

**Feyerabend's Epistemological Anarchism and  
Values-Based Rationality**

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March 1998

A thesis submitted for the degree of Doctor of Philosophy  
of the Australian National University

The content of this thesis is entirely my own work

*Robert Fanell.*

This thesis is dedicated to my two girls: to Lucinda for emotional support throughout, and to Breanna, whose arrival made the writing of this thesis a unique and rewarding process.

My thanks go to Dr. C.B. Christensen for supervision, disagreements, and support in matters non-academic. Thanks are also due to Prof. F. Jackson for helpful comments on later drafts.

## Abstract

The content of this thesis is mainly negative, in that the first four chapters deal with rescuing Feyerabend's philosophy from misunderstandings. Specifically, it has been said that Feyerabend contends that there is no such thing as rationality: there is only power play, prejudice and propaganda. It has also been claimed that Feyerabend's support for epistemological anarchism should be interpreted literally: do what you like and defend what you like, because no theory is better than any other. As evidence for this claim various philosophers have contended that Feyerabend's incommensurability thesis implies that no two theories can be compared in any manner whatsoever. It has also been contended that Feyerabend's ideas constitute a radical, self-refuting, relativism.

I argue in chapters 1-4 that this interpretation of Feyerabend is wrong in all four claims. In particular, I have argued that Feyerabend is conducting a *reductio ad absurdum* of 'Rationalist' philosophies. Once we read Feyerabend with this in mind, attributions of irrationality and literal anarchism cannot be supported. Feyerabend's incommensurability thesis should also be read with his *reductio* in mind, with particular reference to logicist conceptions of theoretical comparison. That is, Feyerabend never denied that theories could be compared. And though Feyerabend was a relativist in many respects, his relativism was not of the radical, self-refuting, kind. In fact, Feyerabend's philosophy was highly realistic in many respects.

In chapters 5-7 I tie together the various strands of Feyerabend's philosophy to produce a positive conception of rationality. This positive conception of rationality is a historical, contextual and dynamic vision, crucially revolving around the ideas of value and plurality. In presenting this conception of rationality, the ideas of Kuhn and Dewey, with specific reference to values-based rationality, are incorporated into Feyerabend's account. This can be seen to be the minimum necessary addition to Feyerabend's ideas, producing a detailed and comprehensive heuristic program for the analysis and understanding of rationality.



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The following abbreviations are given for works cited in brackets in the text. Full bibliographical information can be found in the bibliography.

J. Dewey:

EEL - *Essays in Experimental Logic*

EN - *Experience and Nature*

QC - *The Quest for Certainty*

LTI - *Logic: The Theory of Inquiry*

P. Feyerabend:

AM - *Against Method*

SFS - *Science in a Free Society*

FTR - *Farewell to Reason*

K. Popper:

LSD - *The Logic of Scientific Discovery*

CR - *Conjectures and Refutations*

## Introduction

The misconstrual of Feyerabend's later philosophy has been ubiquitous. He has been attributed as ascribing to complete anarchism, universal relativism, the denial that there is anything that could be called rationality or reason and, consequently, the contention that the world of knowledge is completely dominated by power, propaganda, subjective feelings and prejudice. A lot of the words and terms that I just used are indeed used by Feyerabend when describing his own thoughts and writings; so it might seem paradoxical to assert, as I will in this thesis, that these terms, taken in their usual sense, do not describe Feyerabend's philosophy. The popular image of Feyerabend is a completely misleading one. I feel that it is based upon a too fleeting reading and that a more thorough reading of his writings produces a very different vision of Feyerabend's philosophy. This is not to say that the popular reading of Feyerabend is not understandable: his abrasive, irreverent and polemical style of writing in some of his works is not what is usually expected from the academic philosopher. All this has not encouraged sympathetic readings of Feyerabend. But I would go further and contend that philosophers of science do not like to give Feyerabend a sympathetic reading for this would deprive them of their most valuable straw-man. Feyerabend's views have become a symbolic demon that has to be overcome in a philosopher of science's initiation ceremony. It doesn't matter whether Feyerabend actually held the views criticised, if Feyerabend hadn't been there to fulfil the role, then a position would have had to have been created, with or without a putative spokesperson.

In this thesis I discuss and critically examine Feyerabend's philosophy. More specifically, I argue that Feyerabend is presenting a new conception of rationality. This conception of rationality should be seen as contrasting with the standard conception of rationality, a position I have labelled 'Rationalism', which is characterised by universality, a-temporality, complete objectivity and formal logic. Feyerabend rejects this account, instead seeing rationality as being inherently historical and flexible: ascriptions of rationality are dependent upon the context under consideration. Much of the

misunderstandings of Feyerabend's philosophy can be seen to arise from the perception that Feyerabend is attacking rationality absolutely, that he is not suggesting that it is only one conception of rationality which is being attacked, but that it is a universal condemnation of the very idea of rationality. Of course I will have to argue that this is not the case but I think it can be done. And I think I will be able to show that much of the criticism of Feyerabend can be attributed to the fact that many philosophers simply cannot, or could not, conceive of a different form of rationality other than 'Rationalism'. Thus any criticism of rationality, by Feyerabend or anyone, was an absolute criticism.

If I am to say that Feyerabend is in fact a rationalist of some sort, then I must produce the version of rationality which Feyerabend would uphold, or should uphold. This is not an easy task, for Feyerabend's writings have been primarily destructive. But contrary to some commentators who have only seen the negative in Feyerabend, a closer look reveals that he does provide some suggestions as to what rationality would be in a non-absolute sense. These suggestions are somewhat sketchy and vague but do indicate the direction in which Feyerabend would see fruitful future research.

According to Feyerabend, the way in which rationality can be assessed in different contexts, is achieved with the help of two heuristic principles: tradition and plurality. It is my contention that if we examine these two principles and place them in the context of values-based judgements, then we are led to a vision of rationality wherein rationality is not something reducible to formal logic and unequivocal, but which still preserves rationality as something identifiable and prescriptive.

In chapter 1 I discuss Feyerabend's *reductio ad absurdum* of traditional interpretations of science. It is Feyerabend's contention that if we accept traditional interpretations of science, which incorporate 'Rationalist' conceptions of rationality, then we must label prominent episodes in the history of science as irrational. But this does not imply that Feyerabend himself believed science to be irrational. Feyerabend's use of words such as propaganda, irrational, subjective wishes, and so on, should be understood in terms of his *reductio* of 'Rationalism': as attributes of science only if we accept 'Rationalism'.

In chapter 2 I examine Feyerabend's putative anarchism. Feyerabend's 'anarchism' forms part of his *reductio*, that is, a 'Rationalist' must label any episode in science which does not conform to their standards of rationality as anarchistic: as not governed by the rules of rationality. However, this is not to say that Feyerabend's theory is anarchistic in any real sense. Feyerabend's anarchism amounts to the claim that there are no universal, atemporal rules of rationality. This type of 'anarchism' does not preclude an account of rationality which is prescriptive; it simply contends that prescription is itself contextual.

In chapter 3 I respond to the contention that Feyerabend believes scientific theories to be incommensurable. According to Feyerabend, certain types of theories are incommensurable when interpreted realistically, and in a manner consonant with 'Rationalist' standards of theoretical comparison. If we drop 'Rationalism' and its associated standards of theoretical comparison, then theories are, in fact, commensurable in many ways.

In chapter 4 I discuss the charge of relativism which is often levelled at Feyerabend. It cannot be denied that Feyerabend is a relativist, of sorts, but his relativism is not of the holistic self-defeating variety. More importantly, Feyerabend's relativism is compatible with a version of realism.

In chapter 5 I discuss the themes that have been emerging in the previous four chapters revolving around the ideas of epistemic values and values-based rationality as judgement within a particular context. I try to give a more systematic account of Feyerabend's thought on these issues, incorporating and contrasting them with the views of Kuhn and Laudan.

In Chapters 6 I discuss John Dewey's philosophy of value and rationality. Dewey's conception of value and rationality provides a needed superstructure within which to place Feyerabend's ideas. With Dewey's conception of high-level, general values of inquiry, Feyerabend's sketchy and scattered suggestions concerning an alternative conception of rationality can be placed within a more systematic theoretical context.

In Chapter 7 I bring together the various strands of Feyerabend's positive philosophy and explore some of the ramifications of the alternate vision of rationality

which thus emerges. I hope to show that Feyerabend's views concerning scientific rationality are not limited to science: they point towards a general vision of rationality, one which breaks down the traditional distinction between theoretical and practical reason, but which can still account for the idiosyncrasies of individual disciplines.

# 1 Feyerabend's *Reductio ad Absurdum* of 'Rationalist' Philosophies

## 1.1 Introduction

Paul Feyerabend begins his notorious book *Against Method* in the following manner:

The following essay 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*.

The reason is not difficult to find.

'History generally, and the history of revolutions in particular, is always richer in content, more varied, more many-sided, more lively and subtle than even' the best historians and the best methodologist can imagine. History is full of 'accidents and conjunctures and curious juxtapositions of events' and it demonstrates to us the 'complexity of human change and the unpredictable character of the ultimate consequences of any given act or decision of men'. Are we really to believe that the naive and simple-minded rules which methodologists take as their guide are capable of accounting for such a 'maze of interactions'? And is it not clear that successful *participation* in a process of this kind is possible only for the ruthless opportunist who is not tied to any particular philosophy and who adopts whatever procedure seems to fit the occasion? (AM, 17-18.)<sup>1</sup>

Feyerabend goes on to conclude that "a complex medium containing surprising and unforeseen developments demands complex procedures and defies analysis on the basis of rules which have been set up in advance and without regard to the ever-changing conditions of history". (AM, p. 18)

If we examine these passages carefully, then we will find contained within them aspects of Feyerabend's positive philosophy, and also what Feyerabend finds objectionable about philosophy and theories of rationality. Firstly, Feyerabend states that anarchism is 'excellent medicine' for epistemology and philosophy of science: epistemology and philosophy of science are sick and anarchism will remedy the situation. This way of expressing his thesis is highly important: once we are well we do not continue to take medicine. We can also see that a 'healthy' epistemology would be one which was complex rather than simple; multifarious rather than uniform; contextual

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<sup>1</sup> The first quote within this passage is a paraphrase of V.I. Lenin, "Left-Wing Communism - An Infantile Disorder" in *Selected Works*, Vol.3 p. 401. The further quotes are from H. Butterfield, *The Whig Interpretation of History*, p. 66.



rather than independent of context and universal; and responsive to the idiosyncrasies of history and particular situations, as opposed to unchangeable and atemporal.

We can see that Feyerabend is not contending that there is no such thing as rationality or reason. Rather, Feyerabend is contending that a particular way in which rationality has been conceived, a particular theory of rationality, does not fulfil the function for which it was designed: the complexity, vagaries and unpredictableness of human activity evades explication by way of the theory of rationality which Feyerabend is critical of. Consequently, Feyerabend proposes anarchism as the dialectical antithesis of the received view, not in order to replace that view, but so as to generate the debate which will create a new synthesis more sensitive to context and complexity. Feyerabend states that

My intention is not to replace one set of general rules by another such set: my intention is, rather, to convince the reader that *all methodologies, even the most obvious ones, have their limits*. The best way to show this is to demonstrate the limits and even the irrationality of some rules which she, or he, is likely to regard as basic ... Always remember that the demonstration and the rhetoric's used do not express any 'deep convictions' of mine. They merely show how easy it is to lead people by the nose in a rational way. An anarchist is like an undercover agent who plays the game of Reason in order to undercut the authority of Reason. (AM, pp. 32-3)<sup>2</sup>

*Prima facie*, this seems to be a statement to the effect that the arguments of AM, at least, should be construed as instances of *reductio ad absurdum* argumentation. In order to force the reconceptualisation of rationality, Feyerabend wants to prove that the adoption of 'Rationalist' theories of rationality lead to consequences which are anathema to those very theories: unremitting allegiance to 'Rationalist' theories should lead to the adoption of anarchism. The only objection that can be made is that Feyerabend does not explicitly state, 'I am now conducting a *reductio* of 'Rationalist' philosophy', but this seems to be a weak objection and, moreover, an objection which cannot be upheld in the light of the fact that Feyerabend explicitly states,

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<sup>2</sup> Notice the capital R in reason.

if an argument uses a premise, it does not follow that the author accepts the premise, claims to have reasons for it, regards it as plausible. He may deny the premise but still use it because his opponent accepts it and, accepting it, can be led in a desired direction. If the premise is used to argue for a rule, or a fact, or a principle violently opposed by those holding it, then we speak of a *reduction ad absurdum* (in the wider sense.) (SFS, p.156.)

Feyerabend then goes on to explicitly state that the arguments of AM were indeed meant to be of the nature of a *reductio*. (SFS, p. 157.)

It could be replied that Feyerabend only said this after the fact: after the many criticisms of AM appeared he used the idea of a *reductio* as a means of avoiding those criticisms. But this cannot be supported. Even if we ignore the quotation above from AM there are other passages which support the idea that Feyerabend is conducting a *reductio*. For example, Feyerabend replies to criticisms of his earlier papers given by Machamer, by saying that

Machamer frequently misunderstands my way of arguing. Thus he objects to my saying that Kepler's optics is refuted by simple facts, because I have also stated that theories cannot be refuted by facts. This were a valid point if at the passage in question I had been talking to *myself* ... But I did not talk to myself. I addressed people who *accept* the rule of falsification, and for *them* the example means trouble. Logicians are apt to call this an *argumentum ad hominem*. Quite so: in my essay I am addressing *humans*. (AM, pp. 113-14)<sup>3</sup>

If we accept the idea that Feyerabend is conducting a *reductio ad absurdum* of 'Rationalist' philosophy, we are still left with the question, what exactly is Feyerabend reducing to absurdity? What premises does Feyerabend use? Consequently, I will now present an account of what Feyerabend takes to be 'Rationalist' philosophy, or 'Reason', with a capital 'r'.

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<sup>3</sup> The distinction between *ad hominem* and *reductio ad absurdum* argumentation, or even between those two argument forms and simple *modus tollens*, is very unclear. In some circles *reductio ad absurdum* argumentation is simply a case of *modus tollens*; while in other circles the *reductio* is something more complicated than that. *Ad hominem* argumentation muddys the waters more: it is sometimes used exclusively as the description of informal fallacious argument where a persons character is presented as a reason to not believe what they say. In its non-fallacious form; where a persons substantive claims are shown to result in contradiction, the *ad hominem* argument becomes identical to what some take as *reductio* argumentation. I will go on to assume that a *reductio ad absurdum* argument is most nearly captured by the idea of the non-fallacious *ad hominem* argument. At times I will take it to be in the weaker sense of simple *modus tollens*, but the context should make it apparent which sense is being used.

## 1.2 Feyerabend's Conception of 'Rationalist' Philosophy.

It is my contention that what Feyerabend identifies as 'Rationalist' philosophy, 'Reason', or abstract, universal rationality, is the philosophical theses associated with what has come to be called Formalist rationality, or the classical model of rationality. Formalist rationality is a general picture of rationality which has become increasingly identified and criticised since the downfall of logical empiricist philosophy.<sup>4</sup> This is not to contend that it is solely linked to that philosophy: though it did play a role in logical empiricism in an especially stark manner, it did not originate with that philosophy. In fact, whether supported or denied, it can be seen as having had a long and continuous history, setting the boundaries of philosophical debate from the very beginning of Western philosophical speculation to the present day. It is especially important to note that Feyerabend, and others, do not limit the term Rationalists (with a capital 'r') to the classical philosophical Rationalists: Descartes, Leibniz, Spinoza. In fact, Feyerabend would contend that the classical philosophical Rationalists, together with their traditional opponents, the empiricists, hold a common set of general presuppositions concerning the nature of rationality.<sup>5</sup>

At the heart of this conception of rationality is the contention that to be rational is to follow a set of rules in an algorithmic, or procedurally structured manner. Moreover, these rules are usually conceived as necessary, universal and atemporal. Hooker characterises formalist rationality as

finitely stateable, simple rules, finite sequences of which yield algorithms for the generation of rational solutions to problems, solutions therefore characterised by

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<sup>4</sup>A good account of formalist rationality can be found in H.I. Brown, *Rationality*, Chs. I-III. Brown calls formalist rationality the classical model of rationality; I would prefer not to use that expression, as it may cause confusion with the classical rationalists (Descartes, Leibniz, et al). I will also later give reasons why the expression 'formalist rationality' does not fully, and thus not adequately, capture what it is that Feyerabend is objecting to. Other criticisms can be found in C.A. Hooker, "Between Formalism and Anarchism: A Reasonable Middle Way", in G. Munevar (ed) *Beyond Reason*, pp. 41-107, S. Toulmin, *Human Understanding*, and T. Nickles, "Introductory Essay: Scientific Discovery and the Future of Philosophy of Science" and "Can Scientific Constraints be Violated Rationally", both in T. Nickles (ed) *Scientific Discovery, Logic, and Rationality*. Cf also R. Bernstein's account of 'objectivism' in his *Beyond Objectivism and Relativism*.

<sup>5</sup> See Feyerabend, "Explanation, Reduction and Empiricism", p. 46. in Feyerabend, *Philosophical Papers Vol.1*. See also D. Shapere, "Modern Science and the Philosophical Tradition" in D. Shapere, *Reason and the Search for Knowledge*, pp. 408-17. And Brown, *Rationality*, pp. 40-54.

necessity and hence universality. And behind all this we have ... the fundamental notion of reason as purely formally characterisable.<sup>6</sup>

Thus, rationality must be rule-bound or algorithmic, so that anyone who understands the rules involved can correctly follow them. Judgments of the rationality of a belief, act, or conclusion, consist in ascertaining whether the correct procedure was followed in arriving at the belief, act, or conclusion. A conclusion may indeed be rational and/or necessary, but if the reasoning which led to that conclusion cannot be given, or the reasoning only coincidentally led to that conclusion, then it would be irrationally derived.

Within this general conception of rationality, Feyerabend identifies three theses; the adoption of any one of which is sufficient to label the position as 'Rationalist'. These three theses can be labelled the logical thesis, the factual-foundationalist thesis, and the methodological thesis. All three theses posit something as universal, necessary, and independent of the ongoing activity of knowledge acquisition; thereby supposedly assuring the rational acceptability of our changing beliefs.

The first thesis, the logical thesis, is the idea that logic is an essential constituent of knowledge and rationality. This is not meant to be taken in the weak sense of saying that to be rational, we must at least be logical, or that our knowledge must be logical, in the sense that we do not commit ourselves to blatant contradictions. It means, rather, the stronger contention that logic is to be the model which, (a) our thinking should conform to, and (b) provides the structure of our knowledge. Supporting these ideas is the contention that logic is universal and necessary: logic provides us with seemingly incontrovertible criterion of rationality. If we have the requisite logical skills, but persist in denying the logical conclusion of an argument, then we have a paradigmatic case of unadulterated irrationality. Consequently, the connection between logicity and rationality is often expanded and the further implication is made that logic is not only necessary for rationality, but, given comprehensive mastery of logic, also sufficient for rationality: to be rational is nothing more than to be logical.

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<sup>6</sup> Hooker, "Between Formalism and Anarchism", p. 45.

As a model for knowledge, logic provides the idea that knowledge should be as equally incontestable as the conclusions of logic. Our knowledge should be formalised into axiomatic systems where all particular items of knowledge become logically deducible from the axioms of the system. Comparison of theories should be, as much as possible, solely a matter of logical comparison.

As a model for rationality, logic provides the idea that there is one, and only one, necessary way of deciding what is rational. A desirable theory of rationality is one in which the prescriptions of the theory are as unequivocal as the rules of logic. The theory itself should be as close as possible to pure logic; that is, as far as possible, it should utilise the least number of non-logical terms or concepts.

The pre-eminent position held by logic in philosophical theories should be obvious; most major philosophical positions have given logic pride of place. The theories that Feyerabend dwells upon, logical empiricism and Popperianism, are no exceptions; indeed, it can be argued that they are the most strident theories in their defence of logic. *Logical* empiricism, in its most simplistic form, and as its name indicates, can be described as contending that all knowledge is exhaustively accounted for by logic and empirical experience. Popper wanted to say that his theory of falsificationism was the first 'truly' logical theory of science; being based purely upon deduction and Modus Tollens. Popper considered this to be a powerful reason to prefer his theory over others.

