, one may not be completely free. But if there is no complete freedom then anything cannot go. Hence, the absence of rules does not necessarily imply that anything goes.

## §4.7: Progress and Proliferation.

So far we have seen different arguments which have been put forward as a defence of the anarchistic claim that anything goes. Most of these arguments are also reinforced on the ground of progress of science. I shall first make clear how progress is associated with some of

these arguments, and then I shall examine Feyerabend's position on progress.

We have seen above that the defenders of the consistency condition may not accept a new theory which is inconsistent with the already accepted well-established theories. In the event of any difficulty in the well-established theory, the defenders of the consistency condition may gather new facts: either to defend their theory, or to modify it. Regarding such defences of well-established theories, Feyerabend says that this 'will precipitate real progress'.[92]

In the defence of the thesis of counterinduction, it is suggested that well-established theories may be overthrown by ancient and absurd ideas; Voodoo is cited in this context. Feyerabend says that, "Progress was often achieved by a 'criticism from the past', of precisely the kind that is now dismissed".[93] The notion of progress is thus associated with the thesis of counterinduction. Similarly, a positive relation between irrationality and progress is suggested by saying that 'our chances to progress may be obstructed by our desire to be rational'.[94]

Apart from reinforcing different anarchistic arguments with the notion of progress, Feyerabend directly associates this notion with anarchism itself. It is thus said that 'theoretical anarchism is more humanitarian and more likely to encourage progress than its law-and-order

alternatives'.[95] Or, that 'The only principle that does not inhibit progress is: anything goes'.[96] In other words, what is meant is that progress in science will be achieved if any theory or view in science is allowed to go: 'Proliferation of theories is beneficial for science'.[97] A critic ironically comments: 'Hence let a million flowers bloom'.[98] We may also note that, "proliferation [is regarded] not just as an 'external catalyst' of progress ... but as an essential part of it".[99]

We may, however, inquire what Feyerabend means by progress. It is in this point that one weakness of his position lies. Feyerabend does not define what he means by progress. He says:

> Incidentally, it should be pointed out that my frequent use of such words as 'progress', 'advance', 'improvement', etc., does not mean that I claim to possess special knowledge about what is good and what is bad in the sciences.. . Everyone can read the terms in his own way... . And my thesis is that anarchism helps to achieve progress in any one of the senses one cares to choose.100

Since epistemological anarchism fails to give any view about what it means by progress, its claim to promote progress remains ambiguous and doubtful. However, the absurdity of Feyerabend's claim can be shown in another way.

We have seen that Feyerabend considers Copernicus' theory of the motion of the earth and Galileo's defence of

it as an improvement and therefore as progress upon the Aristotelian view. Galileo is praised for defending Copernicus. Feyerabend says that 'Galileo is to be applauded because he preferred protecting an interesting hypothesis to protecting a dull one'.[101] Praise for the Copernican view is again asserted in connection with progress. Feyerabend refers to the rationalistic view of progress (though he considers it a narrow view), and says: 'And note that progress is here defined as a rationalistic lover of science would define it, i.e. as entailing that Copernicus is better than Aristotle and Einstein better than Newton'.[102] According to this view, progress is made in science when we have a new theory which is more developed, which gives a better understanding of its subject-matter, and which does not make the mistakes made by its earlier alternative. This notion of progress definitely restricts the proliferation of theories; theories of only one type can make for progress - the better and more developed ones. Consequently, there must be some criterion for comparing the theories of science to determine which theory should make progress.

## §4.8: Conclusion.

To conclude this chapter, I shall reassert that epistemological anarchism is an utterly unacceptable

position. This position is based on mere propaganda, and the reasoning that has been used to defend it is equally unacceptable. Feyerabend may hold that, 'An [epistemological] anarchist is like an undercover agent who plays the game of Reason in order to undercut the authority of Reason'.[103] But the undercover agent has failed to understand that he has undercut anarchism itself.