The second thesis, the factual-foundationalist thesis, is the idea that there exist sources of factual knowledge which cannot be doubted as to their veracity. It is usually claimed that these sources are self-justifying, basic, knowledge claims. They provide the foundation from which all claims to truth are to be justified, and/or, the foundations from which all claims to truth are to be derived.<sup>7</sup> This second thesis is endorsed by much traditional philosophy of science: logical empiricism, for example, is a foundationalist philosophy. For logical empiricists the raw data of experience was meant to be the indubitable ground upon which all knowledge was to be erected. Any knowledge claims which could not be reduced to empirical statements would have to be rejected.

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<sup>7</sup> See Feyerabend, "Knowledge Without Foundations"

Foundationalism is commonly paired with the logical thesis, for at least two reasons. Firstly, as Brown points out, "there would not be much point in claiming that a conclusion had been arrived at in a rational manner if we arrived there on the basis of an impeccable algorithm from randomly chosen premises".<sup>8</sup> And secondly, it is of the nature of logic that logical arguments are truth preserving; consequently, if we have true premises, all logical conclusions and deductions from those premises will also be true. It follows, therefore, that if we have an indubitable source of knowledge, then we will be assured that our logical operations on those sources will result in equally indubitable conclusions and deductions. Foundationalism is not, however, a necessary concomitant of the logical thesis; for example, Popper rejects foundationalism whilst retaining a pre-eminent position for logic. Popper contends that if we are logical, and follow the correct methods, then we will, at least, avoid false beliefs.

The third thesis is the idea that, to be rational, knowledge acquisition must be conducted according to the rules and strictures laid down by the independent, unchanging, methods of science. Logical empiricists contended that the methods of science were inductive in nature, and tried to show that all knowledge was, and could be, derived via induction. On the other hand, Popper contended that the method of conjectures and refutation is the only way of proceeding rationally: not only as a prescriptive account, but also as the way in which rational decisions have, in fact, been made over the centuries.<sup>9</sup>

This conception does not imply that the chosen methods are meant to be exceptionless. David Stove, for example, is an inductive fallibilist.<sup>10</sup> Also, Newton-Smith tries to convince his readers that rationalists have always considered the methods of science to be fallible.<sup>11</sup> Feyerabend, however, would label both Stove and Newton-Smith 'Rationalists'. Both Stove and Newton-Smith take methodological fallibilism to be the contention that the application of a particular method of science does not invariably lead

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<sup>8</sup> H.I. Brown, *Rationality*, p. 38.

<sup>9</sup> See, for example, Popper, "Back to the Presocratics", in CR, pp. 136-65.

<sup>10</sup> D.C. Stove, *Popper and After: Four Modern Irrationalists*.

<sup>11</sup> W.H. Newton-Smith, *The Rationality of Science*, pp. 134-5.

scientists to true knowledge. This is not to say that the chosen method is therefore circumscribed in its range of valid application. For example, Stove contends that induction may not always lead an investigator to true knowledge; induction is therefore fallible. Nevertheless, there was no other method, other than induction, which the investigator could have rationally followed. For a philosophical position to be 'Rationalist', it need not posit necessary methods or rules. To be labelled 'Rationalist' the set of circumscribed rules or methods must be atemporal and universal: no matter where or when you are, if you follow the rational rules, then you are acting rationally.<sup>12</sup>

For Feyerabend, as I mentioned above, the adoption of any one of these three theses is sufficient for a position to be labelled 'Rationalist'. However, the kindred nature of these theses usually results in more than one thesis being adopted. In fact, it can be said that, for Feyerabend, the more theses adopted, the more 'Rationalistic' the position. The logical thesis, for example, is not usually accepted without, at least, one of the other theses.

Be that as it may, it could be contended that it is the adoption of the third thesis, the methodological thesis, which is the most characteristic aspect of 'Rationalism'. Thus, even though logical empiricism was a position which, ideally, could reduce knowledge to logic plus experience, logic itself was not only to play the role of structuring knowledge, but it would also take on the role of the method of investigation. Thus it was hoped that the method of induction could be based purely upon deductive logic. And Popper, whilst eschewing foundationalism and induction, but retaining deductive logic, found it necessary to supplement his deductive model with the methodological prescription of content increase for successive scientific theories.

The contention that the methodological thesis is the central thesis, can be supported by the idea that the methodological thesis is the only one of the three theses which has been adopted singularly.<sup>13</sup> It can be argued that Lakatos's

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<sup>12</sup> This point will be further discussed in Section 1.3.

<sup>13</sup> Adopted singularly, in the sense that the adoption of any one, and only one, of the three sub-theses, will produce a coherent and viable account of knowledge acquisition. It is certainly possible that someone could be a foundationalist, or support the logical thesis, without ascribing to any other of the theses. But, as far as I know, there are no major positions, or philosophers, which ascribe to those ideas.

philosophy is one in which the logical and foundationalist theses are not supported. Nevertheless, Feyerabend contends that Lakatos is a 'Rationalist'. Lakatos presents a methodological account of the rationality of science where the methods proposed are meant to be universally applicable to the entire history of science. According to Lakatos, where science progressed rationally, scientists, more or less closely, followed the methods as laid down by Lakatos.<sup>14</sup>

At first glance this conception of rationality seems to be a highly attractive position: if every thing is changing, then it seems that we cannot make defensible judgements concerning states of affairs because the standards by which we judge those states of affairs will be changing as well. We must have something which is not changing so as to allow the very possibility of judgment and comparison. On these views, a theory of rationality is meant to reveal those canons, standards, or unchanging entities which must be present for justified beliefs to be arrived at. But, for Feyerabend, it is just such independent, unchanging and universal, entities, processes and standards, which do not do justice to the reality of human thought, action and interaction. Moreover, Feyerabend, in conducting his *reductio* of this general conception of rationality, contends that the adoption of 'Rationalistic' philosophies will lead to consequences that those philosophies will find objectionable.

### **1.3 Feyerabend's *Reductio*.**

That Feyerabend is conducting a *reductio ad absurdum* of 'Rationalist' philosophy would appear to be very obvious. However, judging by the reviews of AM it seems that it is not so obvious to many; by far the largest portion of criticisms directed at Feyerabend result from an inability to recognise this strategy. One can agree with G. Munévar when he says that "It should be an embarrassment to the profession that many

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<sup>14</sup> I don't think we can say that Lakatos personally believed that he had arrived at the absolutely final set of methodological prescriptions. It is safe to say that he believed he was on the right track, that the broad structure of his theory was correct, but that he was open to development and criticism of his theory. Nevertheless, Lakatos believed that *there is a single and correct method of science*, which, once discovered, will forever be able to explain the history of science.



reviewers were completely unable to see the structure of this simple *reductio*.<sup>15</sup> But it is not only that reviewers of AM were unable to see this *reductio*: despite Feyerabend's frequent exhortations to see the arguments of AM in terms of a *reductio*, many philosophers remain blind to this point.<sup>16</sup> Larry Laudan, for example, has recently written a book wherein a chapter on Feyerabend exhibits a complete misunderstanding of the nature of Feyerabend's *reductio*.<sup>17</sup>

After stating that Feyerabend really believes that scientists should lie, cheat, resort to propaganda, and conduct science in a genuinely anarchistic manner, assertions that already show that Laudan has not appreciated Feyerabend's *reductio*<sup>18</sup>, Laudan goes on to contend that

When we claim that a certain rule is methodologically sound, we are not committed to saying that the ends of science can be promoted *only* by following the rule in question; nor are we saying that the ends of science will *always* be furthered by following said rule. Rather, when we endorse a rule, we are asserting our belief that following that rule is more likely to realize one's goals than violating it will. What makes a rule acceptable as a rule is our belief that it represents the *best strategy* we can imagine for reaching a certain desired end;

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<sup>15</sup> G. Munévar, "Science in Feyerabend's Free Society", in Munévar (ed) *Beyond Reason*, p. 181.

<sup>16</sup> See SFS, Section 3 "Conversations with Illiterates", where Feyerabend responds to reviews of AM, frequently reiterating his point that AM was intended to be a *reductio* of 'Rationalist' philosophies. See, for example, pp. 142-3, pp. 185-7, p. 210, and especially Section 1 of "Marxist Fairytales From Australia" pp. 156-63, where Feyerabend presents his "basic rule": "If an argument uses a premise, it does not follow that the author accepts the premise, claims to have reasons for it, regards it as plausible. He may deny the premise but still use it because his opponent accepts it and, accepting it, can be led in a desired direction. If the premise is used to argue for a rule, or a fact, or a principle violently opposed by those holding it, then we speak of a *reduction ad absurdum* (in the wider sense)" p. 156. Feyerabend follows up his basic rule with four corollaries, the first three of which are (i) "If my opponent accepts historical facts and interpretations of historical events that can be used against him, then these facts can be used against him without any attempt to establish their validity" p. 158. (ii) "In an argument against an opponent an author can use assumptions and procedures he has shown to be unacceptable elsewhere provided they are accepted by the opponent." p. 158. And (iii) "Having used part of a general view *E* to produce a result repulsive to those who accept *E* one may describe the result in terms of *E* thus stressing its distressing (for the defenders of *E*) aspects. If the result concerns a situation which the defenders of *E* hold in high regard then we obtain paradoxical-sounding formulations (for the defenders of *E*, that is)." pp. 158-9.

<sup>17</sup> L. Laudan, *Beyond Positivism and Relativism: Theory, Method, and Evidence*, Ch.5, "For Method: Answering the Relativist Critique of Methodology of Kuhn and Feyerabend." A similar misunderstanding is evident in an article by J.G. McEvoy, "A 'Revolutionary' Philosophy of Science: Feyerabend and the Degeneration of Critical Rationalism into Sceptical Fallibilism", *Philosophy of Science*, 42, (1975), where McEvoy comes so close to seeing Feyerabend's *reductio*, but yet fails to carry the argument the last steps, thus resulting in a frustratingly, because so close, one-eyed interpretation of Feyerabend.

<sup>18</sup> See Section 1.5 for a discussion of Feyerabend's rhetoric in relation to his *reductio*.

but it need not be, and commonly will not be, either a necessary or a sufficient condition for reaching that end.<sup>19</sup>

Given this conception of scientific rules, Laudan contends that

The fact (assuming it to be a fact) that a few successful scientists such as Copernicus, Galileo, and Kepler broke certain familiar rules of scientific rationality and nonetheless 'advanced' science in the process does not establish that the rules they broke should be regarded as inadequate or inappropriate. It may still be reasonable to regard the rules as well established bi-conditionals, linking optimal means to desired ends, even if their violation sometimes 'works'. If Feyerabend seriously intended to discredit methodology then he should have shown, but nowhere does, that *most* of the instances of successful science have been the result of scientists violating what we regard as the methodological norms of science.<sup>20</sup>

Laudan concludes by saying that "what is transparently clear is that neither Feyerabend nor anyone else has shown that all the *extant* rules of scientific methodology are inadequate, let alone that all possible rules are discredited."<sup>21</sup>

These arguments of Laudan's would be cogent *if* Feyerabend did not employ his arguments in terms of a *reductio ad absurdum* of 'Rationalist' philosophies. But that is the point: once we place Feyerabend's arguments within such an interpretive scheme, the protestations of Laudan are seen as having simply missed the point. Firstly, given the character of formalist rationality: rational rules characterised by universality, necessity and atemporality, algorithmically applied, then the presentation of a counter-example, by Modus Tollens, would refute such positions. Feyerabend sometimes thought of his *reductio* in these terms; for example, he states that "in order to show that 'all ravens are black' is upheld by questionable means, it is sufficient to produce one white raven ... one may safely ignore the many black ravens which no doubt also exist." (AM, p. 112) Leaving aside for the moment the incompleteness of such an approach, the point to be highlighted is that in conducting this form of argumentation, as Feyerabend correctly stresses, the many positive instances can be ignored: in refuting necessary principles, only *one* counter-example is required. To require of Feyerabend, as Laudan does, that he go through the entire history of science and show that every possible permutation of

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<sup>19</sup> Laudan, *Beyond Positivism and Relativism*, p. 103.

<sup>20</sup> *Ibid.*, p. 104.

<sup>21</sup> *Ibid.*, p. 105.

'Rationalist' philosophies fails more often than it succeeds in explicating the rationality of science, shows that Laudan has not understood the structure of Feyerabend's argument. If Feyerabend can produce the white ravens required to refute logical empiricism, Popper and Lakatos, interpreted as adhering to necessary principles of rationality, then it, at least, opens up the possibility that rationality is not exhaustively characterised by 'Rationalist' philosophical positions.

However, as it stands, this argument is incomplete and the conclusion avoidable. For the argument to be convincing it must be contended that the adoption of a set of 'Rationalist' theses is both a necessary condition of rationality, and a sufficient condition: it must be contended that decisions made in accordance with 'Rationalist' theses, *will always* be rational, without exception. Laudan denies that rational rules must be of a necessary and sufficient nature, such that the production of a counter-instance to a rational rule disproves or refutes that rule. For example, it may be contended that in a particular situation the utilisation of a set of 'Rationalist' theses led to what appeared to be irrational decisions, but, the objection continues, this can be explained by saying that the decision was made without taking into account all relevant information available at the time. If all relevant information available had been taken into account, then the 'Rationalist' theses would have produced the correct, rational decision: 'Rationalist' theses are necessary, but not sufficient to ensure correct rational conclusions. Alternatively, we may interpret rational rules as does Laudan: as non-necessary procedures believed to be successful more often than not, and therefore considered to be the optimal means of ensuring the best results.<sup>22</sup>

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<sup>22</sup> Both Laudan and Newton-Smith contend that Feyerabend is arguing against a straw-man. Thus, Newton-Smith contends that "Feyerabend's easy defeat of a straw man (the rationalist who believes in infallible exceptionless rules) is construed by him as a victory over a real man (the rationalist who believes in general guiding fallible principles of comparison) who is in fact enlisted in the battle with the straw man!" *The Rationality of Science*, pp. 134-5. I find this contention to be disingenuous: one only needs to peruse Popper's writings vis a vis the demarcation criterion and the rejection of pseudo-science; and many logical empiricists vis a vis their rejection of non-empirical, metaphysical, nonsense, to see that *necessary* conditions of rationality have indeed been proposed quite frequently. Both Laudan and Newton-Smith presented their arguments *after* Feyerabend, one can see that they have adjusted what they call rationalism, specifically to avoid the type of arguments Feyerabend presents. But even here, I do not think that they have avoided Feyerabend's arguments, as I try to show below.

Again, these types of responses would have been effective against Feyerabend if he had left his *reductio* at such a simple level. We may have been inclined to agree, in part, with Newton-Smith when he says that "The way in which Feyerabend regards putative counter-productive instances to a principle of comparison indicates that he erroneously assumes that the rationalist is committed to believing in exceptionless algorithmic principles of comparison"<sup>23</sup> But Feyerabend did not present his *reductio* solely at such a simplistic level: Feyerabend's project was not limited to finding any old counter-example; rather, Feyerabend's strategy was to select episodes in the history of science which were presented, by '*Rationalists*', as paradigmatic cases of rationality. "My aim ... was to show that some very simple and plausible rules and standards which both philosophers and scientists regarded as essential parts of rationality were violated in the course of episodes ... they regarded as equally essential". (SFS, p. 13.) Feyerabend's argument is as follows:

- (1) If we adopt a set of 'Rationalist' theses, then, *ceteris paribus*, the decisions arrived at via the employment of those theses will be rational.
  - (2) Decision 'X' is accepted by all 'Rationalists' as a paradigmatic case of rationality.
- therefore;
- (3) Decision 'X' should be rationally explicable according to 'Rationalist' theses.
- But;
- (4) Decision 'X' is not rationally explicable according to 'Rationalist' theses
- therefore;
- (5) 'Rationalist' theses are inadequate to the task of explicating rational decisions.

This argument is much stronger, for it is surely a reasonable requirement of any theory of rationality that paradigmatic episodes be proffered as exemplifying the workings of the preferred rational rules. If such paradigmatic episodes do not stand up to scrutiny, then we have reason to doubt the efficacy of those rules in question more generally. Laudan,

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<sup>23</sup> Newton-Smith, *The Rationality of Science*, p. 134.

for example, even though he contends that rational rules are exceptionable, should nevertheless provide paradigmatic examples of rational science explicable in terms of his philosophy; moreover, these examples should be episodes which are generally conceived of as rational. If we then examined these episodes in the history of science and found them to be inexplicable in terms of Laudan's philosophy, then we would have reason to doubt the efficacy of Laudan's preferred rational rules. Feyerabend concludes that 'Rationalist' theses, even in situations where the examples are provided by the 'Rationalists' themselves, and which therefore would seem to have the strongest claim to being explicable via 'Rationalist' theses, nevertheless, do not provide rational decisions.

Feyerabend's argument is further enhanced by saying that, not only were *prima facie*, generally agreed upon rational decisions not made according to 'Rationalist' theses, but also that the considerations which led to the rational decisions were *completely inexplicable* in terms of 'Rationalist' theses: the rational decisions were arrived at in a manner demonstrably contrary to the whole program of 'Rationalist' philosophy. Moreover, Feyerabend wants to show that strict allegiance to the dictates of 'Rationalist' rules, in these paradigmatic situations, would have retarded the progress of science, and ruled out those very moves deemed to be rational.<sup>24</sup>

We can see that Laudan does not address these more subtle levels of argumentation. Laudan simply relies upon the idea of the more-often-than-not strategy for defending rational rules, even in cases such as Galileo. Thus, Laudan has missed the point of Feyerabend's arguments and failed to appreciate the structure of Feyerabend's *reductio*. Moreover, Laudan's ideas are moving towards a conception of rationality in

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<sup>24</sup> Feyerabend has a fall back position of even greater unassailability, and he again borrows the argumentative form from the 'Rationalists', that is, after discussing Galileo, Feyerabend goes on to say that his historical study need not be correct; "If it turns out to be a fairy-tale, then this fairy-tale tells that a conflict between reason and the preconditions of progress is *possible*, it indicates that it might arise and it forces us to conclude that our chances to progress *may* be obstructed by our desire to be rational" (AM, p. 156) It may be objected that to show that something is possible has no bearing on whether it really is the case or not. I would agree. But the argument from possibility to actuality is a quite ubiquitous argumentative form, with a pedigree unequalled in philosophy: something like it can be found in Plato and in Descartes. It still survives today amongst some 'Rationalist' inclined philosophers and to that extent is used by Feyerabend as part of his *reductio*. But it is a throw-away argument and Feyerabend does not, and should not, put any weight upon it. If Feyerabend can make good his main argument, then there is no need of this argument from possibility. If Feyerabend cannot make good his main argument, then the argument from possibility will not rescue his position.

line with what Feyerabend was proposing: if great scientists can produce theories of the highest level of rationality, without utilising the optimal rational strategy, then we are led to the idea that there are multiple, possibly inconsistent, ways to be rational. What rules to apply in a given situation thus becomes a matter of judgment in relation to the specific circumstances of the situation requiring rational decision. Rationality becomes more complex, more contextual, less standardised and less universal.

We can see from this discussion that Feyerabend believes he has provided unquestionable counter-examples to 'Rationalist' theses and that, according to their own standards, 'Rationalists' should accept that their rules are inadequate to the task of explicating rationality. Of course, Feyerabend has to prove his point through concrete examples; consequently, I will now present Feyerabend's case against 'Rationalist' philosophies by way of an analysis of Feyerabend's interpretation of Galileo.

#### **1.4 Feyerabend, Galileo and 'Rationalist' Philosophies.**

##### **1.4.1 Introduction.**

Feyerabend conducts his *reductio* of 'Rationalist' philosophies via an analysis of the arguments Galileo employs against the Aristotelian thesis of the stability of the earth. Feyerabend chooses Galileo because he believes that this episode in the history of science is one which 'Rationalists' must be able to explicate; after all, it is the 'birth of modern science'. Consequently, we should be able to see the methods and rationality of science inherent in this paradigmatic episode.