## Notes: Chapter 4

[1]Carthoys,J. and Suchting,W., "Feyerabend's Discourse Against Method", Inquiry 20(1977), p.249.

[2]The term 'humanitarian' has been used by Feyerabend. What he means by 'humanitarian' is indicated in his Against Method, p. 20.

[3]Feyerabend, P., Against Method, pp. 27-28.

[4]This is H. Butterfield's view, quoted by Feyerabend (ibid., p. 17).

[5]Cf., ibid., pp. 18-19.

[6]Ibid., p. 19.

[7]Feyerabend explains and elaborates on these points later in his book, and a corresponding account of them will be given later in this chapter.

[8]Munévar, G., Radical Knowledge (Amersham: Avebury Publishing Company, 1981), p. 15.

[9]Ibid., p. 12.

[10]Cf. ibid., p. 31.

[11]Ibid., p. 58.

[12]Feyerabend, P., Against Method, pp. 17-18.

[13]This thesis that anything goes is also stated in his " 'Science' The Myth and its Role" (Inquiry 18[1975], p.179; and in "Consolations for the Specialist", in I.Lakatos and A.Musgrave ed., Criticism and the Growth of Knowledge (Cambridge: at the University Press, 1970), p.229. "Consolations for the Specialist" has been reprinted in

P. Feyerabend, Philosophical Papers (Cambridge: Cambridge University Press, 1981), vol. 2. References to this paper are given from I.Lakatos and A.Musgrave ed., Criticism and the Growth of Knowledge.

[14]Feyerabend, P., Against Method, p. 20.

[15]Ibid., pp. 21-22.

[16]Ibid., p. 22n.

[17]Ibid., p. 20, my italics.

[18]Ibid., p. 29.

[19]We may also note that 'It is one thing to envisage a hypothesis, to entertain a possible explanation as possible. It is another thing to assent to the explanation as probably or certainly correct. ... it is one thing to maintain that a multiplicity of conflicting theories ought to be envisaged as possible; quite another to claim that they could or should simultaneously be assented to as true' (H.Meynell, "Feyerabend's Method", Philosophical Quarterly 20 [1978], p. 246).

[20]Feyerabend, P., Against Method, p. 30.

[21]Ibid., p. 35.

[22]Ibid., p. 37.

[23]Ibid., p. 52. Feyerabend also says that 'There is no idea, however ancient and absurd that is not capable of improving our knowledge' (ibid., p. 47). Regarding this claim Ernest Nagel holds that, 'But since it is unclear from his discussion what he understands by "knowledge" and its "improvement" or whether he thinks that knowledge is ever achieved, it is difficult to say whether he is faithful to his anarchism in this pronouncement' (E.Nagel, "Review of Feyerabend's Against Method", The American Political Science Review 71 [1977], p.1133).

[24]Feyerabend, P., Against Method, p. 32, my italics.

[25]Ibid., p. 32.

[26]Meynell, H., "Feyerabend's Method", p. 245.

[27]Ibid., p. 245. As one instance of Feyerabend's use of methodological procedures, we may note that he does not 'object to the procedure of abstraction itself. But when abstracting from a particular feature of science we should make sure that science can exist without it, that an activity, not necessarily science, that lacks it, is (physically, historically, psychologically) possible' (P.Feyerabend, Against Method, p. 184n). Thus, he accepts the procedure of abstraction. Moreover, he elaborates on this methodology: 'the only way of finding out whether a certain feature is necessary for science is to carry out a functional study of this feature (in the sense of modern anthropology) that examines its role in the growth of science' (loc. cit.). Here Feyerabend is clearly contradicting himself and proposing a certain methodological procedure for science.

[28]Similarly P.K.Machamer holds that, "In characteristic overstatement Feyerabend says he wants to argue that the methodology of science is, and ought to be, 'Anything goes' " (P.K.Machamer, "Feyerabend and Galileo: The Interaction of Theories, and the Reinterpretation of Experience", Studies in History and Philosophy of Science 4 [1973-74], p. 3, my italics).

[29]For the proof and a detailed discussion, see N. Griffin, "There Can Be No Method", unpublished paper, (McMaster University, Canada, 1982).

[30]Feyerabend, P., Against Method, p. 64.

[31]Similar contradictions, as we will see, are abundent in Feyerabend's Against Method. Even a critic who thinks well of Feyerabend's work, admits that "Against Method is a good book, possibly a great one. [But] It is full of contradictions, over and understatements, and enough ad hominem statements to give even the most liberal student of rhetoric apoplexy" (I.I.Mitroff, "Review of Feyerabend, Against Method", Contemporary Sociology 5 [1976], p. 347).

[32]Carthoys, J. and Suchting, W., op. cit., p. 264.

[33]Private communication from Dr. N. Griffin.

[34]Feyerabend, P., Against Method, p. 70.

[35]Ibid., p. 73. A similar view is arrived at by Feyerabend which asserts that a distinction has to be made between the fact that one possesses a certain sensation, and the interpretation of the sentence being uttered at the presence of that sensation. (P.Feyerabend, "Explanation, Reduction, and Empiricism", in H.Feigl and G.Maxwell ed., Minnesota Studies in the Philosophy of Science [Minneapolis: University of Minnesota Press, 1962], vol. 3, p. 94; reprinted in P.Feyerabend, Philosophical Papers, vol.1).

[36]Feyerabend, P., Against Method, p. 73.

[37]Ibid., p. 71.

[38]Ibid., pp. 71-72.

[39]Ibid., p. 71.

[40]Ibid., p. 69.

[41]It may be noted here that there is some distinction between what we may understand by rational interpretation and what Feyerabend calls natural interpretation. It is true that, like natural interpretations, rational interpretations are also involved in the above mentioned mental operations. But the rational interpretation, as we normally understand it, does not contain irrational elements such as prejudices, tricks, etc. And when Feyerabend says that natural interpretations involve mental operations, he leaves room for irrational elements such as prejudices and tricks.

[42]Feyerabend, P., Against Method, p. 74.

[43]Ibid., p. 76.

[44]Private communication from Dr. N. Griffin.

[45]Feyerabend, P., Against Method, p. 75.

[46]Ibid., cf. p. 81.

[47]Ibid., p. 77.

[48]From a similar standpoint Rom Harré holds that, "Feyerabend's spectacular motto, 'anything goes' is not established by his Galileo example. Indeed, I think what follows from his historical examples is that not anything goes" ("Review of Feyerabend, Against Method", Mind 86 [1977], p. 297).

[49]Feyerabend, P., Against Method, p. 68.

[50]'In fact, the empiricism Feyerabend criticizes as incompatible with scientific progress, is simply duplicated in this test [Against Method], in a distorted form' (T.Counihan, "Epistemology and Science - Feyerabend and Lecourt", Economy and Society 5 [1976], p. 77). We see that though Feyerabend is against method, he himself uses method; and though he is against all rules, he advocates a rule-like thesis. Counihan thus further claims that, Feyerabend's 'anarchism is simply a weak-kneed attempt to evade criticism and is in turn a result of the contradictory character of this form of opposition to empiricism' (ibid., p. 77).

[51]Feyerabend, P., Against Method, cf., pp. 35ff. A similar view, which Feyerabend also rejects, is the following: only such theories are admissible for explanation and prediction in a given domain which either contain the theories already used in this domain, or are at least consistent with them (P. Feyerabend, "Explanation, Reduction, and Empiricism", p. 44).

[52]Feyerabend, P., Against Method, p. 37. This claim, of course, seems in conflict with Feyerabend's account of Galileo's success where the contradiction was successfully resolved. It is surely some form of the consistency condition which forces scientific progress. Had Galileo merely accepted the contradiction, that would never lead to the progress enhanced by Galileo.