Feyerabend has three specific targets: empiricism, Popper and Lakatos. In the following I will present Feyerabend's arguments against each of these positions in turn.<sup>25</sup>

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<sup>25</sup> These arguments can be found scattered about in AM, chs 6-15. Feyerabend does not address these positions consecutively, or independently. I have reconstructed Feyerabend's account so as to give a clearer picture of the arguments involved. To anticipate the argument; it will be shown that through the analysis of Galileo's arguments, empiricism, and Popper's falsificationism, I believe, fall prey to Feyerabend's *reductio*. The case of Lakatos is more complicated: a *reductio* of Lakatos, *via an analysis of Galileo's arguments*, does not succeed.

### 1.4.2 The Reductio ad Absurdum of Empiricism.

The empiricism which Feyerabend attacks is a very generic one; but I do not think that this mitigates Feyerabend's argument. Any empiricist position which contends that sensations are the foundation of knowledge; that all theoretical terms should be reducible to observational terms; and that logic is the tool of scientific and philosophical analysis, are encompassed within Feyerabend's critique.

Feyerabend contends that the distinction between theory and observation *does not exist*, he states that

we must be clear about the nature of the total phenomenon ... There are not two acts - one, noticing a phenomenon; the other, expressing it with the help of the appropriate statement - *but only one*, viz. saying in a certain observational situation, 'the moon is following me', or, 'the stone is falling straight down'. We may, of course, abstractly subdivide this process into parts ... But under normal circumstances such a division does not occur; describing a familiar situation is, for the speaker, an event in which statement and phenomenon are firmly glued together. (AM, p. 72).

That is, though we may be able to abstractly provide some criteria with which to draw a distinction between sensory impressions and our beliefs or interpretations of those sensory impressions, this distinction is not absolute, natural or, strictly speaking, correct.<sup>26</sup>

This can be construed as an argument against empiricism, that is, inherent and integral to empiricism is the theory/observation distinction. In the above quote Feyerabend denies that this distinction exists; therefore, Feyerabend is denying the veracity of empiricism. But the denial is no more than that: no arguments are brought forth to support the contention. At any rate, an empiricist can go on assuming the veracity of the theory/observation distinction until *unequivocal* contrary evidence is produced.

Feyerabend does not go down the path of trying to refute empiricism from without, so to speak, Feyerabend is attempting to refute empiricism from within, via a *reductio*. Consequently, the above quotation should be construed as a signpost indicating

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<sup>26</sup> Cf AM, p. 168.

Feyerabend's *actual* philosophical position, before he embarks on his *reductio*; for the sake of which he assumes an empiricist framework. Thus, Feyerabend contends that we must

make the ... assumption that the quality and structure of sensations (perceptions) or at least the quality and structure of those sensations which enter the body of science, is independent of their linguistic expression. I am very doubtful about even the approximate validity of this assumption ... Yet, I shall for the moment, remain quite consciously within these limits. (AM, pp. 72-3)

Feyerabend consciously accepts, for the moment, the empiricist idea that, though we may express our knowledge in different linguistic manners, incorporating differing theoretical commitments, nevertheless, *sensations* are the same for everyone and can consequently function as a neutral arbiter in case of theoretical disagreement. Feyerabend goes on to say that "we can now distinguish between sensations and those 'mental operations which follow so closely upon the senses'" (AM, p. 73)<sup>27</sup> These operations Feyerabend calls 'natural interpretations'.

Feyerabend then presents the tower argument against the rotation of the earth. If the earth rotated, then a rock dropped from the top of a tower would strike the ground away from the base of the tower. This would be so because while the rock was descending to the ground the earth and the tower would move away from the rock. But a rock dropped from the top of a tower strikes the ground at the base of the tower. Therefore, the earth does not rotate.

Feyerabend contends that this argument convinced people of the absurdity of a rotating earth because of the seeming unshakeability of the natural interpretation which underpinned it. This natural interpretation was derived from the Aristotelian theory of perception; a theory of perception which was through and through empirical in nature, and which was intimately interrelated with the entire Aristotelian world-view. In this view perception is

a special case of a comprehensive view of motion that includes locomotion, increase and decrease, qualitative alteration, generation and corruption. This

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<sup>27</sup> The quote is from Bacon, *Novum Organum*.



comprehensive view defines motion as the transition of a form from an agent to a patient which terminates when the patient possesses exactly the same form that characterized the agent at the beginning of the interaction. Perception, accordingly, is a process in which the form of the object perceived enters the percipient as precisely the same form that characterized the object so that the percipient, in a sense, assumes the properties of the object. (AM, p. 148.)

We can see that this view of perception "does not permit any major discrepancies between observations and the things observed." (AM, p. 148.) According to the Aristotelian theory of perception, the sensations we have, given that we are normal observers: "an observer whose senses are in good order and who is not drunk or sleepy, etc." (AM, p. 149.), give us true and real information about external reality. Given this conception of perception and motion, it is easy to see how the tower argument could be considered to be a forceful argument: "the everyday thinking of the time assumes the 'operative' character of *all* motion ... it assumes *a naive realism with respect to motion* ... apparent motion is identical with real (absolute) motion". (AM, p. 74-5) This natural interpretation concerning the operative character of motion is, at first glance, quite plausible. In fact, it seems to be *empirically* well substantiated: "How could one possibly be unaware of the swift motion of a large bulk of matter such as the earth is supposed to be!" (AM, p. 75) How could "there exist large-scale processes which involve vast cosmic masses and yet *leave no trace* in our experience." (AM, p. 149.) It is easy to see how this particular natural interpretation came to be considered natural, or more correctly, how this natural interpretation was not even considered to be an 'interpretation' at all. Disregarding objects that are violently moved or animated, our *senses* inform us that *objects have no motion*. Consequently, "From the point of view of 17th-century thought and language, the [tower] argument is, therefore, impeccable and quite forceful". (AM, p. 75)

Feyerabend goes on to say that "In considering the [tower] argument, Galileo at once admits the correctness of the sensory observation made". (AM, p. 71) In fact, Galileo contends that "It is ... better to put aside the appearances, on which we all agree, and to use the power of reason either to confirm its reality or to reveal its fallacy". (AM, p. 71) This statement of Galileo's suggests that there is a distinction between

appearances, or sensory impressions, and reality: we can be deceived by appearances. More strongly, this statement suggests that Galileo subscribes to some sort of theory/observation distinction: he implies that a distinction can be made between the impressions (observations) which everyone receives, and the various interpretations (theories) with which we explain those impressions.<sup>28</sup> Feyerabend contends that

Galileo is one of those rare thinkers who neither wants forever to *retain* natural interpretations nor altogether to *eliminate* them ... He insists upon a *critical discussion* to decide which natural interpretations can be kept and which must be replaced ... natural interpretations are *necessary*. The senses alone, without the help of reason, cannot give us a true account of nature. (AM, p. 73).

This passage suggests that Feyerabend believes *Galileo* is, in important respects, anti-empiricist. For Galileo, sensory experience is not the indubitable foundation of knowledge. Reason, not experience, is the arbiter of theoretical disputes.

If we accept that Galileo was, in effect, anti-empiricist, it may not be damaging to empiricism: there may have been convincing empirical reasons for a choice of one natural interpretation over another, regardless of Galileo's position. However, in this situation this is not the case: the new natural interpretation which Galileo introduces does not alter our sensory impressions. In this respect, Galileo does not impugn the veridicality of *sensory impressions*, he impugns the veracity of *interpretations* of sense impressions:

The interpretation which Galileo uses restores the senses to their position as instruments of exploration, *but only with respect to the reality of relative motion*. Motion 'among things which share it in common' is 'non-

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<sup>28</sup> Finocchiaro has challenged these assertions of Feyerabend: he contends that Galileo did not know of, or use, any sort of theory/observation distinction; and that Galileo was unaware of the natural interpretations presupposed by Aristotelianism. See M. Finocchiaro, *Galileo and the Art of Reasoning: Rhetorical Foundations of Logic and Scientific Method*, pp. 182-200 esp. p. 195. I am uncertain as to the veracity of that assertion, nevertheless, even if it is true, Feyerabend's program is unaffected. Feyerabend is conducting his analysis of Galileo with 'Rationalist' philosophies in mind. Many of those philosophies accept some sort of theory/observation distinction. It does not matter whether Galileo accepted such a distinction, that is not the point, in fact, if Finocchiaro is right, then it gives more support for Feyerabend: it shows that 'Rationalist' philosophies of science which support a theory/observation distinction, cannot explicate the rationality of paradigmatic cases of rationality as they were at the time. Moreover, the point of Feyerabend's analysis is that *given* a theory/observation distinction, as empiricists maintain, there is, at best, no advantage in accepting the Galilean natural interpretation, and, given the balance of evidence, more reason to accept the Aristotelian interpretation. I will say more on this in section 1.5 below.

operative', that is, 'it remains insensible, imperceptible, and without any effect whatever'. (AM, p. 78)<sup>29</sup>

Just as a person sitting still on a moving ship is, in fact, in motion with the ship, so too a rock dropped from the top of a tower on a rotating earth is, in fact, in motion with the earth. The motion which is perceived, the straight down motion of the rock, is that motion which is peculiar to the rock, in relation to the tower and the earth.

These two natural interpretations: that most motion is 'operative' and therefore perceptible;<sup>30</sup> and that all motion is 'non-operative' and therefore imperceptible; *are empirically indistinguishable*. If we are empiricists, then there is no way of deciding which of these interpretations to accept. All observations which support the one, support the other. Consequently, an inductive inference from the observations, or any other logical operation upon the observations, cannot favour one or the other interpretation. But, as I previously stated, the Aristotelian natural interpretation is empirically very plausible: we do not *see* the earth moving. Therefore, even though the sensory impressions are the same for both interpretations, there seems to be more empirical common-sense in favour of the Aristotelian interpretation.

The point Feyerabend makes is that the rotating earth, and Galileo's new natural interpretation, are correct. The stable earth and the Aristotelian natural interpretation are incorrect. But an empiricist philosophy of science and rationality, at the time of the debate, cannot come to that conclusion.

An empiricist who starts from experience ... now loses the very ground on which he stands. Neither the earth, 'the solid, well-established earth', nor the facts on which he usually relies can be trusted any longer. It is clear that a philosophy that uses such a fluid and changing experience needs new methodological principles which do not insist on an asymmetric judgement of theories by experience. (AM, p. 89)

According to its own standards, empiricism cannot give us the rational, correct conclusions.

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<sup>29</sup> The quote is from Galileo, *Dialogue Concerning the Two Chief World Systems*.

<sup>30</sup> I say most motion is 'operative' and therefore absolute because the Aristotelians could, and did, accept that there are instances of 'non-operative' relative motion; for example, movement on ships in motion. But it must be remembered that these instances of non-operative motion were all instances of *forced* motion, as opposed to natural motion. They were also perceptible: non-operative, relative motion can occur, but when it does occur it is *perceptible*.

An empiricist will object to this conclusion: it may be granted that Galileo's arguments provide a paradigmatic case of rationality which an empiricist must be able to explain, but Feyerabend has not satisfied the *ceteris paribus* clause of the argument given in the last section. All relevant information, available at the time, has not been included in the analysis. Thus, Galileo had other empirical evidence at his disposal, in particular, Galileo had evidence from observations of the stars and planets. The empiricist will say that if these additional observations are included in the analysis, then inductive procedures will arrive at the desired conclusions.

The trouble is that if we include these other factors in our analysis, we find that empiricism is no better off than before, in fact, it can be said that empiricism fares much worse. Before the invention of the telescope and its application to observation of the heavens, the empirical evidence against the motion of the earth was convincing and unequivocal. Though the phenomena which apparently contradicts the idea that the earth rotates can be defused by proposing new natural interpretations, as we saw above, the annual movement of the earth around the sun creates insurmountable difficulties;

Mars, when it is close to us ... would have to look sixty times as large as when it is most distant. Yet no such difference is to be seen ... Venus ... ought to appear to us a little less than forty times as large as when it is beyond the sun and near conjunction. Yet the difference is almost imperceptible. (AM, pp. 101-2)<sup>31</sup>

That is, the empirical evidence goes against the idea of the motion of the earth. An inductive inference from the observations available would give the conclusion that the earth is motionless. If we add to these considerations the problem that no stellar parallax is observed, then the case against Copernicanism is very strong indeed.

Empiricists must conclude from this that, before the invention of the telescope, anyone who believed that Copernicanism was correct, upheld those beliefs irrationally. Copernicus, Galileo and Kepler were all irrational!<sup>32</sup> I think that this is a convincing

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<sup>31</sup> Quoted from Galileo, *Dialogue*, p. 334.

<sup>32</sup> Finocchiaro reconstructs Galileo as saying that "Copernicus's *procedure* is not rationally justifiable because he was inferring truth from mere simplicity and disregarding clear empirical counterevidence" *Galileo and the Art of Reasoning*, p. 129. If this is so, then Galileo indicts himself as having been irrational: Galileo was supporting Copernicanism by 1597, at least, see S. Drake, *Galileo Studies*:

argument, but an empiricist will point to the telescope and say that it is the expansion of the empirical domain brought about by the telescope's application to the heavens which makes Copernicanism rationally justifiable. In the end, this was the case, but the telescope initially provided no empirical support for the Copernican hypothesis. Until the technical difficulties could be overcome; the physical counterarguments defused; and the required psychological readjustments achieved, the telescope did not win the day for Copernicanism.

Early telescopes were notoriously inaccurate and there was no principled manner in which to go about rectifying their problems: "the optical theories existing at the time were not sufficient as a theoretical foundation for building the telescope". (AM, p. 114) This foundation was only later provided by Descartes and others; before then trial and error were the main source of telescopic improvement. This meant that early observations with the telescope were often very wrong: illusions, distortions and other problems were rife.

On the surface of the earth ... the telescope will of course work well; these are familiar things and our knowledge of them eliminates most distortions ... Moreover, all the familiar cues (such as background, overlap, knowledge of nearby size, etc.), which constitute and aid our vision on the surface of the earth, are absent when we are dealing with the sky. (AM, pp. 119-22).

This situation meant that early observations made with the telescope were equivocal, debatable and sometimes contradicted by naked eye observations.<sup>33</sup>

It is important to see that Feyerabend does not deny that once the problems associated with the telescope were overcome, empirical facts do, in fact, support the Copernican hypothesis; but that is not the point at issue. What Feyerabend wants to emphasise is that *before* the telescope was developed sufficiently, empiricist philosophies, strictly applied, would have rejected the Copernican hypothesis in favour of the Aristotelian/Ptolemaic alternative. Even though the Copernican hypothesis had many

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*Personality, Tradition, and Revolution*, p. 73 and p. 200. But Galileo did not use the telescope until 1609.

<sup>33</sup> See pp. 123-34 of AM for a plethora of examples of the illusions, distortions and uncertainties seen and felt at the time in question.

good reasons to be upheld, these reasons are not explicable in terms of the empiricist 'Rationalist' program. As Feyerabend contends, "the ideas survived and they can *now* be said to be in agreement with reason" (AM, p. 155). Thus, a paradigmatic case of scientific rationality cannot be explicated as rational by empiricism. Therefore, empiricism should be rejected, according to its own standards, as an adequate theory of scientific rationality.

#### 1.4.3 The Reductio ad Absurdum of Popper's Falsificationism.

Popper contends that we must make bold conjectures. These bold conjectures must have an increased truth-content relative to their predecessors: a new theory must include within it the truth-content of the previous theory; exclude the known falsity content of the previous theory; and make new assertions, as yet unfalsified, but falsifiable. This increased truth-content is achieved by proposing theories with an ever increasing level of generality, with its correlative increasing degree of falsifiability. Falsifiability is of the utmost importance for Popper, hence, new theories must include as few auxiliary hypotheses as possible. Those auxiliary hypotheses which are acceptable are ones which increase the degree of falsifiability of the total system. Related to this is the idea that theories must not be protected from falsification by *ad hoc* hypotheses. Finally, theories can be conclusively refuted and it is the aim of science to subject theories to severe tests in order to refute them.<sup>34</sup>

It can be claimed that a Popperian account of science would (a) frown upon the introduction of the original Copernican hypothesis as being scientifically improper: it had *less* truth-content than its rival. And (b) would have considered the continued support of Copernicanism as dogmatically unscientific: the original Copernican hypothesis *should have been considered refuted* and subsequently rejected.

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<sup>34</sup> In this short summary of Popper, and in what follows, I interpret Popper as a naive, or dogmatic falsificationist. Some philosophers may object to this characterisation, but I believe that there is ample textual evidence to support this claim. I take sophisticated falsificationism, of which there is some textual evidence in Popper, as best represented by Lakatos, who I will discuss next.

To begin with, it must be said that the original Copernican hypothesis was, for its time, a very bold conjecture. But its truth-content was certainly *not* in excess of its Aristotelian rival. The proposition that the earth rotates daily around its own axis, and yearly around the sun, creates insurmountable difficulties in relation to the remainder of the accepted scientific knowledge of the day: the Copernican hypothesis is not compatible with the general Aristotelian world-view. For knowledge in general, the Copernican hypothesis amounts to a *reduction* in truth-content.

Even if we stay within the confines of astronomy, the Copernican hypothesis is, *at best*, only equal in truth-content to its Ptolemaic/Aristotelian rival. As we saw in the last section, the Copernican hypothesis predicts sizes and magnitudes of Mars and Venus which are not discernible. The apparent sizes of Mars and Venus were inter-subjectively testable; constituted accepted 'basic statements' for all parties concerned; and lent support to the Ptolemaic system. Consequently, even in astronomy it can be argued that the Copernican hypothesis constituted a reduction in truth-content.

Secondly, it can be argued that the original Copernican hypothesis was *falsified*. The example of Mars and Venus can be taken to be refuting instances, relying as they do upon accepted basic statements. The absence of stellar parallax was left unexplained by Copernicus. Also, the tower argument, and other similar arguments, can be seen as refutations of the Copernican hypothesis.

I don't know how Popper would respond to the claims that the original Copernican hypothesis constituted a reduction in truth-content. I can only surmise that Popper would restrict the debate to astronomy and deny that in this area the Copernican system had less truth-content, overall, than its Ptolemaic rival. I don't think that this is a valid response: the whole intellectual climate needs to be taken into account, simply because it is *relevant*. We would not say that a new biological theory exhibited increased truth-content if that theory contradicted the remainder of the corpus of accepted physical theory.

The apparent refutation of the Copernican hypothesis, via Mars and Venus, could be defused by saying that the Ptolemaic system does not get it exactly right either, and

that, in some areas, Copernicus is better than Ptolemy. This may be true, but the apparent refutation provided by the tower argument is more difficult for Popper to avoid. It has been said that the tower argument does not refute Copernicanism; rather, the tower argument refutes Copernicanism *plus* Aristotelian physics. Consequently, the falsification can be validly attributed to Aristotelian physics and diverted from the Copernican hypothesis.<sup>35</sup> According to this account, Galileo acted wholly in the spirit of falsificationism. This may be so, but if it is, then falsificationism gives wildly differing, if not contradictory, prescriptions concerning one and the same state of affairs. Consider this passage from Popper;

By means of this mode of inference [*modus tollens*] we falsify *the whole system* ( the theory as well as the initial conditions) which was required for the deduction of the statement *p*, i.e. of the falsified statement. Thus it cannot be asserted of any one statement of the system that it is, or is not, specifically upset by the falsification. Only if *p* is *independent* of some part of the system can we say that this part is not involved in the falsification. With this is connected the following possibility: we may, in some cases ... attribute the falsification to some definite hypothesis- for instance to a newly introduced hypothesis. (LSD, pp. 76-7.)

In relation to the tower argument, the falsified statement *p*, that a stone dropped from the top of a tower should fall away from the base of the tower, is independent of Aristotelian physics; therefore, Aristotelian physics is not involved in the falsification. According to the above prescriptions, it is the newly introduced Copernican hypothesis which should be considered falsified.<sup>36</sup>

If we now turn away from the original Copernican hypothesis and look at Galileo's expansion of Copernicanism, we find very similar states of affairs. Firstly, if we grant, for the sake of argument, that the tower argument falsification can be diverted away from the Copernican hypothesis, and on to Aristotelian physics, then, according to Popper, the new hypothesis which is to replace Aristotelianism must have increased

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<sup>35</sup> This response has been made by G. Andersson, "Feyerabend on Falsifications, Galileo, and Lady Reason", p. 289. And J. Worrall, "Feyerabend and the Facts", p. 338. Both in Munevar (ed) *Beyond Reason*.

<sup>36</sup> This specific argument is not Feyerabend's, it is mine. It seems to be a valid point; being derived from Popper's own writings, and it is certainly in a Feyerabendian spirit. The general argument, that Popper's notion of content increase preserves the older theory, is of course a central part of Feyerabend's critique of Popper.



truth-content: it must be more falsifiable than its predecessor. *But this is not the case:* Galileo's new natural interpretations concerning the non-operative character of motion and the principle of circular inertia, while intended to replace a comprehensive cosmology, do not preserve the truth-content of that cosmology.

Galileo's procedure drastically reduces the content of dynamics. Aristotelian dynamics was a general theory of change, comprising locomotion, qualitative change, generation and corruption ... Galileo's dynamics and its successors deal with *locomotion* only ... Other kinds of motion are pushed aside with the promissory note ... that locomotion will eventually be capable of explaining all motion. (AM, pp. 99-100).

Worse than this for Popper, however, is the fact that neither the principle of the non-operative character of motion, nor the principle of circular inertia, lead to *new* falsifiable predictions: "No independent argument was given for its [the non-operative character of motion] validity. Galileo's support for the principle of circular inertia is of exactly the same kind". (AM, p. 91) This claim of Feyerabend's is completely valid: other aspects of Galileo's physics led to falsifiable, new predictions; for example, Galileo's ideas concerning acceleration. But these aspects could be accommodated within an Aristotelian framework. The crucial assumptions necessary to support the Copernican hypothesis: non-operative motion and the principle of circular inertia, provided no new falsifiable predictions.

Popper contends that "As regards *auxiliary hypotheses* we propose to lay down the rule that only those are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary, increases it." (LSD, pp. 82-3.) In Popper's philosophy, auxiliary hypotheses which do not meet this requirement are *ad hoc*; "'Always choose the hypothesis which goes as little beyond the evidence as possible!' ... can be shown to be equivalent ... to 'Always choose the hypothesis which has the highest degree of *ad hoc* character.'" (CR, p. 287.) These definitions of *ad hocness* implies that Popper should label Galileo's theories *ad hoc*. Therefore, for Popper, Galileo's science is 'bad' science.

Feyerabend has also argued that Galileo's ideas on motion are *ad hoc* in another manner:

My guess is that a *clear idea of permanent motion with(out) impetus developed in Galileo only together with his gradual acceptance of the Copernican view*. Galileo changed his view about ... motions ... in order to make them compatible with the rotation of the earth and in order to evade the difficulties of the tower argument. His ideas concerning such motions are, therefore, at least partly *ad hoc*. Impetus in the old sense disappeared, partly for methodological reasons ... partly because of the vaguely perceived inconsistency with the idea of the relativity of all motion. The wish to save Copernicus plays a role in either case. (AM, pp. 96-7)

This 'guess' of Feyerabend's seems at least possible: Galileo's earliest writings on motion, written some five years before his acceptance of Copernicanism, though showing much dissatisfaction with Aristotelian accounts, do not, for the most part, indicate his mature views, or support Copernicanism.<sup>37</sup> It is only *after* Galileo accepted the Copernican hypothesis that his views on motion became decidedly pro-Copernican. Be that as it may, we do not have to rely upon this hypothesis to show that the Popperian account of science is in trouble.

In the *Dialogue*, Galileo assumes that the planets move in perfectly circular orbits. This idea constitutes a dramatic loss of content for the Copernican hypothesis. Even in Copernicus' early writings, planetary orbits were not perfectly circular: epicycles needed to be added to procure the fit between theory and phenomena. In his later writings, Copernicus needed to add even more machinery to achieve a greater fit. Perfectly circular orbits simply did not fit the facts: Galileo's circular orbits were known by all to be falsified. Needless to say, no new facts were predicted by postulating circular orbits. Galileo's reliance on circular orbits is even more puzzling when we consider the fact that Kepler had, many years previously, published his elliptical orbit theory: a theory which was in close agreement with the facts. One can only suggest that Galileo disregards the problems associated with exact astronomical prediction and supports a simplified system because circular orbits cohere with Galileo's dynamical theory of

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<sup>37</sup> See Drake, *Galileo Studies*, pp. 72-3. and pp. 261-4.

circular inertia; that is, the postulation of circular orbits is *ad hoc*, it is supported by Galileo because it would lend support to his principle of circular inertia.<sup>38</sup>

One may add at this point that the explanation given by Galileo to resolve the difficulty created by the absence of stellar parallax: that the fixed stars are much further away than was thought to be the case at the time, is completely *ad hoc*. It only serves to save the Copernican hypothesis from refutation.

Turning to the telescope does not produce the desired increased truth-content necessary to rescue Popper's account. Recall that Popper believes that we should be parsimonious with the introduction of auxiliary hypotheses, for two reasons. Firstly, auxiliary hypotheses could be introduced simply to save a theory from refutation; this is not allowable. Secondly, the fewer auxiliary hypotheses we have, the more falsifiable a theory becomes. However, for the telescope to be considered a valid means of extending sense experience, various additional, unproven, and controversial auxiliary hypotheses needed to be proposed.

Accepting for the moment the technical adequacy of the telescope, auxiliary hypotheses concerning the nature and behaviour of light were needed to ensure that observations of the heavens could be considered as accurate as those of terrestrial observations. Feyerabend contends that for the Aristotelians "celestial objects and terrestrial objects are formed from different materials and obey different laws. This idea entails that the result of an interaction of light ... with terrestrial objects cannot, without further discussion, be extended to the sky" (AM, p. 121) This seems to be true: we can admit that light is being transmitted to us from the ethereal spheres, but the light transmitted may be distorted in some manner; for example, no one will deny that light behaves noticeably different when passing through water; might not a similar process be

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<sup>38</sup> Drake, *Ibid.*, pp. 257-68 has contended that Galileo never believed in circular inertia. For Drake, circular inertia, and the general reverence for all things circular to be found in the *Dialogue*, is part of an attempt by Galileo to show that he is more Aristotelian than the Aristotelians: it is part of Galileo's rhetoric of persuasion, designed to show that the Copernican hypothesis is not so radically un-Aristotelian as many thought. That may be so, but the question is, how does this type of legitimate scientific argument - the *Dialogue* was immensely influential in securing the victory of Copernicanism - square with a Popperian analysis? Nowhere in Popper can I find exhortations to engage in rhetoric. See section 1.5 below for more on this.

happening when light is transmitted through the ether? Galileo seems to take it for granted that there is no problem here, and, in relation to Popper, no independent tests are provided to prove that light behaves exactly the same in the two realms.

If we turn to the question of the technical adequacy of the telescope, we find that the functioning of the telescope could not be explained by the optical theories of the time; that is, the extant optical theories which could have been used to support the reliability of the telescope were false: they had been refuted.<sup>39</sup> If we add to this the fact that it was accepted that

illusions can be readily demonstrated by examining the images produced by curved mirrors, or by crude lenses (and remember that the lenses used by Galileo were far from the level of perfection achieved today): they are distorted, the lens-images also have coloured fringes, and they may appear at a place different from the place of the object. (AM, pp. 148-9)

then the need for an adequate explanatory hypothesis which could explain and exclude these illusions was pressing indeed. In this situation, the Aristotelian theory of perception, which discouraged the use of instruments because "they interfere with the processes in the medium", (AM, p. 148) seems to be a more adequate and untroubled theory when compared to the new theory of vision, fraught with difficulties, which was necessary to give support to telescopic observation.

As I said in the previous section, the requisite skill to correctly interpret what was seen through the telescope was lacking: the usual visual cues and familiarity provided by terrestrial objects is lacking when we look through the telescope at the sky. If we add to these psychological difficulties the early technical inadequacies of the telescope, then we find that the observations made via the telescope are often false, and sometimes falsified by naked eye observations. For example, Galileo's description of the moon was refuted by Kepler's naked eye observations. (AM, p. 127)

If we bring all these factors together the prognosis is not very promising for Popper's theory of scientific rationality. A new instrument is introduced whose workings cannot be explained. A number of auxiliary hypotheses are necessary to make the use of

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<sup>39</sup> See AM, pp. 136-7, and pp. 59-60.

the instrument relevant; however, none of these auxiliary hypotheses can be independently justified. Naked eye observations can refute some of the observations made with the new instrument. The situation seems to imply, according to Popper, that the telescope should not even be considered to be a valid instrument for the investigation of nature.

However, if we place these worries to one side, we find that there are at least some phenomena seen through the telescope which do support the Copernican hypothesis: the phases of Venus and the moons of Jupiter. Consequently, Popper may say that, at least in these situations, Copernicanism passed crucial tests intended to falsify it. But the situation is not so simple: "The moon and some of the planets, such as for example Jupiter, were enlarged while the apparent diameter of the fixed stars decreased: the former were brought nearer whereas the latter were pushed away." (AM, p. 128) This result was unexpected: Copernican theory had not predicted it, in fact, the telescope was meant to magnify objects, not to diminish them. Was this taken to be a refutation of the Copernican hypothesis? No, the result was explained away by Galileo by asserting that the eye introduces hindrances to sight, which the telescope removes. We can agree with Feyerabend when he says that "Galileo's hypothesis received support mainly from its agreement with the Copernican point of view and was, therefore, largely *ad hoc*" (AM, p. 139)

To conclude this section on Popper, I think we can agree with Feyerabend when he says that

detailed study of historical phenomena such as these, creates considerable difficulties for the view that the transition from the pre-Copernican cosmology to that of the 17th century consisted in the replacement of refuted theories by more general conjectures which explained the refuting instances, made new predictions, and were corroborated by the observations carried out to test these new predictions ... while the pre-Copernican astronomy *was in trouble* ... the Copernican theory *was in even greater trouble* ... but that being in harmony *with still further inadequate theories* it gained strength, and was retained, the refutations being made ineffective by *ad hoc* hypotheses and clever techniques of persuasion. (AM, p. 143)

It must always be remembered that Feyerabend is not saying that pre-Copernican astronomy was not refuted, in the end. Nor is he saying that post-Copernican science remained *ad hoc*. The *ad hoc* hypotheses and the auxiliary hypotheses necessary were *eventually* substantiated. For example, by the middle of the 17th century optical theory had come a long way in providing theoretical explanations of the working of the telescope; consequently, bigger and better telescopes were being produced which were vindicating the Copernican hypothesis and providing a firm refutation of the Ptolemaic astronomy.<sup>40</sup> The point Feyerabend is trying to make is that Popper's account of science leads us to label significant scientific episodes as irrational: if the protagonists at the time had been Popperians, then they would have rejected the Copernican hypothesis as irrational, time and time again.<sup>41</sup>

#### 1.4.4 Galileo and Lakatos's Methodology of Scientific Research Programmes.

Lakatos's methodology of scientific research programmes presents Feyerabend a far harder nut to crack than either empiricism, or Popper's naive falsificationism. In fact, Feyerabend cannot conduct a *reductio* of Lakatos's philosophy via an analysis of Galileo, however much he would like to. This is because Lakatos agrees with Feyerabend's analysis. In the case of empiricism, this conclusion is perhaps wholly within expectations, Lakatos is, after all, sympathetic with the anti-empiricist Popper.<sup>42</sup> But in relation to Popper it may seem a little surprising: one could expect Lakatos to defend Popper, or at least mitigate the main conclusions, but this is not the case. Lakatos contends that "if we apply the falsificationist criterion to the question of when Copernicus' theory superseded not only Ptolemy's but also Tycho Brahe's ... then falsificationism has only an absurd reply: that it did so *only in 1838*"<sup>43</sup> This is so because, though the establishment of the

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<sup>40</sup> So, we can say that by about 1650, the Ptolemaic astronomy had been refuted, and the Copernican hypothesis accepted, but, as it turns out, the Copernican hypothesis *should not have been accepted*, even then. More on this in the next section.

<sup>41</sup> For a broader discussion concerning the historiographical falsification of falsificationism, see Lakatos, "History of Science and its Rational Reconstruction", in Howson (ed) *Method and appraisal in the Physical Sciences*, pp. 21-7.

<sup>42</sup> See I. Lakatos, and E. Zahar, "Why Did Copernicus' Research Program Supersede Ptolemy's?", in R.S. Westman (ed) *The Copernican Achievement*, pp. 356-8.

<sup>43</sup> *Ibid.*, p. 359.

veridicality of the observations made through the telescope refuted Ptolemy's astronomy, it did not refute geocentrism. In particular, Tycho Brahe's system was compatible with the results of telescopic observations. The conclusive refutation of geocentrism only came with the establishment of the existence of stellar parallax in 1838. Needless to say, by that time geocentrism had been considered refuted for centuries. Lakatos sums this situation up as follows:

Something must be wrong with the falsificationist account. This is a typical example of how history of science can undermine a philosophy of science - too much of the actual history of science was irrational if scientific rationality is falsificationist rationality.<sup>44</sup>

This conclusion is identical to Feyerabend's, but Lakatos does not draw the further conclusion, as does Feyerabend, that 'Rationalism' is therefore an inadequate way in which to approach the rationality of science. On the contrary, Lakatos presents a stripped down 'Rationalism' designed to avoid the difficulties Popper's views encountered.

There were three factors which militated against Popper's falsificationism (i) take refutations seriously (ii) avoid *ad hoc* hypotheses and (iii) always aim for increased truth-content for successive theories. Lakatos repudiates strict versions of these theses, although, as I will show, he retains aspects of all three.<sup>45</sup>

Firstly, for Lakatos, a *new* research programme does not have to fulfil the truth-content increase requirements of Popper. New research programmes are almost always characterised by a reduction in truth-content. This is as it should be: it takes time for new research programmes to show their fruitfulness and adaptability. However, once a new research programme has been proposed, successive versions of the research programme *must* show increased truth-content: there must be increased truth-content *internal* to a particular research programme. If this internal truth-content increase does not eventuate,

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<sup>44</sup> *Ibid.*, p. 360.

<sup>45</sup> For detailed discussion of the following points, see I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes" in I. Lakatos and A. Musgrave (eds) *Criticism and the Growth of Knowledge*. In what follows my aim is to show that Lakatos avoids the problems associated with Popper's falsificationism vis a vis Feyerabend's *reductio*. As such, the arguments are highly simplified and selective. In chapter 2 I will give a detailed analysis of Lakatos's methodology of scientific research programmes.

Lakatos describes the research programme as entering a degenerating problem-shift. If a programme continues to degenerate, this constitutes tentative reasons for abandoning the programme.

Secondly, if new research programmes do not have to fulfil the requirement of truth-content increase, then it is easy to see that new programmes are 'born' refuted. Consequently, new research programmes should not be abandoned because of apparent refutations: in time the research programme may be able to overcome these apparent refutations. What is refuted is a version of the research programme, not the programme itself. If new versions are proposed which exhibit internal truth-content increase, then the programme is considered to be unrefuted. Even if a programme is degenerating, it is not considered refuted: it may stage a successful comeback.

Thirdly, *ad hoc* hypotheses are acceptable. As long as there is *some* increased truth-content between successive theories within a programme, *ad hoc* hypotheses are useful in staving off apparent falsifications. Of course, *ad hoc* hypotheses should eventually be replaced or transformed into testable hypotheses, but Lakatos does not rule out theories as unscientific if they happen to include *ad hoc* hypotheses.

For Lakatos, science is rational if relations of truth-content increase between successive theories within research programmes can be demonstrated. In the case of the Copernican hypothesis, Lakatos contends that both Ptolemy and Copernicus were working within the one (super) programme: "Both *programs* branched off from the Pythagorean-Platonic program whose basic principle was that since heavenly bodies are perfect, all astronomical phenomena should be saved by a combination of as few uniform circular motions ... as possible"<sup>46</sup> According to Lakatos, the Copernican hypothesis constituted an increase in truth-content for this Pythagorean-Platonic research programme.<sup>47</sup>

<sup>46</sup> Lakatos and Zahar, "Why Did Copernicus' ..." p. 370.

<sup>47</sup> For details, see *Ibid.*, pp. 370-81. Whether Copernicus can be said to be continuing the problematic of the Pythagorean-Platonic research program, or working within a new research program, or continuing an entirely different, Aristarchan, research program, is a highly controversial theoretical and historical problem. In any case, the question of whether Copernicus' system constituted truth-content increase is a moot question steeped in complexity. See the discussion of these points in N. Thomason, "Could



This is the only point at which Feyerabend attempts some sort of *reductio* via this particular historical episode. Feyerabend contends that "Lakatos ... notes progressive changes but only because he omits the dynamical and the optical problems and concentrates on kinematics, pure and simple." (AM, p. 178) That is, Lakatos separates Ptolemaic astronomy from the context of the general Aristotelian world-view, then proclaims that Copernicus' hypothesis is a dramatic example of truth-content increase. Feyerabend's point is that if we do not separate astronomy from its Aristotelian context, then the contention that Copernicus' hypothesis constituted truth-content increase becomes highly debatable indeed. Of course, Lakatos could respond to this by saying that the loss of content sustained in the non-astronomical domains was balanced out by the gain in content in the astronomical domains. Adding up the scorecard here would be an impossible task. At any rate, even if it could be proved that there was an overall content loss, Lakatos could still defend his position by saying that there was at least *some* content increase, and that is all that is required. The fact that the tower argument seems to refute the Copernican hypothesis is also easily accommodated within Lakatos' system: refutations do not have to be taken seriously; or they can be temporarily explained away via *ad hoc* hypotheses.

Recall that Galileo incorporated circular orbits within his astronomy, and that this seemed to be an obvious case of content decrease. Lakatos solves this problem, and the other problems associated with Galileo's system (general loss of content, *ad hoc* hypotheses, etc.), by dissolving it: "From the point of view of the methodology of scientific research programs the Copernican program was not further developed but rather abandoned by Kepler, Galileo and Newton".<sup>48</sup> This is an ingenious move; as we saw above, Lakatos explicitly supports the idea that *new* research programmes do not have to fulfil the requirements of content increase. New research programmes can avoid

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Lakatos, Even With Zahar's criterion for Novel Fact, Evaluate the Copernican Research Programme?", *British Journal for the Philosophy of Science*, 43, 1992.

<sup>48</sup> Lakatos and Zahar, "Why Did Copernicus' ...", p. 375. But Cf. "Falsification ..." p. 124, text and fn. 3, and p. 142, fn. 3. where Lakatos presents *Copernican* astronomy as a new research programme, grafted onto, and inconsistent with, Aristotelian physics. While Galileo's circular inertia hypothesis is characterised as *ad hoc*, "the only purpose of which is to hide the 'deficiency'" p. 142, arising from an inconsistent research programme.

refutations. And new research programmes are allowed to propose *ad hoc* hypotheses: they need breathing space. Consequently, it seems that Lakatos can construct a 'Rationalist' account of scientific change based upon a modified, but nevertheless, *universal* concept of truth-content increase.

Though Lakatos' strategy is certainly questionable, I think that we must accept the fact that Lakatos can avoid Feyerabend's *reductio* via an analysis of Galileo's arguments. Feyerabend does not leave the issue there; he still believes that he can conduct a *reductio* of Lakatos' philosophy, but his *reductio* of Lakatos is conducted via different issues. However, analysis of this issue will have to wait until chapter 2.