[53]Feyerabend, P., Against Method, p. 42.

[54] Ibid., p. 44, my italics.

[55] Ibid., p. 44. One example of enforcing myth by cruel priesthood by using fear, is the 'witch-bishop' and his ten-year (1623-33) reign of terror (see H. R. Trevor-Roper, The European Witch-Craze of the 16th and 17th Centuries [Pelican, 1969], p. 84).

[56] Feyerabend, P., Against Method, p. 45.

[57] Private communication from Dr. N. Griffin.

[58] See, for example, H.R.Trevor-Roper (op. cit.,p.68) for the view that witch-craze was extent all over the 16th century Europe, and in everyday life of people involving even their literature and legal profession.

[59] Feyerabend's argument also seems to involve the confusion between similarities of two theories and two theories playing similar roles (private communication from Dr. N. Griffin). It is possible that two completely different theories may play the same social role; e.g. the theory of the evolution and the theory of special creation have the same role, among others, of explaining the origin and development of life. But they are two completely different theories. Feyerabend is confusing this distinction.

[60] Feyerabend, P., Against Method, p. 45.

[61] For instruments of witchcraft, see, F. M. Guazzo, Compendium Maleficarum, tr., E.A.Ashwin (Secaucus, New Jersy: University Books, 1974), p. 163. (First published in Latin in Milan, 1608).

[62] Trevor-Roper, H.R., The European Witch-Craze of the 16th and 17th Centuries, pp. 40-41, my italics.

[63] Feyerabend, P., Against Method, p. 44n.

[64] Private communication from Dr. N. Griffin.

[65] Feyerabend, P., Against Method, p. 146.

[66] Ibid., p. 149.

[67]Ibid., p. 152.

[68]Ibid., p. 153.

[69]Ibid., pp. 153-54.

[70]Ibid., p. 84.

[71]Curthoys and Suchting give a similar criticism in the context of Feyerabend's thesis of counterinduction. It is said that, 'Feyerabend gives no theoretical justification for counterinduction, and his historical strand rests ultimately on totally subjective considerations' (op.cit., p. 265). Curthoys and Suchting quote (loc.cit) Feyerabend in support of their claim. Feyerabend accepts Galileo's case because 'a new theory, like all new things, will give a feeling of freedom, excitement...' (Against Method, p.98); and this is a subjective consideration. It is true that, if not totally, to some extent Feyerabend's thesis is based on subjective considerations. It is thus further confirmed that Feyerabend's treatment of Galileo's case as a paradigm remains unsupported.

[72]Drake, S., Galileo Studies (Ann Arbor: The University of Michigan Press, 1970), p. 253; cf. P. Feyerabend, Against Method, p. 97n.

[73]Feyerabend, P., Against Method, p. 97n.

[74]Drake, S., Galileo Studies, p. 253.

[75]Ibid., p. 10.

[76]Ibid., p. 74.

[77]Drake, S., Galileo (Oxford University Press, 1980), p.vi.

[78]Feyerabend, P., Against Method, p. 50.

[79]Ibid., p. 146.

[80]Ibid., p. 66.

[81] Ibid., p. 66.

[82] Lakatos, I., "History of Science and its Rational Reconstructions", p. 113.

[83] Ibid., p. 115.

[84] Feyerabend, P., Against Method, p. 185. It may be noted here that this passage in exactly identical form is also found in another work of Feyerabend. See P. Feyerabend, "On the Critique of Scientific Reason", in C. Howson ed., Method and Appraisal in the Physical Sciences (Cambridge: Cambridge University Press, 1976), p. 322.

[85] Private communication from Dr. N. Griffin.

[86] Feyerabend, P.,"Consolations for the Specialist", p. 205.