### **1.5 Feyerabend's Rhetoric: Propaganda, Irrationality and Subjective Wishes.**

From the above analysis we can conclude that, for Feyerabend, 'Rationalist' philosophies, according to their own standards, should label *prima facie* rational activity as irrational. If a 'Rationalist' *should* label what are generally considered to be rational episodes in the history of science as irrational, then how can the 'Rationalist' account for the acceptance of those ideas? Feyerabend contends that 'Rationalist' philosophies leave no room for anything other than their 'Rationalist' theses as explications of rational activity. Therefore, if anything else was involved in these historical episodes, those factors must be, at best, a-rational, if not irrational.

It must be remembered that Feyerabend has chosen Galileo as his main case study because it is generally considered, by 'Rationalists', to be a paradigmatic example of scientific rationality. This pushes aside the objection that 'Rationalists' do not contend that their standards are exceptionless, and that therefore any one case study does not impugn the validity of their standards. 'Rationalists' *must* be able to produce some unequivocal, paradigmatic examples, where their standards, without any other intervening factors, produced rational decisions.

The paradigmatic example of Galileo cannot be reduced to the 'Rationalist' theses of empiricism, or Popper. Consequently, Feyerabend contends that "Galileo's utterances are ... arguments in appearance only. For Galileo uses *propaganda*. He uses

*psychological tricks* in addition to whatever intellectual reasons he has to offer. These tricks are very successful: they lead him to victory." (AM, p. 181) Notice here that Feyerabend does not deny that there are 'intellectual reasons', arguments, put forth by Galileo. So when Machamer claims that "Feyerabend is at pains to show that Galileo had *no* reason in 1632 to hold the Copernican hypothesis",<sup>49</sup> he has misinterpreted Feyerabend's project. What Feyerabend denies is that *all* of Galileo's tactics, including some of the most cogent ones, can be encompassed within 'Rationalist' explanations. Consequently, *empiricists* and *Popper*, must say that Galileo uses propaganda and psychological tricks in order to secure victory for his ideas, because their 'Rationalist' accounts have been exhausted. Any other factors which proved to be determinative cannot, on their account, be rational and must therefore be a-rational or irrational: propaganda. Feyerabend believes that Galileo had all sorts of reasons for defending Copernicanism. For Feyerabend, the fact that 'Rationalist' accounts of rationality cannot explicate Galileo's reasoning, reduces their accounts of rationality to absurdity.

It is interesting to note that some philosophers have labelled some of Galileo's argumentative moves as propaganda. As I mentioned in the previous section, Stillman Drake regards Galileo's support of circular inertia in the *Dialogue* as propaganda:

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 ... [however, Galileo] knew when he wrote the *Dialogue* that strong opposition could be expected from the professors of philosophy, most of them convinced Aristotelians. It was for this reason ... [that] he deliberately conceded ... to the philosophers everything he possibly could ... Accordingly, when I read the metaphysical praise of circles in the *Dialogue*, I do not conclude with most historians that its author was unable to break the spell of ancient traditions; rather, I strongly suspect an ulterior purpose in those passages.<sup>50</sup>

Finocchiaro has objected to this interpretation of Drake's, contending that one of the main reasons behind this attribution of propaganda to Galileo is the fact that circular inertia does not cohere with modern science; consequently, since Galileo was a great

<sup>49</sup> P.K. Machamer, "Feyerabend and Galileo: The Interaction of Theories, and the Reinterpretation of Experience", *Studies in History and Philosophy of Science*, 4, 1973, p.35.

<sup>50</sup> Drake, *Galileo Studies*, p. 253.

scientist, his support of circular inertia *must* have been for the purposes of propaganda.<sup>51</sup> If Finocchiaro is right, then we can conclude that *aspects of Galileo which do not cohere with modern expectations are labelled as propaganda*; just as Feyerabend has proposed. On the other hand, if this interpretation of Drake's is correct, then we can conclude that *propaganda may be a powerful tool in scientific debates*. We can agree with Feyerabend that

Galileo's propaganda machinations are often guided by the insight that established institutions, social conditions, prejudices may hinder the acceptance of new ideas and that new ideas may therefore have to be introduced in an 'indirect' manner, by forging links between the circumstances of their origin and the forces that may endanger their survival. (AM, p. 106, fn. 22.)

It is easy to see that such 'propaganda', that is, rhetoric, is nowhere mentioned in the writings of 'Rationalists' as an acceptable scientific tool.

In this context it is imperative to give an example of what Feyerabend actually means by propaganda. Recall that Feyerabend contends that Galileo introduced new natural interpretations to replace the Aristotelian natural interpretations. This process would be unobjectionable and relatively easy *if* Galileo had had recourse to the theory/observation distinction: he would simply be proposing a new theory to account for the accepted observations. But, as Finocchiaro has pointed out, it can be argued that the theory/observation distinction was not an accepted distinction at the time.<sup>52</sup> In fact, the Aristotelian theory of perception seems to forbid such a distinction: Aristotelian perception

is a process in which the form of the object perceived enters the percipient as precisely the same form that characterized the object so that the percipient, in a sense, assumes the properties of the object. A theory of perception of this kind ... does not permit any major discrepancy between observations and the things observed. (AM, p. 148)

In this situation, how can a new natural interpretation, which seems to run counter to the most obvious empirical experience, be introduced *without impugning the Aristotelian*

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<sup>51</sup> Finocchiaro, *Galileo and the Art of Reasoning*, pp. 88-9.

<sup>52</sup> *Ibid.*, pp. 182-200 esp. p. 195.

*theory of perception?* "Galileo 'reminds' us that there are situations in which the non-operative character of shared motion is just as evident and as firmly believed as the idea of the operative character of all motion is in other circumstances". (AM, p. 81) For example, events on boats, where a cannon-ball dropped from the top of a mast lands at the base of the mast. These circumstances, where Aristotelians accept that there is relativity of motion, are then used by Galileo to point out that the relativity of motion *has always been accepted*: to put it crudely, you always have believed in the relativity of motion, so why can't you accept it for the earth as a whole? Galileo then introduces the Plato's theory of *anamnesis*, that is, it really is the truth, you have always believed it, you only need to be shown its veracity and you will come to believe that it is true. Without recourse to the theory/observation distinction, Galileo must employ rhetoric in order to preserve the idea that observation provides us with true pictures of the world; and in order to get his arguments a hearing. Thus, reasonable arguments sometimes need to be accompanied by rhetoric - propaganda - so that the cogency of the argument can be appreciated.

Feyerabend contends that propaganda/rhetoric is sometimes necessary in order to present an argument or view in its best light. This is especially so if the argument or view is controversial, or is in opposition to prevailing, accepted views. For example, after saying that scientific debates are characterised by political propaganda, Feyerabend goes on to say that this is not a bad thing and that it in fact constitutes a tremendous step forward, for

the fact that we are dealing with party lines is ... not really a drawback ... party lines play a most important role in many civilised institutions, such as in the democratic process, in the process of trial by a competition of opposing opinions which allows for the examination of even the most fundamental assumption and the most expert testimony.<sup>53</sup>

What Feyerabend is saying here is that there is never a foolproof, absolutely certain, answer to the questions which are asked in such diverse areas as democratic institutions, legal processes and scientific debate. Given this idea, it is allowable for there to be

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<sup>53</sup> Feyerabend, "Classical Empiricism", in *Philosophical Papers Vol.2*. p. 51.

rhetoric and propaganda to ensure that correct views, such as Galileo's, are not thrown out of court on the weight of circumstantial evidence, as it were. Of course, Feyerabend has not shown where the distinction between good propaganda and bad propaganda lies, but one feels that Feyerabend won't be drawing any absolute lines here. An important clue can be gathered if we consider this quote: "If we choose different kinds of standards, for example if we permit standards to change, in an 'opportunistic' manner from one case to the next, then the 'propaganda' turns into reason." (SFS, p. 214.) I interpret this passage as saying that what may appear, from one perspective, to be propaganda, intellectually unfounded, may be the epitome of rationality from a different perspective. Thus, Feyerabend is arguing that what is believed to be rational is now dominated by 'Rationalist' standards; consequently, those moves which do not admit of 'Rationalist' explication are labelled as propaganda. But if we were more 'opportunistic', more contextually sensitive, we may find that the 'propaganda' proves itself to be successful, and thus becomes reasonable.

If Feyerabend's attribution of propaganda to Galileo, and scientists in general, can be explicated as arising out of his *reductio*, then so too can Feyerabend's frequent assertion that science is governed by 'subjective wishes'. 'Rationalists' contend that their standards are rationally objective; therefore, if paradigmatic cases of rationality cannot be explicated in terms of these objective standards, then the determinative factors must be either non-rationally objective, social, for example; or they must be governed by the subjective wishes of the protagonists involved. These conclusions are unpalatable to the 'Rationalist', but what *Feyerabend* means by subjective wishes is very different to what is usually meant:

They are connected partly with the tradition in which the scientist works, with the beliefs and the prejudices which are characteristic of that tradition; and they are connected partly with his own personal idiosyncrasies. The formal apparatus available, and the structure of the language that he speaks, will also strongly influence the activity of the scientist ... Another factor which strongly influences theorizing is

metaphysical beliefs ... theories may be influenced even by aesthetic motives.<sup>54</sup>

Now, saying that a scientist's choice is affected by the tradition in which the scientist works is a wholly uncontroversial idea. Scientists always make their choices in the light of their tradition's beliefs. A biologist will make the assumption that an organism's physical constitution is determined by genes and the environment. Non-physical factors are said to be causally non-existent: they are simply ruled out of consideration. Personal factors obviously enter into scientific decision-making: scientists are not robots. The formal apparatus available strongly affects scientific decision-making: non-linear physics was for a long time not utilised in theory choice because it was put into the too-hard basket. The advent of computers has engendered the blossoming of the use of theories utilising non-linear equations.

Feyerabend places whatever is not explicable in terms of 'Rationalist' theses under the rubric of propaganda or subjective wishes. This can be seen as part of the *reductio* of 'Rationalist' philosophies: 'Rationalists' should contend that propaganda and subjective wishes are rife in science, simply because their 'Rationalist' standards cannot explicate paradigmatically rational scientific episodes. Feyerabend contends that there is rhetoric and other factors in science, which are rational, but which cannot be subsumed under a 'Rationalist' banner.

In conducting a *reductio* a philosopher doesn't have to support the premises of the argument; usually they don't. I would want to contend that Feyerabend doesn't support the 'Rationalist' premises upon which he conducts his *reductio*. However, others have said that Feyerabend does believe the 'Rationalist' premises and is therefore serious in his attribution of irrationality to science. For example, Hooker says that

Feyerabend has stressed [that] the deployer of a *reductio ad absurdum* is not committed to believing its premises. So then, I cannot convict Feyerabend of tacit formalist presuppositions merely by noting the crudity of his formulations. But then reasonable people are entitled to ask for the positive account which is to replace that which has been rejected.<sup>55</sup>

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<sup>54</sup> Feyerabend, "Explanation, Reduction and Empiricism", in *Philosophical Papers Vol.1*. pp. 59-60.

<sup>55</sup> Hooker, "Between Formalism and Anarchism", p. 95.

This, in itself, is an acceptable demand; however, Hooker goes on to say that Feyerabend does not provide a positive account, and that he therefore tacitly supports 'Rationalist' theses, and therefore Feyerabend seriously believes that science is irrational. But I believe that Hooker is wrong. In the following chapters I hope to further substantiate this conclusion.



## 2 Feyerabend, Lakatos and Anarchism

### 2.1 Introduction

In the previous chapter we saw that Lakatos's Methodology of Scientific Research Programmes (MSRP) can avoid Feyerabend's attempted *reductio* of 'Rationalist' philosophies: the methodological tool-kit of MSRP is able to circumvent the conclusions resulting from Feyerabend's analysis of the arguments of Galileo. In this chapter I want to show that the same strategies which protect Lakatos's MSRP from Feyerabend's *reductio* in individual cases, such as that of Galileo, are also the means by which Feyerabend constructs a general *reductio ad absurdum* argument against Lakatos's MSRP.

In particular, Feyerabend's 'epistemological anarchism' must be understood within the context of his *reductio* of Lakatos's 'Rationalist' MSRP. Just as, according to Feyerabend, Empiricists and Popper must either give up their 'Rationalist' positions, or accept the idea that large amounts of intuitive, paradigmatically rational, episodes in the history of science were irrational, so too Lakatos must either give up the remaining 'Rationalist' strands in his MSRP, or accept the idea that the history of science is a chronicle of 'anything goes' anarchism. Feyerabend does not believe that the history of science is irrational, in any absolute sense. What he does believe is that science *appears* irrational when viewed from the perspective of certain 'Rationalist' philosophies. Similarly, Feyerabend is not a committed anarchist, and his support of anarchism is of a very idiosyncratic nature; that is, Feyerabend doesn't mean by anarchism what many people mean by anarchism: he doesn't take it to mean chaos or complete disorder. However, from the perspective of 'Rationalist' philosophies, the history of science seems to exhibit unprincipled change, chaos and anarchism.

In this chapter I will, (i) present Feyerabend's *reductio* of Lakatos's MSRP and examine the question of whether Lakatos can reply to these charges of internal incoherence. (ii) Elucidate a second argument of Feyerabend's to the effect that, even if Lakatos can avoid the charges of internal incoherence, he is still guilty of begging the

question concerning the nature of rationality. (iii) Dispel the idea that Feyerabend welcomes the conclusion that science is anarchistic. And (iv) Show that, through the analyses of (i), (ii) and (iii), we can begin to see the positive aspects of Feyerabend's philosophy.

## **2.2 Feyerabend's *Reductio* of Lakatos's MSRP.<sup>1</sup>**

### **2.2.1 Introduction.**

At its most basic level, Lakatos's philosophy can be seen as an attempt to provide positive answers to three questions. Can we draw a demarcation between science and non-science? Can we distinguish rational factors in science from irrational, or a-rational factors? And, based upon the answers to the first two questions, can we give prescriptive, heuristic advice to working scientists? The specific content of Lakatos's answers to these questions must be seen as resulting from his exposure to the philosophies of Popper and Kuhn. From Popper, Lakatos inherited the 'Rationalist' ideals that the answers to the questions must be of a universal, a-temporal and unequivocal nature. From Kuhn, Lakatos came to realise that Popper's normative methodology did not provide a reliable guide when it came to explaining the history of science.

Applying Popper's methodology to the history of science resulted in too much science, and too many scientists, being labelled irrational. But if this is the case, then an explanation of the apparent progressiveness and rationality of science becomes inscrutable: if scientists behave irrationally, but, nevertheless, science continues to rationally progress, then one of the few options is to enlist the 'cunning of history' to compel world<sub>2</sub> entities to mirror the denizens of world<sub>3</sub>. Lakatos does not take up this option. Instead, Lakatos liberalises methodology so as to be able to rationally explicate the history of science, whilst maintaining the universal, a-temporal, non-psychologistic

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<sup>1</sup> In what follows I take my cue from Feyerabend. I elaborate on, expand, and add to Feyerabend's arguments, which can be found in AM, ch.16. and "On the Critique of Scientific Reason", in C. Howson (ed) *Method and Appraisal in the Physical Sciences*, pp. 309-39. However, I do not rely specifically upon Feyerabend's presentation of the arguments. In this sense, I want to bring out the *objective* characteristics of Lakatos's MSRP, which are the source of Feyerabend's *reductio*

standards of 'Rationalist' philosophy and world<sub>3</sub>. As we will see, the attempted preservation of this balancing act is the source of Lakatos's problems and the key to Feyerabend's *reductio*.

### 2.2.2 Theory Appraisal: Intra-Research Programme.

Lakatos's entire methodological programme is summed up very clearly in the following passage:

Let us take a series of theories,  $T_1, T_2, T_3, \dots$  where each subsequent theory results from adding auxiliary clauses to (or from semantical reinterpretations of) the previous theory in order to accommodate some anomaly, each theory having at least as much content as the unrefuted content of its predecessor. Let us say that a series of theories is *theoretically progressive* (or '*constitutes a theoretically progressive problemshift*') if each new theory has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact. Let us say that a theoretically progressive series of theories is also *empirically progressive* (or '*constitutes an empirically progressive problemshift*') if some of this excess empirical content is also corroborated, that is, if each new theory leads us to the actual discovery of some *new fact*. Finally, let us call a problemshift *progressive* if it is both theoretically and empirically progressive, and *degenerating* if it is not. We '*accept*' problemshifts as 'scientific' only if they are at least theoretically progressive; if they are not, we '*reject*' them as 'pseudoscientific' ... We regard a theory in the series 'falsified' when it is superseded by a theory with higher corroborated content.<sup>2</sup>

If we add that series of theories, or research programmes, are connected by way of an irrefutable, by decision, hard-core, and that the *modus tollens* of refutation is directed at the protective belt of auxiliary assumptions surrounding the hard-core, then we have a fairly complete picture of Lakatos's MSRP.

This *seems* to present a very clear methodological picture of science.<sup>3</sup> We have a demarcation criteria: a necessary and sufficient condition of being scientific is that of

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<sup>2</sup> Lakatos, "Falsification ..." p. 118.

<sup>3</sup> I will ignore the qualifications which seem to be endemic in Lakatos's writings, that is, the excessive use of scare quotes around important terms seems to leave interpretation highly ambiguous. When we delve deeper into Lakatos's thought, many of these qualifications can be explicitly spelled out. Nevertheless, one can't help feeling that the qualifications assume the status of immunisation strategies.

theoretical progressiveness.<sup>4</sup> It seems to preserve the rationality of science, in that we have criteria with which to judge the actions of scientists: the acceptance of some theory T, was rational, in that T represented an empirically progressive problemshift; while the acceptance of theory T' was irrational, in that it represented a degenerating problemshift. Consequently, it seems to offer quite explicit advice to scientists: only propose theories which are theoretically progressive; do not propose, and reject other proposals of, theories which are theoretically degenerate.

But, as it turns out, this is not the case: Lakatos's system is neither clear, nor straightforward. Demarcation, rationality and advice become muddled notions, even *within* research programmes, where the problems should be easier to handle. For example, Lakatos inherits from Popper an abhorrence of 'conventionalist stratagems': moves made by a scientist to protect a theory from falsification; *ad hoc* manoeuvres. Lakatos identifies three different types of *ad hoc* stratagems, all "unfailingly pejorative".<sup>5</sup> *Ad hoc*<sub>1</sub> theories are theories which "had no excess content over their predecessors",<sup>6</sup> that is, they are not theoretically progressive. *Ad hoc*<sub>2</sub> theories are "theories which predicted novel facts but completely failed: none of their excess content got corroborated",<sup>7</sup> that is, they are not empirically progressive. And *ad hoc*<sub>3</sub> theories are theories which "do not form an integral part of the positive heuristic [of a research programme]".<sup>8</sup>

When we look at actual examples, we find that these three senses of *ad hocness*, far from being 'unfailingly pejorative', are tolerated, accepted and sometimes encouraged

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<sup>4</sup> But it is not so clear; in the next page after the quote in the text above, Lakatos contends that "A given fact is explained scientifically only if a new fact is explained with it." (italicised in the original) p. 119. Also, that "A theory without excess corroboration has no excess explanatory power; therefore, according to Popper, it does not represent growth and therefore is not 'scientific'; therefore, we should say, it has no explanatory power" fn.1. p. 119. That is, a theory is only scientific if it is, not just theoretically progressive, but also empirically progressive. This requirement, apart from the fact that it contradicts what Lakatos says in the passage quoted in the text above, leads to what are intuitively absurd results. The simple fact that a theory's novel predictions may not have been tested yet, implies that that theory is unscientific. I think it is best to stick with the idea presented in the passage above. But this starts to bring to light the general prevarication which we find in Lakatos.

<sup>5</sup> Lakatos, "Falsification ..." fn. 3. p. 175.

<sup>6</sup> *Ibid.*, fn.2. p. 175.