[87] We have seen that Popperian falsification may not be final, and it may obstruct development of scientific theories by falsifying them the moment they are born. Lakatos' view to give theories a 'breathing space' is aimed against this Popperian view of falsificationism. Regarding this proposal of Lakatos, Feyerabend says that standards of 'this kind have practical force only if they are combined with a time limit... . But introduce the time limit and the argument against naive falsificationism reappears only with a minor modification...'. (P.Feyerabend, "Consolations for the Specialist", p. 215).

[88] Lakatos, I., "History of Science and its Rational Reconstructions", p. 104, "Note". This claim of Lakatos, however, seems to be an overstatement. We have seen above that Lakatos is in favour of improving methodologies; but if the present claim is true then improvement looses its significance.

[89] Ibid., p. 104, "Note". Feyerabend reads this view of Lakatos in the following way: 'the methodology of research programmes provides standards that aid the scientist in evaluating the historical situation in which he makes his decision; it does not contain rules that tell him what to do' (Against Method, p. 186).

[90]Feyerabend, P., Against Method, p. 186.

[91]Ibid., p. 184n.

[92]Ibid., p. 37.

[93]Ibid., pp. 48-49.

[94]Ibid., p. 156.

[95]Ibid., p. 17.

[96]Ibid., p. 23.

[97]Ibid., p. 35. 'Invent, and elaborate theories which are inconsistent with the accepted point of view, even if the latter should happen to be highly confirmed and generally accepted' (P.Feyerabend, "Reply to Criticism", in R.S.Cohen and M.W.Wartofsky ed., Boston Studies in the Philosophy of Science [Dordrecht:D.Reidel, 1965], vol. 2, pp.223-24). This paper has been reprinted in P.Feyerabend, Philosophical Papers, vol. 1.

[98]Gellner, E., "Beyond Truth and Falsehood", p. 333.

[99]Feyerabend, P., Against Method, p. 48, n.2.

[100]Ibid., p. 27.

[101]Ibid., p. 98.

[102]Ibid., p. 156.

[103]Ibid., pp. 32-33.

# Conclusion

In this thesis I have presented a defence of the empirical scientific knowledge against epistemological negativism. I have examined three negativist positions: scepticism, fallibilism and epistemological anarchism. We have seen that these negativist positions fail to undermine scientific knowledge and that we can obtain certainty, justification and knowledge in empirical sciences. In the following I shall first recapitulate the main points I have dicussed in the above chapters and then I shall emphasize the main positive findings of my investigation.

In the first chapter we have formulated the negative positions and distinguished between them. In the formulation of scepticism and epistemological anarchism, we have accomplished this task by considering their basic tenets as propounded by their prominent exponents. Scepticism rejects the possibility of knowledge, or more weakly, the existence of knowledge; epistemological anarchism rejects all methodological requirements of epistemology and science, and consequently leads to scepticism. In our attempt to formulate fallibilism, we have seen the deficiencies of a number of currently available definitions; we have, therefore, described an alternative which entails scepticism

about certainty though at the same time permiting the possibility of justification and knowledge. The possibility of mistake in one's belief renders one's belief uncertain; but the possibility of mistake in justification does not make justification impossible.

In the second chapter we have presented a thorough examination of a recent and rigorous sceptical case - presented by Peter Unger. We have seen that Unger's reformulation of the evil-demon argument for scepticism results in a completely new sceptical argument. An analysis of this argument has revealed that the argument fails to establish scepticism. Unger's next major argument, his argument for universal ignorance, is equally unsatisfactory. We have also discussed in detail whether or not we can be absolutely certain about empirical statements. We have seen that we can be absolutely certain about empirical statements, and without being dogmatic. Unger's rejection of certainty, and his view that unless we are sceptics we will be dogmatic, are both false. Lastly, Unger's claim that we shall be irrational in believing in anything is also unacceptable.

In the next chapter we have given a critical appraisal of Popper's fallibilist attempt to undermine empirical scientific knowledge. We have seen that Popper's fallibilism develops from two main factors, viz., (i) his dissatisfaction with the prevailing methods of science - the

method of verification and the method of induction; and (ii) his attempt to provide an alternative method, the method of falsification. But on the one hand falsification cannot be final, and on the other, the methods of falsification, deductive testing and corroboration involve salient features of induction which Popper rejects. Popper's fallibilist attempt to undermine justification of knowledge claims is also unsatisfactory. His view that we cannot know the truth is also wrong. We have seen that in spite of our fallibility which Popper emphasizes, we can obtain certainty, justification and the truth of empirical statements, and consequently, that we can obtain knowledge.

In the last chapter we have examined Feyerabend's epistemological anarchism. We have seen that Feyerabend's defence of his anarchistic ideas are unconvincing. It is not true that anything goes in science, or that anything can be done in the name of scientific investigation, or that anything can be believed or practised in science. Equally untrue is the claim that there is no difference between rationality and irrationality in science, and between science and mythology. His motto, 'against method' and his rejection of all methodological requirements are insufficiently supported. Lastly, we have seen that it is unlikely that the progress of science will be enhanced by the proliferation of the theories Feyerabend espouses.

Apart from these aspects of our investigation, there are more particular points where our investigation has either contributed a new insight or has significantly extended any insight already gained. Among these we may mention the following.

In the first place, if verificationism is accepted then (since its demands are so strong) it leads to scepticism about a large area of scientific knowledge; hence, verificationism is unacceptable in the first place. But it has been left unexplored how scientific knowledge could have defended itself against the scepticism which is likely to follow from verificationism. Thus one possible way of exploration and insight into scientific investigation has been missed. Amidst the wide practice of considering verificationism without considering defences against its sceptical consequences, Norman Malcolm is one of the exceptions. But Malcolm's attempt does not go very far, for he does not show how absolute certainty could be obtained without dogmatism. Keeping this perspective in mind, we have examined whether we can obtain certainty and rationality about the empirical statements which are at the very basis of scientific investigation. Following Odegard's resolution of this problem, we have shown that we can obtain absolute certainty about the empirical statements without being dogmatic.

We also need some method or procedure for obtaining absolute certainty. It is true that no method will be universally satisfactory; and I do not have any reason to claim that I have presented a universally satisfactory one. But I have at least presented the minimal necessary condition for such a method: the method of careful examination. The necessary condition which I have presented will also be sufficient at least in some cases. In accepting any method whatsoever, one must meet one minimum necessary condition: it must be a careful examination <u>in the sense explained in this thesis</u>. On the other hand, in examining any problem whatsoever, we must again satisfy one minimum necessary condition: it must be a careful examination <u>in the sense explained</u>. Apart from this necessary condition of carefulness, the other conditions on a method will be a matter of the particular fields of investigation. For instance, the procedure of investigation in physics will be different from that of medicine; but always and in all cases whatsoever, there must be a careful examination.

We have seen that by careful examination we can obtain absolute certainty in some cases, without being dogmatic, in the temporal sense. But we cannot get <u>atemporal</u> or <u>eternal</u> absolute certainty; but that is not a problem for us. Neither the empirical sciences, nor we in this thesis, are so ambitious as to strive for eternal, atemporal

certainty. It may be objected that the notion of careful examination cannot be applied to many problems, i.e. that there are many problems which we are unable to examine carefully, simply because science is unable to deal with many things. But again, neither the empirical sciences, nor our present investigation, are that ambitious. Our claim is modest: it is possible for us to examine carefully many problems, not all, and to obtain absolute certainty about them in a temporal context.

In consequence of these findings, we are able to say that it is possible for us to obtain justification for empirical scientific statements. If we can be absolutely certain about them, then we are justified in believing in them. In the same way, we have seen, we can get truth. If we can be absolutely certain that such and such is the case, then we are justified in believing the statement to that effect; to deny this would be a contradiction. As a result of these findings, we see that our beliefs about empirical scientific statements can be justified and true beliefs, i.e. we can obtain knowledge in the empirical sciences.

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