<sup>7</sup> *Ibid.*

<sup>8</sup> Lakatos, "History ..." fn. 36.

as an essential ingredient in scientific progress. According to Lakatos, *ad hoc*<sub>1</sub> theories are the worst possible theories: they do not even qualify as scientific. This should imply that no instances of *ad hoc*<sub>1</sub> theories are positively evaluated. But, as we saw in the previous chapter, many of the developments in Galileo's argument are *ad hoc*<sub>1</sub>; for example, the principle of circular inertia, which Lakatos admits is *ad hoc*<sub>1</sub>,

introduced no improvement since it did not forbid anything that had not been forbidden by the relevant theories he intended to improve upon (that is, by Aristotelian physics and by Copernican celestial kinematics). This theory was therefore *ad-hoc* and therefore- from the heuristic point of view- valueless.<sup>9</sup>

But, from an historical point of view, the principle of circular inertia was highly valuable in that it enabled Galileo's theories to be taken more seriously by his Aristotelian opponents, thereby creating "a breathing space"<sup>10</sup> for the emerging Copernican world-view. This idea of a 'breathing space' as a rationale for overlooking *ad hoc*<sub>1</sub> stratagems in new research programmes is repeated a number of times by Lakatos. For example, Lakatos contends that "some of the most important research programmes in the history of science were grafted on to older programmes with which they were blatantly inconsistent".<sup>11</sup> These inconsistencies are "usually concealed by *ad-hoc* stratagems ... the only purpose of which is to hide the 'deficiency'".<sup>12</sup> But, these stratagems "are harmless as long as they are clearly seen as problems, not as solutions";<sup>13</sup> but even more than that, "it may be *rational* to put the inconsistency into some temporary, *ad-hoc* quarantine, and carry on with the positive heuristic of the programme."<sup>14</sup> This seems to suggest, contrary to some of Lakatos's statements, that there is no real 'deficiency' in *ad hoc*<sub>1</sub> theories; hence the scare quotes above. Rather, there are times when the proposal of *ad hoc*<sub>1</sub> theories is rational, and also necessary for the continued growth of science.<sup>15</sup>

<sup>9</sup> Lakatos, "Falsification ..." p. 124.

<sup>10</sup> Lakatos, "History ..." p. 25.

<sup>11</sup> Lakatos, "Falsification ..." p. 142. Italicised in the original.

<sup>12</sup> *Ibid.*

<sup>13</sup> *Ibid.*, fn. 3.

<sup>14</sup> *Ibid.*, p. 143. My italics. Cf. also Lakatos, "History ..." pp. 24-5. where the existence and tolerance of *ad hoc* stratagems is seen by Lakatos as an advantage of MSRP over Popper's methodology.

<sup>15</sup> Here, I may mention that Lakatos's conception of *ad hoc*<sub>1</sub> hypotheses, dependent as it is upon content increase and novelty, does not do justice to *prima facie* acceptable moves in science. For example, the

As mentioned in footnote 4 above, Lakatos has presented two interpretations of *ad hoc*<sub>2</sub> theories. A version of Lakatos's more radical interpretation has been championed by Elie Zahar.<sup>16</sup> Zahar contends that theories are "*ad-hoc*<sub>2</sub> (at time t) if none of its excess content over its rivals has, at time t, been corroborated".<sup>17</sup> This does seem to be consistent with the passages from Lakatos I mentioned in fn. 4 above, and also brings with it an air of absurdity. If no-one had bothered to look, or thought of the possibility, that Einstein's General theory of Relativity could explain the previously anomalous precession in the perihelion of Mercury, then the General theory of Relativity would have been *ad hoc*<sub>2</sub> until the observation of the eclipses of 1919. Or, alternatively, if the precession in the perihelion of Mercury had *not* been an anomalous phenomena for Newtonian mechanics, again, the General theory of Relativity would have been *ad hoc*<sub>2</sub> until the eclipse observations! Consequently, because *ad hocness* is 'unfailingly pejorative', Einstein's General theory of Relativity was not good science. This seems to me to be an unacceptable conclusion: it seems irrational to label a theory unscientific at one point in time, but then to relabel the unchanged theory as eminently scientific at another. Apart from the fact that this does huge violence to our common-sense notions concerning *ad hocness*, the real problem concerns how Lakatos, given the conclusion above, can justify saying that *ad hocness*, in all of its varieties, is 'unfailingly pejorative'.

Lakatos's more moderate criterion of *ad hoc*<sub>2</sub> theories does not seem to carry these particular absurdities: *ad hoc*<sub>2</sub> theories are at least scientific. But even this conclusion creates problems; that is, in this situation, the dictum which proclaims, 'avoid *ad hoc* conventionalist stratagems', exhorts scientists to know in advance that some of the excess content of their theories will be corroborated; if they do not know that their theory will be corroborated, they are obliged to reject the theory, regardless of the fact

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latest theory, T, in a research programme, may be simpler than its predecessor, requiring fewer auxiliary hypotheses, while not possessing any excess content over its predecessor. Or, T may provide a physical interpretation for a previously uninterpreted mathematical formalism, again, without excess content. The point is that Lakatos's MSRP has nothing to say about these *prima facie* acceptable types of theories, except that they are unacceptable because they are *ad hoc*.

<sup>16</sup> E. Zahar, "Why Did Einstein's Programme Supersede Lorentz's?", in C. Howson, *Method and Appraisal in the Physical Sciences*, pp. 211-75.

<sup>17</sup> *Ibid.*, fn. 12. p. 216.

that the theory is, nevertheless, wholly scientific. This requirement seems to be unrealistic and, in many cases, unrealisable. If taken seriously, it would rule out of consideration important scientific theories simply because they had not yet been tested.<sup>18</sup>

When we come to *ad hoc*<sub>3</sub> theories, we find that the situation is very unclear. Recall that an *ad hoc*<sub>3</sub> theory is one which does not form an integral part of the positive heuristic of a research programme, or, as Zahar phrases it, a "theory is said to be *ad-hoc*<sub>3</sub> if it is obtained from its predecessor through a modification of the auxiliary hypotheses which does not accord with the spirit of the heuristic of the programme".<sup>19</sup> Lakatos developed this idea so as to rule out theories which are

not *ad-hoc* in the senses which we had earlier discussed. ... [they] may predict novel facts some of which may even be corroborated. Yet one may achieve such 'progress' with a patched up, arbitrary series of disconnected theories. Good scientists will not find such makeshift progress satisfactory; they may even reject it as not genuinely scientific.<sup>20</sup>

Consequently, theories which are not in the spirit of the heuristic of the programme are *ad hoc*<sub>3</sub> and must be avoided. At first glance, this requirement seems quite reasonable, however, upon reflection, and deeper reading, we find that Lakatos *encourages* the proposal of *ad hoc*<sub>3</sub> hypotheses. Lakatos contends that "it occasionally happens that when a research programme gets into a degenerating phase, a little revolution or a *creative shift* in its positive heuristic may push it forward again".<sup>21</sup> But what is a 'creative shift' in the heuristic of a programme, other than a shift occasioned by a proposal of an *ad hoc*<sub>3</sub> hypothesis?<sup>22</sup> I think the answer is that *ad hoc*<sub>3</sub> hypotheses can be the means in

<sup>18</sup> With regard to this particular point, Popper seems to have a better grasp of our common-sense notions of *ad hocness*. For Popper, any theory which is falsifiable is good science; the fact that a theory receives no corroboration of its excess content does not impugn this fact. For example, Popper applauds the theory of Bohr, Kramers and Slater, which, even though it was refuted immediately, epitomised the spirit of scientific theorising. CR, pp. 242-3 Popper contends that "One might well say that it is merely an historical accident if a theory is refuted after six months rather than after six years, or six hundred years". *Ibid.* But according to Lakatos it makes a big difference: it transforms a second-class, *ad hoc* theory, into a successful and laudatory example of scientific theorising.

<sup>19</sup> Zahar, "Why Did Einstein's ...", p. 217.

<sup>20</sup> Lakatos, "Falsification ...". p. 175.

<sup>21</sup> *Ibid.*, p. 137.

<sup>22</sup> Cf. A. Musgrave, "Method or Madness?: Can the Methodology of Research Programmes be Rescued From Epistemological Anarchism?", in R.S. Cohen, P.K. Feyerabend and M.W. Wartofsky (eds) *Essays in Memory of Imre Lakatos*, pp. 457-91. "Lakatos was led by Noretta Koertge to admit that 'creative shifts' were an anomaly for him, since any creative shift is *ad-hoc*<sub>3</sub> in his sense ... He hoped to accommodate this meta-anomaly in a non-*ad-hoc* fashion" n. 21. p. 484.

which degenerating programmes are revived; or the means by which new research programmes arise out of older programmes. For example, Lakatos contends that "Planck's original '*ad-hoc*' formula ... could be explained *progressively* within the new quantum theoretical programme, while ... [it] could be explained within the classical programme ... at the price of a degenerating problemshift",<sup>23</sup> that is, the rise of one of the most successful research programmes in scientific history was partly occasioned by the proposal of an *ad hoc*<sub>3</sub> hypothesis. In general, we can conclude that the advice to avoid *ad hoc* hypotheses would have significantly impaired the development of science, and, what is worse, there does not seem to be any principled way in which Lakatos can delineate between good and bad *ad hoc*<sub>3</sub> hypotheses.<sup>24</sup>

### 2.2.3 Theory Appraisal: Inter-Research Programme.

So much for rationality of decisions made *within* particular research programmes. If we now turn to the question of what the relationships are between different research programmes, the unclarity of Lakatos's MSRP is compounded. The idea of series of theories, or research programmes, was proposed by Lakatos to recover Popperian insights embarrassed by the fact that new research programmes are invariably born refuted; and often exhibit a reduction in content compared to their established rival. As we saw above, Lakatos "regard[s] a theory in the series 'falsified' when it is superseded by a theory with higher corroborated content."<sup>25</sup> Falsification and subsequent rejection, instead of constituting a universal *must* of scientific procedure, becomes a notion with normative power only with respect to successive theories within a series of theories, or research programme. Collaterally, the requirement of content increase is reserved for the relationship between successive theories within a research programme. A new research

<sup>23</sup> Lakatos, "Falsification ..." p. 167. Lakatos gives Planck's formula as a paradigm of an *ad hoc*<sub>3</sub> hypothesis, *ibid.*, fn. 3. p. 175.

<sup>24</sup> Logically, Lakatos's taxonomy of *ad hocness* entails *five* different types of *ad hocness*, rather than the three mentioned. We have *ad hoc*<sub>1</sub> and *ad hoc*<sub>2</sub> as explained in the text above; *ad hoc*<sub>1</sub> plus *ad hoc*<sub>3</sub>, that is, a theory not in the spirit of the heuristic which has no excess content; *ad hoc*<sub>2</sub> plus *ad hoc*<sub>3</sub>, that is, a theory not in the spirit of the heuristic, but with excess content; and, what we might call *pure ad hoc*<sub>3</sub>, that is, not in the spirit of the heuristic, but with corroborated excess content. Can this help Lakatos? Not at all.

<sup>25</sup> Lakatos, "Falsification ..." p. 118.



programme may exhibit dramatic loss of content, in comparison to its established rival, and may also face large amounts of unexplainable anomalies. But these considerations are irrelevant: all that matters is if the new research programme can be seen to be producing a progressive problemshift.

These manoeuvres seem to preserve Popper's ideals, while at the same time incorporating historical reality. Consequently, 'Rationalist' philosophy can preserve our basic value judgments concerning the rationality of science, utilising universal, a-temporal standards, to create a veridical rational reconstruction of the history of science. But this only seems to shift the problem from the evaluation and appraisal of individual theories, to the evaluation and appraisal of research programmes. Though the demarcation problem may be the same, the question of the rational preferability of research programmes, and the question of what advice can we give to scientists as to which research programme to pursue, now have to be answered anew.

At times Lakatos seemed to imply that the framework outlined above for the rational appraisal of theories within research programmes could be applied to the rational appraisal of competing research programmes. For example, Lakatos contends that

I give criteria of progress and stagnation within a programme and also rules for the 'elimination' of whole research programmes ... If a research programme progressively explains more than a rival, it 'supersedes' it, and the rival can be eliminated.<sup>26</sup>

But this hope was never realised: "One may rationally stick to a degenerating programme until it is overtaken by a rival *and even after*."<sup>27</sup> The reasons for this are summed up by Feyerabend very nicely:

Considering a research programme in an advanced state of degeneration one will feel the urge to abandon it, to replace it by a more progressive rival. This is an entirely legitimate move. *But it is also legitimate to do the opposite* and to retain the programme. For any attempt to demand its removal on the basis of a *rule* can be criticized by arguments almost identical with the arguments that led to the 'securing of a breathing space' in the first place. (AM, p. 185)

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<sup>26</sup> Lakatos, "History ..." p. 11.

<sup>27</sup> *Ibid.*, p. 15.

If a new 'budding' research programme is allowed a breathing space where inconsistent foundations, loss of content, apparent refutations and *ad hoc* stratagems, are all tolerated because of future promise, heuristic fruitfulness, then an old programme, currently degenerating, should be allowed the same leeway to recover its future promise. After all, as Lakatos states, "programmes may get out of degenerating troughs".<sup>28</sup>

Feyerabend contends that if we allow a breathing space for every research programme, then the standards of rational appraisal are vacuous: they do not forbid any action whatsoever, they are mere "verbal ornaments".<sup>29</sup> On the other hand, if we impose some sort of time limit upon acceptable periods of degeneration, then we are left with two problems (i) Any specific demarcation line will be arbitrary: "if you are permitted to wait, why not wait a little longer?"<sup>30</sup> And (ii) It is probable that history won't oblige you with neat corroboration; for example, heliocentrism and atomism. Lakatos's response to this is to deny that methodology is in the business of giving advice to scientists: "both Feyerabend and Kuhn conflate *methodological* appraisal of a programme with firm *heuristic* advice about what to do",<sup>31</sup> while, for Lakatos, "*methodology* is separated from *heuristics*, rather as value judgments are from 'ought' statements".<sup>32</sup>

Lakatos is now on the slippery slope towards Feyerabend's *reductio*. The idea that methodology is separated from heuristics seems to contradict Lakatos's own *methodology* of scientific research programmes. To say that new theories in a series of theories within a research programme are arrived at via the negative and positive heuristic of that research programme, and must exhibit content increase over its predecessor, is to provide a *heuristic* programme. Lakatos's 'methodology' is 'heuristics' through and through. How can we separate the appraisal of theories from the acceptance or rejection of those theories? Consider a scientist working on a new theory. The scientist goes through the process of formulating and refining that theory, presents the

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<sup>28</sup> Lakatos, "Falsification ... " p. 164.

<sup>29</sup> Feyerabend, "Consolations for the Specialist", in Lakatos and Musgrave, (eds) *Criticism and the Growth of Knowledge*, p. 215.

<sup>30</sup> *Ibid.*

<sup>31</sup> Lakatos, "History ..." pp. 15-16.

<sup>32</sup> *Ibid.*, fn. 2. p. 2.

theory to his peers who reject it out of hand because it either does not follow from the research programme, does not exhibit content increase, or is *ad hoc*. If Lakatos's MSRP is a valid methodological guide for the appraisal of theories, then a scientist will consider the criteria of appraisal as a *precondition* of theory construction: the scientist will not expend energy on proposing theories which will be dead in the water. The criteria of appraisal are, or should be, the heuristic guides to theory construction.<sup>33</sup> Just as the putative distinction between value statements and ought statements, which, admittedly, can be made, is simply a verbal distinction of no substantive content: who could deny that there are deep and profound connections between value statements and ought statements? So too the attempt to distinguish appraisal and advice amounts to no more than a verbal distinction. If something is rational to do, then there is heuristic force behind the do-ing of that action.

Lakatos makes his distinction between methodology and heuristics to save his theory from refutation. It is an obvious fact, as we saw above, of scientific history that Lakatos's criteria are often, fruitfully, transgressed. If Lakatos has presented his criteria as heuristic guides which must, or should, be followed to produce good science, then the counter-examples would have crushed his programme. Instead, Lakatos hedges his bets, preserving his theory in a purely *ad hoc* manner in order to save his philosophical programme. But if it is rational to stick to degenerating programmes, if it is rational to propose *ad hoc* theories, and no criterion can be given as to when these moves are irrational, then it is hard to see that there is any normativeness, of the 'Rationalist' variety, in rationality. One is forced to agree with Feyerabend that, for Lakatos, "Any choice of the scientist is rational, because it is compatible with ... [his] standards" (AM, p. 186) and that Lakatos abandons "permanent standards *in fact* though retaining them *in words*".<sup>34</sup> That is, Lakatos's rhetoric is of 'Rationalism', while his actual position is one which is in fact anti-'Rationalistic'.<sup>35</sup>

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<sup>33</sup> Cf. Musgrave, "Method or Madness?", pp. 474-6.

<sup>34</sup> Feyerabend, "Consolations ..." p. 216.

<sup>35</sup> Cf. Feyerabend, AM, pp. 186-7.

If Lakatos's point is that some actions are *more* rational than others, that rationality is not something categorical, but hypothetical and diverse, then that can be accepted.<sup>36</sup> But in that case there should be no heuristic restriction upon adoption of any one of the range of actions which have some degree of rationality; this for two reasons: (i) If it is rational to do something, then there is no reason to *not* do it. And (ii) Judgments as to degree of rationality of various alternative actions may be awry. If everyone followed what was considered to be the most rational course of action, then the possibilities inherent in the other courses of action will not be followed. Consequently, fruitful lines of research will be ignored, to the possible detriment of the progress of knowledge.<sup>37</sup>

But this is not Lakatos's response to the conclusion that judgments of rationality have no heuristic force.

This does not mean as much licence as might appear for those who stick to a degenerating programme. For they can do this mostly in private. Editors of scientific journals *should* refuse to publish their papers which will, in general, contain either solemn reassertions of their positions or absorption of counterevidence ... by *ad-hoc*, linguistic adjustments. Research foundations, too, *should* refuse money.<sup>38</sup>

That is, in the absence of rational advice, science is determined by *sociological* power structures. And, it may be added, where does the justification of the two 'shoulds' above come from? These 'shoulds' find no legitimation within Lakatos's philosophy. Lakatos goes on to say that "I do, of course, *not* claim that such decisions [to refuse to publish; to withhold funds] are necessarily uncontroversial. In such decisions one has to use also

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<sup>36</sup> For a good discussion of categorical rationality, see R.N. Giere, "The Cognitive Study of Science", in N. Nersessian (ed) *The Process of Science: Contemporary Philosophical Approaches to Understanding Scientific Practice*, esp. pp. 143-4.

<sup>37</sup> The solution implied here is one which Musgrave, following Kuhn, adopts. That is, if we cannot give specific advice to individual scientists as to the research programme they should pursue, we can nevertheless give advice to the community of scientists: research programme R seems to be the most rationally pursuable alternative, therefore, most scientists should work on it. Research programme R', is degenerating, therefore, fewer scientists should work on it. See Musgrave, "Method or Madness?" pp. 479-80.

<sup>38</sup> Lakatos, "History ..." p. 16. My emphasis.

one's *commonsense*".<sup>39</sup> Lakatos goes on to say that commonsense is actually informed by particular philosophical predispositions and he contends that

My [commonsense] judge directs the jury to agree on appraisals of progressive and degenerating research programmes ... although it is important to reach agreement on such verdicts, there must also be the possibility of appeal. In such appeals inarticulated commonsense is questioned, articulated and criticised.<sup>40</sup>

That is, if we are to appeal against the decision which places a degenerating research programme on the outer, we must use commonsense. But here 'commonsense' means decisions made in accordance with the criteria which produced the controversial decision in the first place. Commonsense is posited as something *different* than the criteria of MSRP, but for Lakatos, it is not. In any case, Lakatos actively countenances the shunning of scientists working within degenerating research programmes: they can't get published, they can't get funding, they are effectively stifled. How then can they make themselves heard in a court of appeal? Especially when 'commonsense' itself denies the validity of their actions.

#### 2.2.4 Feyerabend's Reductio of MSRP.

We can see now that the position of Lakatos is looking increasingly desperate. Of the three questions we began this analysis with, which Lakatos wants to answer positively, we have found that his own proposals designed to ensure positive answers have, in fact, led to negative answers to the questions concerning advice and rationality. The positive answer to the demarcation question is also looking very shaky. But, as we will see, the incoherence of Lakatos's MSRP has not yet been fully diagnosed.

Feyerabend asks us to entertain the idea of epistemological anarchism. In this he distinguishes epistemological anarchism from positions with which it may be confused.

Epistemological anarchism differs both from scepticism and from political (religious) anarchism. While the sceptic either regards every view as equally good, or as equally bad ... the epistemological anarchist has no compunction to defend the most trite, or the most outrageous statement.

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<sup>39</sup> *Ibid.*, fn. 58, p. 16.

<sup>40</sup> *Ibid.*

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. (AM, p. 189)<sup>41</sup>

Feyerabend distinguishes epistemological anarchism from both scepticism and political/religious anarchism. This is an important point to keep in mind: many criticisms of Feyerabend's epistemological anarchism have failed to appreciate that he distinguishes epistemological anarchism from scepticism. For example, Musgrave contends that epistemological anarchism is "the thesis that any theory or research programme is as good as any other".<sup>42</sup> But this is the position Feyerabend labels scepticism; a position Feyerabend unequivocally *contrasts* with epistemological anarchism. Though the statement of Feyerabend's that "the epistemological anarchist has no compunction to defend the most trite, or the most outrageous statement" may be considered to be tantamount to scepticism, at this point we should take Feyerabend's words at face value and suppose that he has further, cogent, reasons for distinguishing epistemological anarchism from scepticism.<sup>43</sup>

Feyerabend goes on to say that

The one thing he [the epistemological anarchist] opposes positively and absolutely are universal standards, universal laws, [and] universal ideas ... though he does not deny that it is often good policy to act as if such laws (such standards, such ideas) existed, and as if he believed in them. (AM, p. 189).

Feyerabend then asks us to imagine an epistemological anarchist living at the time of the Copernican revolution:

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<sup>41</sup> This passage, and the ones surrounding it, can be used against Watkins and Gellner, who accuse Feyerabend of implicitly supporting political anarchism and violence. Feyerabend does indeed mention violence, but this is said in relation to political anarchism. *Feyerabend is contrasting political anarchism and epistemological anarchism*. Part of the contrast is that violence is not a part of epistemological anarchism. As the quote above implies, an epistemological anarchist does not want to remove or destroy anything; they may become useful at some future date. For Watkins assertions see his "Corroboration and the Problem of Content Comparison", in *Ibid.*, pp. 339-78. esp. 339-44. For Feyerabend's vindicated outrage, see his "The Gong Show - Popperian Style", in *Ibid.*, pp. 387-92.

<sup>42</sup> Musgrave "Evidential Support, Falsification, Heuristics, and Anarchism", in G. Radnitzky and G. Andersson (eds) *Progress and Rationality in Science*, p. 192.

<sup>43</sup> Feyerabend's epistemological anarchism and the factors which distinguish it from scepticism are discussed further in Section 2.3 and Section 7.6.

What will be his attitude? What moves will he recommend? What moves will he oppose? What will he say? What he will say depends on his interests, on the 'social laws', the social philosophy, the opinions concerning the contemporary scene he has decided to adopt *for the time being*. (AM, p. 191).

Through a series of hypothetical situations (AM, pp. 191-5) Feyerabend shows that an epistemological anarchist could adopt any position whatsoever, depending upon the personal predilections of the hypothetical anarchist. This conclusion seems to be correct: the way in which Feyerabend has constructed epistemological anarchism entails that no outward signs, no specific behaviour or thoughts, can distinguish an epistemological anarchist from a 'Rationalist'.

Feyerabend then asks whether activity governed by Lakatos's standards can be distinguished from the "intellectual freelancing of the anarchist". (AM, p. 196) The answer is that there is so much leeway built into Lakatos's standards, as a consequence of which, they can not distinguish the activities of an anarchist from a 'Rationalist'. This is not a happy conclusion for Lakatos. It leaves open the possibility that the entire history of science could be a chronicle of the actions of opportunistic epistemological anarchists, which somehow, nevertheless, through some sort of cunning of reason and history, led to rational results. This possibility goes against Lakatos's entire programme: Lakatos wants to write internal, rational, world<sub>3</sub> history; thereby excluding to a minimum the amount of external, non-rational, world<sub>2</sub> history necessary to complement the internal account.<sup>44</sup> But the possibility of an anarchist history makes the progress and process of science wholly dependent upon psycho-social, world<sub>2</sub> entities.

This conclusion is further enhanced when we consider the fact that we don't need the hypothetical assumption of an anarchistic history of science to show that Lakatos's MSRP, far from showing science to be 'internal', inevitably leads us to 'external' explanations of the history of science. If all moves in science are, to some extent, rational, then the decisive factors which lead to cessation of work on a particular research programme, and the beginning of work on another, different research

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<sup>44</sup> This is the rationale behind Lakatos's "History ..." article, and his historiographical research programme.

programme, are *necessarily external*. Otherwise, research programmes would never be abandoned. The MSRP is ambiguous enough such that any move can be justified according to its standards. As Feyerabend contends, we can either adopt a conservative attitude to the standards, where the simple fact that a research programme is degenerating is reason enough to abandon it, or we can adopt a liberal attitude towards the standards, where the fact of degeneration has no effect upon the acceptance or rejection of a research programme. (AM, pp. 196-200) The standards themselves are not the prime consideration here: the standards are but vehicles through which particular, conservative or liberal, psycho-social predilections are justified.

It follows that the fight between the conservatives and the liberals and the final victory of the conservatives is not a 'rational change' but a 'power struggle' pure and simple, full of 'sordid personal controversy'. It is a topic not for methodology, or for the theory of rationality, but for 'mob psychology'. (AM, p. 199)

I don't see how Lakatos can avoid these conclusions, especially considering that Lakatos explicitly adopted a conservative attitude himself. Lakatos's support for the idea that journals should refuse to publish, and foundations refuse to fund, degenerating research programmes, cannot be interpreted in any way other than a personal preference for a conservative attitude towards his own standards.<sup>45</sup>

Lakatos contends that "progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational [internal]",<sup>46</sup> that is, the more history a particular methodology can rescue and explicate as rational (internal), the better the methodology. Thus, whereas other methodologies: falsificationist, inductivist, conventionalist, have to bring in external, non-rational explanations where their methodologies cannot explicate particular scientific episodes as rational, the MSRP is better in that it can reconstruct

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<sup>45</sup> In this respect, it is interesting to consider the contention of M. Grene that Lakatos reached his conclusions concerning his methodology of research programmes because "science depends on public or industrial support; so we have to develop a conceptual apparatus that will show which research programme is likely to pay off" M. Grene, "Imre Lakatos: Some Recollections", in Cohen, *et al*, *Essays in Memory of Imre Lakatos*, p. 210. In other words, Lakatos was concerned with science policy, not with rationality or methodology.

<sup>46</sup> Lakatos, "History ..." p. 32. Italicised in the original.



those same episodes without introducing additional external explanations. But, in the light of what has been discussed above, this is not a very surprising or profound achievement: if any move a scientist makes can be explicated as rational, then all of science becomes internal. Where others see 'Newton-worship' in relation to the prolonged acceptance of the corpuscular theory of light, research programmers see rational adherence, despite the success of the wave programme, to an intermittently progressive research programme.<sup>47</sup> The details of particular cases are neither here nor there; the general point to be made is that it is hard to conceive of a situation which could not be explicated internally by MSRP. I think it is significant that in all of Lakatos's own case studies, and that of his followers, the admission of external factors is non-existent.<sup>48</sup> Where Lakatos would admit external explanations are in the case of extremely wild situations:

Let us imagine ... that in spite of the objectively progressing astronomical research programmes, the astronomers are suddenly all gripped by a feeling of Kuhnian 'crisis'; and then they are all converted, by an irresistible *Gestalt-switch*, to astrology. I would regard this catastrophe as a horrifying *problem*, to be accounted for by some empirical externalist explanation.<sup>49</sup>

But even here, what justification, in terms of Lakatos's standards, can be given for Lakatos's horror? If astrology received new impetus such that it became progressive, then there is no valid, Lakatosian, justification for the horror evinced.

In fact, as a testament to the outright external character of MSRP, and its absurd internal incoherence, Lakatos claims that the sophisticated falsificationist

sees nothing wrong with a group of brilliant scientists conspiring to pack everything they can into their favourite research programme ... with a sacred hard core. As long as their genius - and luck - enables them to expand their programme '*progressively*', while sticking to its hard core, they are allowed to do it. And if a genius comes determined to *replace* ('progressively') a most uncontested and corroborated theory which he happens to dislike on philosophical, aesthetic or personal grounds, good

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<sup>47</sup> Cf. J. Worrall, "Thomas Young and the 'Refutation' of Newtonian Optics: A Case Study in the Interaction of Philosophy of Science and History of Science", in Howson, *Method and Appraisal in the Physical Sciences*, pp. 107-79.

<sup>48</sup> See Howson, *Method and Appraisal in the Physical Sciences*.

<sup>49</sup> Lakatos, "History ... " p. 34.

luck to him. If two teams, pursuing rival research programmes, compete, the one with more creative talent is likely to succeed ... The direction of science is determined primarily by human creative imagination and not by the universe of facts which surround us. Creative imagination is likely to find corroborating novel evidence even for the most 'absurd' programme, if the search has sufficient drive ... Scientists dream up phantasies and then pursue a highly selective hunt for new facts which fit these phantasies ... A brilliant school of scholars (backed by a rich society to finance a few well-planned tests) might succeed in pushing any fantastic programme ahead, or, alternatively, if so inclined, in overthrowing any arbitrarily chosen pillar of 'established knowledge'<sup>50</sup>

But scientists are not allowed to pursue the 'fantastic' programme of astrology on pain of being externally explained? According to the passage above, with enough creative imagination and money, astrology could *overthrow* the established astronomical order. But the passage is important in another respect: the rational pursuability of any research programme is dependent upon the creativeness and imagination of individual scientists. To determine the rational acceptability of any particular research programme, we don't have to examine the substantive theories proposed in rival programmes, all we need do is conduct psychological studies to ascertain the respective creativeness and imaginativeness of the scientists involved. The objective merits of a research programme is not an issue: creative scientists *make whatever they work on progressive*. Therefore, nothing can objectively entice a scientist to work on a specific research programme, however, money, funding and prestige can.

### 2.2.5 Content Increase and the Circularity of MSRP.

Feyerabend makes one further criticism of Lakatos: the idea of content increase and its associated concept of novelty is arrived at in a circular manner, and is therefore in need of deeper justification than that which is offered by Lakatos.<sup>51</sup> This criticism is offered in two parts, (i) Lakatos's standards are circular, in that he derives them from the very same scientific episodes from which he seeks corroboration. And (ii) no independent justification is given for these standards.

<sup>50</sup> Lakatos, "Falsification ... " pp. 187-8. I quoted this passage at length for its sheer audacity, and for its all too obvious contradiction of so many of Lakatos's other passages.

<sup>51</sup> Cf. AM, pp. 201-8. and Feyerabend, "In Defence of Aristotle: Comments on the Condition of Content Increase", in G. Radnitzky and G. Andersson, (eds) *Progress and Rationality in Science*.

Firstly, the circularity arises when we consider the fact that Lakatos wants to provide a system which can explain the success and rationality of science. But the standards which Lakatos proposes are said to be taken from the history of science: Lakatos accepts the "basic value judgments"<sup>52</sup> of scientists as to what has been great science. He then contends that his MSRP is able to give a rational account of those episodes in science *from which his system was created*. If we ask the question, 'What's so great about science?' The answer is that it follows the rational standards of MSRP. If we then ask, 'What's so great about these standards?' The answer is, scientists follow them.<sup>53</sup> It is Feyerabend's contention that Lakatos gives no *independent* arguments for the validity or preferability of his standards.

When basic value judgments not addressed by Lakatos are brought up, which are difficult for Lakatos to explain; for example, the basic value judgment that Darwin's theory of evolution was a great scientific step forward, we are given replies that impugn the rationality of the scientists involved. Darwinian evolution, as promulgated by Darwin, had no specific predictive power: there were no testable consequences. The vast geological time-scales which Darwin evoked as necessary for the process of evolution, and which therefore constituted part of the hard-core of Darwinian evolution, effectively ruled out any detailed predictions. Therefore, according to Lakatos, Darwinian evolution was *ad hoc*<sub>1</sub>. Not only that, but the huge number of anomalies; for example, gaps in the fossil record, while not in themselves indicting evolution as pseudo-scientific, can be seen as a major problem for Darwin's doctrine of evolution by selection of small effects: 'saltationists' and Lamarckians could more easily, with more faithfulness to the empirical data, explain the gaps in the fossil record. The resources of Darwinian theory to deal with these, and other, problems, was greatly curtailed in that they were only explicable, according to Darwinian selectionist criteria, in terms of a very weak heuristic.

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<sup>52</sup> Lakatos, "History ..." p. 31.

<sup>53</sup> Cf. N. Koertge, "Rational Reconstructions" in Cohen, *et al*, *Essays in Memory of Imre Lakatos* esp. pp. 363-4. where Koertge indicts Lakatos for preselecting the historical episodes which are intended to be the tests of historiographical theories (reconstructions), that is, Lakatos selects those episodes which can best be explicated via his account.

The weak heuristic, according to Lakatosian conceptions, of the original Darwinian program is very striking, for two reasons. Firstly, the most fundamental hard-core assumption of Darwinian evolution is the idea of natural selection. But the mechanics of inheritance and variation, which underscored the idea of natural selection, were completely unknown at the time. The maxim that 'like begets like' passed as knowledge concerning inheritance; as Darwin stated,

The laws governing inheritance are quite unknown; no one can say why the same peculiarity in different individuals of the same species, and in individuals of different species, is sometimes inherited and sometimes not so; why the child often reverts in certain characters to its grandfather or grandmother or other more remote ancestor; why a peculiarity is often transmitted from one sex to both sexes, or to one sex alone, more commonly but not exclusively to the like sex.<sup>54</sup>

When we come to the mechanics of variation there is again general lack of knowledge. Darwin states that "I have ... spoken as if the variations ... had been due to chance. This, of course, is a wholly incorrect expression, but it serves to acknowledge plainly our ignorance of the cause of each particular variation."<sup>55</sup> he then goes on to conjecture that "the reproductive system is eminently susceptible to changes in the conditions of life; and to this system being functionally disturbed in the parents, I chiefly attribute the varying or plastic condition of the offspring."<sup>56</sup> Thus, two central assumptions crucial to the idea of natural selection were left as virtual black-boxes. Possible avenues of development are left wide open and no specific testable predictions are given.

Secondly, and more importantly, "Darwin did not repudiate explanations of the Lamarckian kind altogether, as is sometimes assumed, and there are numerous references to the effects of use and disuse, and the inheritance of acquired characteristics in *The Origin*."<sup>57</sup> That is, even though Darwin promulgated the idea that natural selection can account for speciation and variation, from the very beginning he softened his thesis by

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<sup>54</sup> C. Darwin, *The Origin of Species by Natural Selection or The Preservation of Favoured Races in the Struggle for Life*, J.W. Burrow (ed) p. 76.

<sup>55</sup> *Ibid.*, p. 173.

<sup>56</sup> *Ibid.*

<sup>57</sup> J. Burrow, "Editor's Introduction" in *The Origin of Species*, p. 37. For a representative passage from Darwin, see the section entitled "Effects of Use and Disuse" pp. 175-9.

admitting the possibility of limited Lamarckian explanations. This maneuver substantially lowers the falsifiability of Darwinian evolution by introducing the possibility of *ad hoc* stratagems: if some new evidence is put forth which seems to impugn natural selection, Darwin can turn around and explain away the anomalous evidence by invoking Lamarckian explanations.

In fact, this possible use of Lamarckian considerations to offset problems became actual. Two problems necessitated this eventuation. Firstly, "a physicist, the future Lord Kelvin, ... proposed to reduce the estimated age of the earth ... to a hundred million years or less. This estimate was profoundly damaging to Darwin's supposition of the slow accumulation of chance variations".<sup>58</sup> That is, natural selection required a far longer span of time to account for evolution. Secondly, the mechanics of variation was beset with problems: "The current theory was that the characters of each parent were 'blended' in the offspring ... in that case the chances that important variations would be transmitted, and not blended away, were minute".<sup>59</sup> Consequently, Darwin "in later editions of *The Origin* began to lay more stress than he had done at first on the direct influence of the environment, and on the inheritance of acquired characteristics, as Lamarck had done."<sup>60</sup>

In summary, we can see that the comprehensive character of evolutionary theory, its 'metaphysicalness', would seem to suggest that, in Darwin's formulation, no conceivable evidence could contradict it. Additionally, as evidenced by Darwin's weakenings, the positive heuristic was not followed consistently. Consequently, the theory of evolution constituted an exceedingly degenerating research programme. However, contrary to Lakatos, evolutionary theory survived throughout its putative degeneration.<sup>61</sup> M. Grene gives an anecdotal account where she asked Lakatos about

<sup>58</sup> J. Burrow, "Editor's Introduction", p. 46.

<sup>59</sup> *Ibid.*, p. 47.

<sup>60</sup> *Ibid.*, Burrows explains that the first edition of the book is published, even though Darwin brought out six editions in his lifetime, because of the subsequent editions' concessions to Lamarckian evolution. See the "Note on This Edition" p. 49. For a general discussion of the difficulties outlined above, see M. Ruse, *The Darwinian Revolution*, ch. 8. and P.J. Bowler, *Evolution: The History of an Idea*, ch.7.

<sup>61</sup> Lakatosians may nevertheless feel encouraged by the fact that the Darwinian natural selection explication of evolution was on shaky ground in the late nineteenth and early twentieth centuries, being challenged by neo-Lamarckian, orthogenetic and Mendelian explications of evolution. But this situation was the result of Neo-Darwinians pushing for the exclusive acceptance of natural selection as the mechanism of evolution; something Darwin himself never promulgated. Ironically, for Lakatos, when

Darwin; Lakatos's reply, "I have a student whose dissertation will show that Darwin was a lousy scientist".<sup>62</sup> This can be construed as further evidence that Lakatos wants his standards to be interpreted conservatively: he wants them to have a 'Rationalist' character. But this creates a dilemma for Lakatos: if we interpret his standards conservatively, then Darwin is labelled as irrational and Darwinian evolution should have been abandoned. But this creates a major counter-example to his own project of capturing the basic value judgments of scientists; and thus a major counter-example to his historiographical research program. On the other hand, if we interpret Lakatos's standards liberally, then Darwin and his followers were rational because research programs can get out of degenerating troughs. But this too creates problems for Lakatos: it seems to rob Lakatos's MSRP of any 'Rationalist', universalistic bite.

If we turn now to point (ii), Feyerabend argues that the standards of MSRP, for example, that of content increase and the essential role novelty plays in it, are not given independent justification. Feyerabend contends that if we look at the Aristotelian world-view, we find that the idea of content increase and novelty have no role in that system.<sup>63</sup> Much of Aristotle's thought is built around the ideas that the material world is finite and continuous. These two deep cosmological-metaphysical assumptions structure the entire Aristotelian world-view. Within this world-view, which is backed by good philosophical and empirical arguments, it follows that content increase and novelty have no place: the basic value judgments of Aristotelianism are antithetical to such notions. Consequently, it is no use, Feyerabend claims, in saying that Aristotelianism is therefore unacceptable: the acceptability or unacceptability of content increase is the issue under discussion.

why is the Aristotelian procedure unacceptable? The only legitimate reason that might be given at *this* stage of the argument would be a difficulty for Aristotelianism that is *independent* of its being a 'degenerate research

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Darwinians remained within the mildly immunised structure, with its correspondingly weak heuristic, set out by Darwin, Darwinism survived. However, once natural selection was set up without any Lamarckian fall-back positions, it ran the risk of dropping out of science altogether. For a discussion of these points see P. J. Bowler, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades Around 1900*, and his *Evolution*, ch. 9.

<sup>62</sup> M. Grene, "Imre Lakatos ..." p. 211.

<sup>63</sup> See Feyerabend, "In Defence of Aristotle", pp. 144-54. For his analysis of Aristotelianism.

programme'. We certainly do not want to take content increase for granted, we want to argue for it and against its opponents.<sup>64</sup>

If we look at the reasoning behind the adoption of content increase in science, we find that

the idea that nature is infinitely rich both qualitatively ... and quantitatively leads to the desire to make new discoveries and thus to the principle of content increase which gives another standard to judge theories by: theories that have excess content over what is already known are preferable over theories that have not.<sup>65</sup>

That is, underlying the standards of science are two deep cosmological-metaphysical principles: the world is infinite and discontinuous. *Given these assumptions*, given these basic value judgments, content increase and novel predictions for successive theories are then natural extrapolations. So the question remains, whose set of basic value judgments are preferable?

the restless change of modern science that announces itself with Galileo, its loose use of concepts, its refusal to accept customary norms, its 'unempirical' procedures violated the professional ideology of the Aristotelians and was an example of incipient degeneration *for them*. In forming this judgement the Aristotelians made use of *their* general philosophy, *their* desiderata ... and of the basic value judgements of *their* science. (AM, p. 207.)

Consequently, anyone, like Lakatos, who bases their principles of rationality upon accepted basic value judgments, is ultimately engaged in a process of question-begging. Unless further arguments can be developed, not reliant upon the value judgments in question, then the position is defended in a circular manner, and is correspondingly weaker for that very reason.

The point of this discussion is to see that Feyerabend contends that, in analysing science and rationality, we must see the values of science as arising out of deep cosmological-metaphysical world-views. Arguments concerning rationality must be traced back to this deepest level, if they are not, arguments appear dismissive and reliant upon power.<sup>66</sup>

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<sup>64</sup> *Ibid.*, p. 153.

<sup>65</sup> *Ibid.*, p. 164.

<sup>66</sup> This idea will be explored further in chapter 7. It must be remembered that Feyerabend is not claiming that Aristotelianism has never been adequately addressed by the proponents of modern science,

### 2.3 Conclusion.

Feyerabend contends that Lakatos's MSRP is internally incoherent, in that the objectives which Lakatos was trying to fulfil: demarcation, internal rationality, and heuristic advice, by the adoption of his standards, were not able to be achieved. In fact, the standards meant to fulfil the objectives were the reason and cause of those objectives not being met. In this sense, we can say that Lakatos's MSRP, according to its own standards, constitutes a degenerating research programme.<sup>67</sup> In this situation, I think we can see that Feyerabend's 'anarchism' forms part of his *reductio ad absurdum* of 'Rationalist' philosophies, in particular, Lakatos's 'Rationalist' philosophy: 'Rationalists' contend that without 'Rationalist' theses to guide us, all that is left is anarchism. The question still remains, however, does Feyerabend welcome such a conclusion? That is, is Feyerabend's anarchism really anarchism, in the usual sense of the word?

Firstly, we only have to look at appendix 3 of AM (pp. 215-20) to see that Feyerabend's anarchism does not imply chaos and complete disorder. Here Feyerabend states that "Science needs people who are adaptable and inventive, not rigid imitators of 'established' behavioural patterns." (AM, p. 215) He then goes on to say that this is what he means by 'Anything goes', that is, that is what he means by anarchism. Well, that isn't really anything goes: being adaptable and inventive is consistent with an ordered life-style and thought. That anything goes is not Feyerabend's own philosophy can be easily seen when he then goes on to discuss 'intellectual pollution':

Illiterate and incompetent books flood the market, empty verbiage full of strange and esoteric terms claims to express profound insights, 'experts' without brains, without character, and without even a modicum of intellectual, stylistic, emotional temperament tell us about our 'condition' and the means for improving it. (AM, p. 219)

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or that the modern scientific world-view cannot marshal formidable arguments against Aristotle. What is claimed is that *Lakatos*, and other 'Rationalists', do not produce these arguments.

<sup>67</sup> Cf. M. Pera, "Methodological Sophisticationism: A Degenerating Project.", in K. Gavroglu, Y. Goudaroulis and P. Nicolacopoulos (eds) *Imre Lakatos and Theories of Scientific Change*, pp. 169-87.



It is not anything goes, it is simply a freeing of intellectual standards, not in the sense of allowing sloppy or inane viewpoints accessible to everyone, but in the sense of allowing alternate standards the leeway to develop their point, thus Feyerabend states that

the change of perspective [in a liberal education] makes it clear that there are many ways of ordering the world that surrounds us, that the hated constraints of one set of standards may be broken by freely accepting standards of a different kind, and that there is no need to reject *all* order and to allow oneself to be reduced to a whining stream of consciousness.(AM, p. 218)

We cannot reject all order, that is not the hoped for outcome of a Feyerabendian anarchist; what is hoped for is a provisional understanding of our knowledge, so that if we are wrong, which we most likely are, then the transcendence of our current accepted knowledge will be much more able to be effected.

More than this, Feyerabend contends that the concept of rationality was taken over by 'Rationalists'. Consequently,

A society that is based on a set of well-defined and restrictive rules so that being a man becomes synonymous with obeying these rules, *forces the dissenter into a no-man's-land of no rules at all and thus robs him of his reason and his humanity* (AM, p. 218)

This implies that Feyerabend believes that we do have reason and we do have humanity, even if we do not follow any strict code of first-order accepted rules.<sup>68</sup> Feyerabend then goes on to say that "It is a paradox of modern irrationalism that its proponents silently identify rationalism with order and articulate speech and thus see themselves forced to promote stammering and absurdity" (AM, p. 218) And in *Farewell to Reason*, he states that "Rationalism did not introduce order and wisdom where before there was chaos and ignorance; it introduced a special kind of order, established by special procedures and different from the order and the procedures of historical traditions". (FTR, p. 118.)

Feyerabend wants to wrest rationality back from the clutches of the 'Rationalists' and their conception of order and invest it with a broader vision of order and a more humane,

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<sup>68</sup> I say first-order rules, because a system which contends that rationality consists in choosing among an open-ended set of rules according to the peculiarities and context of the situation requiring rational decision, can be formulated as a strict meta-rule.

less mechanical, vision which it has always had, but which has been covered up and obscured.<sup>69</sup>

Secondly, it should be said that Feyerabend's criticisms of Lakatos outlined in the previous sections can and should be balanced by more positive assessments of Lakatos's philosophy. Joseph Margolis has contended that Lakatos presented a methodology and meta-methodology which was intended to fulfil the need for a new conception of rationality: one which doesn't fall to Feyerabend's criticisms of 'Rationalism'.<sup>70</sup> This may have been the case, but Lakatos still clung to vestiges of 'Rationalism', as evidenced by Lakatos's reliance upon content increase for succeeding theories in a research programme. As we saw, this was the major defect, in Feyerabend's eyes, of Lakatos's system. But many of the other ideas of Lakatos Feyerabend welcomed whole-heartedly.

Feyerabend contends that the only difference between himself and Lakatos is one of rhetorics (AM, p. 187): Lakatos's rhetorics are those of the 'Rationalist', whereas Feyerabend's rhetorics are those of the anarchist; the underlying theoretical viewpoints are the same. Thus Feyerabend applauds Lakatos for he sees "the methodology of research programmes [as] provid[ing] *standards* that aid the scientist in evaluating the historical situation in which he makes his decisions; it does not contain *rules* that tell him what to do" (AM, p. 186). The point and focus of Feyerabend's own philosophy has been to show that science is not run according to strict methodological rules, but that it is an idiosyncratic historical process which only allows us to formulate provisional standards which are taken as no more than rules-of-thumb: "my 'anarchism' does not eliminate

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<sup>69</sup> Well, this was the case when he wrote AM, he now seems to have given up any hope of rescuing the *words* reason and rational and rationality from the clutches of the authoritarians, he contends that they have become too contaminated with absolutistic and universalistic connotations to be now of any use. See FTR, p. 13. I don't believe that they have become contaminated beyond hope of rescue; enough people are trying to reconceptualize them so that there can be a general reappraisal. At any rate, we don't have the alternative words with which to express what we mean and neologisms will not strike a receptive chord.

<sup>70</sup> J. Margolis, "Scientific Methods and Feyerabend's Advocacy of Anarchism", in Munévar (ed) *Beyond Reason*, pp. 465-86. It is quite ironic in an article on Feyerabend, that Margolis, in an addendum to his original article, points out that the demand for content increase in Lakatos's theory was a left over from the classical conception of rationality, and attributes the criticism of content increase to Laudan and Grunbaum!

methodologies, but merely reforms them: instead of 'principles', 'presuppositions', 'necessary conditions of being scientific' we have rules of thumb".<sup>71</sup>

The rules-of-thumb, standards view of science leads Feyerabend to contend that "Reason as defined by Lakatos does not *directly* guide the actions of the scientist. Given this reason and nothing else, 'anything goes'" (AM, p. 186) So, anything-goes anarchism means not being directly guided by rules: not having your actions dictated to. Once we get beyond the impression created by Feyerabend's rhetoric of anarchism, we can start to see that what Feyerabend is proposing is not something totally destructive and negative. Reasoning, for Feyerabend, is something done within a context; consequently, in identifying a context, it may be apparent that there was only a limited number of rational options available to the people within that context. It may even be the case that when we look at the rational options available, some abstract, general rule may seem to be at work. But that is only an illusion, in a different context, one of the rational options may be to abandon, say, for example, the law of excluded middle. But these results cannot then be generalised: the option in Quantum Mechanics associated with abandoning the law of excluded middle is an option specifically connected to the problem situation in Quantum physics. We can see that Lakatos's philosophy is also, at its heart, and in its effect, a contextual philosophy; in that respect it is supported by Feyerabend. What Feyerabend objects to in Lakatos is Lakatos's 'lack of honesty' regarding this ultimate contextual nature of judgments of rationality. That Feyerabend never supported anarchism, in the usual sense of the word, can be further evidenced by the fact that Feyerabend states that "Having concluded my essay, I shall ... join Lakatos rather than continuing to beat the drum of *explicit* anarchism." (AM, p.214.) That is, Feyerabend's 'anarchism' was always meant to be understood in terms of his *reductio*, not as a confession of belief.<sup>72</sup>

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<sup>71</sup> Feyerabend, "Concluding Unphilosophical Conversation", in Munévar (ed) *Beyond Reason*, p. 503.

<sup>72</sup> In relation to the last quote, it is interesting to note that chapter 16 of the first edition of AM, the chapter critical of Lakatos, has been edited out of the third edition of AM. Is this an indication that Feyerabend came to feel that the criticism of Lakatos was no longer necessary, in that many of the changes in the intellectual atmosphere, wished for by Feyerabend, had been effected, and that defense of explicit anarchism was therefore no longer necessary?

Thus it can be contended that Feyerabend has no *absolute* objection to 'Rationalist' philosophies; rather, what Feyerabend objects to is the attempt to apply 'Rationalist' philosophies absolutely. Feyerabend believes that there should be a balance between generalised 'Rationalist' principles and a contextualised, 'anarchic', 'opportunistic' response to the idiosyncrasies of context. 'Rationalism', says Feyerabend, "like cars, airplanes, nuclear reactors ... has side effects which may make it necessary to regulate its use",<sup>73</sup> 'Rationalism' is not inherently bad: 'Rationalistic' principles can be applied and do have a place, but they need to be regulated and assessed as to their applicability to the individual circumstances in which rational decisions must be made. If we look at Feyerabend in this manner, the apparent extremity of some of his viewpoints can be seen as a heavy dose of a medicinal draught administered to an ailing patient.

I do not say that epistemology should become anarchic, or that the philosophy of science should become anarchic. I say that both disciplines should receive anarchism *as a medicine*. Epistemology is sick, it must be cured, and the medicine is anarchy. Now medicine is not something one takes all the time. One takes it for a certain period of time, *and then one stops* ... Anarchism, I say, will heal epistemology and *then* we may return to a more enlightened and more liberal form of rationality. (SFS, p. 127.)

Radiation therapy administered to a healthy person can make them ill, but given to a person with cancer it may make them well.<sup>74</sup>

Consequently, Feyerabend considers his espousal of anarchism as a dialectical move within the either/or dichotomy of "law-and-order methodologies and anarchism in science" (AM, p. 171). Feyerabend uses the dichotomy within the context of his *reductio*; once we have ventured past his *reductio*, we can transcend the dichotomy and the assumptions which underlie it. That is, the law-and-order/ anarchism dichotomy is a dichotomy adhered to by 'Rationalists': if events or thoughts do not conform to the laws and principles of a 'Rationalistic' philosophy, then they are lawless, and therefore anarchistic. Consider this quote:

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<sup>73</sup> "Concluding Unphilosophical Conversation", p. 498.

<sup>74</sup> Whether or not philosophy was, in fact, in need of the medicine is a completely different question. I tend to side with Feyerabend on this. But whatever you say about the particular diagnosis in this case, the general strategy seems defensible.

The difference between science and methodology which is such an obvious fact of history ... indicates a weakness of the latter, and perhaps of the 'laws of reason' as well. For what appears as 'sloppiness', 'chaos' or 'opportunism' when compared with such laws has a most important function in the development of those very theories which we today regard as essential parts of our knowledge of nature. (AM, p. 179)

Notice that it is only what *appears* sloppy, chaotic or opportunistic, *in comparison with the putative laws of reason*. It is not that they *are* sloppy, chaotic or opportunistic in any absolute sense: "there is rigour in all these prima facie chaotic procedures ... but it is a rigour that fits the situation, is complex, changes and differs greatly from the 'objective' rigour of our less gifted logicians and epistemologists" (FTR, p. 10.) For Feyerabend, to say that something is anarchistic is simply to say that it doesn't conform to any proposed universal laws of reason. Notice also that Feyerabend does not reject methodology, he only says that there is a weakness in any methodology which labels significant episodes in the history of science as anarchistic. This implies that methodology, far from being rejected, is seen as in need of reconceptualisation so that the significant episodes in the history of science, and significant procedures which were used in those episodes, which currently escape rational explication, will be able to be brought into the sphere of rational discussion. We can then produce theories of rationality which would be far more sensitive to the demands of contextual and situational vagaries.

I think we can conclude that the 'anything goes' slogan and the apparent anarchism of Feyerabend's philosophy is not a description of his substantive position. These terms and ideas follow from the fact that Feyerabend is conducting a *reductio*: "'anything goes' is definitely *not* Feyerabend's motto. It is only a description of how a despairing rationalist sees the situation once the nature of science has been examined closely".<sup>75</sup> If you are looking for universal, necessary, a-temporal, rational standards, and it is only standards of this sort which are deemed adequate, then the only standard of this sort which can be consistently defended is 'anything goes'. It is interesting to see philosophers who have noticed and appreciated Feyerabend's *reductio*, but who have not seen his anarchism as part of this *reductio*; thus Hooker comments that "as far as I can

<sup>75</sup> G. Munévar, "Introduction", in Munévar (ed) *Beyond Reason*, p. xvi.

see, he [Feyerabend] leaps from a critique of the formalist theory of reason directly into his anarchism".<sup>76</sup> Well, I would want to say that Feyerabend's 'anarchism', when put into the context of his *reductio*, is not really an anarchism at all.<sup>77</sup>

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<sup>76</sup> Hooker, "Between Formalism and Anarchism" p. 90.

<sup>77</sup> These somewhat sketchy remarks as to the positive nature of Feyerabend's 'anarchism' will be expanded and clarified in further chapters.

## 3 Feyerabend's Incommensurability and the Pragmatic Theory of Observation.

### 3.1 Introduction

In the first two chapters I have been arguing that Feyerabend is not an irrationalist: Feyerabend can be seen as arguing against a particular conception of rationality, not against the very idea of rationality itself. There are three reasons why this interpretation of Feyerabend has not been fully appreciated. Firstly, Feyerabend's rhetorical, provocative and polemical style of writing has sometimes made it difficult to separate the substantive positions which Feyerabend defends from the blatant provocations which we sometimes find him uttering. Consequently, quick readings can produce misunderstandings. Secondly, it has often been asserted that Feyerabend's theories are inherently radically relativistic, and that this radical relativism precludes any systematic account of rationality. Thirdly, Feyerabend's thesis of incommensurability is said to make rational choice between competing theories impossible. The first point will be dealt with throughout this thesis. I will examine the second point in the next chapter. In this chapter I will show that the third point concerning the putative irrationality following from the admission of incommensurable theories does not, in fact, follow. Feyerabend would want to say that theories are, in fact, commensurable, in certain ways, and that changes of theory are rationally motivated. However, a 'Rationalist' account of theory comparison and inter-theory relations leaves certain types of theories incommensurable. If no other principles of rationality are countenanced other than the 'Rationalist' ones, then Feyerabend's conclusion is that the 'Rationalist' account of rationality leaves some types of theory change without the possibility of rational explication.

In this chapter I want to do three things. Firstly, I want to give an account, taken exclusively from Feyerabend's writings, of Feyerabend's notion of incommensurability. This will include where Feyerabend saw incommensurability arising; the range of applicability of the notion; and the sorts of legitimate comparisons that Feyerabend

contended can be made between incommensurable theories. Secondly, in giving this account of Feyerabend's notion of incommensurability, it will become apparent that there exist large discrepancies between standard interpretations of incommensurability and the interpretation which we find Feyerabend supporting. In this respect my primary concern is not to vindicate Feyerabend in his support for incommensurability, but simply to show that the discussions surrounding incommensurability, in the most part, do not even address Feyerabend's position. Thirdly, I want to construct, out of the clues that are littered throughout Feyerabend's writings, a clearer picture of the impact that Feyerabend's notion of incommensurability has on the general structure of scientific decision-making. More specifically, I want to say that Feyerabend is offering an account of the rationality of science which differs significantly from some of the most prominent standard accounts.

### **3.2 Feyerabend's Incommensurability as Formal Incommensurability.**

#### **3.2.1 Universal Theories.**

The thesis of incommensurability is probably the most famous of Feyerabend's doctrines. There seems to be an inverse function operating here: the more famous the doctrine, the more misunderstood it is. Popular accounts of the meaning and consequences of incommensurability only have tenuous relations to what Feyerabend actually intended the meaning and consequences of incommensurability to be.

It is often contended that the thesis of incommensurability implies that any two theories cannot be compared in any way. Newton-Smith, for example, states that

the thought that theories are incommensurable is the thought that theories simply cannot be compared and consequently there cannot be any rationally justifiable reason for thinking that one theory is better than another ... taken literally it is implausible because it suggests that I could never have rationally justifiable grounds for holding any belief whatsoever ... [but] if I could have grounds for rationally preferring one ... belief to ...



[an]other why could I not have grounds for preferring one theory to another?<sup>1</sup>

It must be asked, whose theory is Newton-Smith talking about? Certainly not Feyerabend's or Kuhn's; indeed it is not a position I have seen anyone defend. It is a complete dummy position: a straw man of the highest order. Feyerabend himself states that "I never said ... that *any two* rival theories are incommensurable ... what I *did* say was that *certain* rival theories, so-called 'universal' theories, or 'non-instantial' theories, *if interpreted in a certain way*, could not be compared easily". (AM, p. 114)

It is crucial that we are clear as to what exactly universal, non-instantial theories are, for, as the quote above implies, this idea is the crux around which Feyerabend builds his conception of incommensurability. According to Feyerabend, universal non-instantial theories are theories which, unlike empirical generalisations, are not directly testable via empirical experience.<sup>2</sup> They are theories which make claims about "everything there is"<sup>3</sup>, and they are testable only after suitable initial conditions, boundary conditions, and associated empirical generalisations are added.<sup>4</sup>

I shall assume that the rules (assumptions, postulates) constituting a language (a "theory" in our terminology) form a *hierarchy* in the sense that some rules presuppose others without being presupposed by them. A rule R' will be regarded as being more fundamental than another rule R", if it [is] presupposed by more rules of the theory, R" included, each of them

<sup>1</sup> Newton-Smith, *The Rationality of Science*. p. 148. This conception of incommensurability has been held by many philosophers, for example, D. Shapere, "Meaning and Scientific Change", in his *Reason and the Search for Knowledge* and J. Giedymin, "The paradox of Meaning Invariance", *British Journal for the Philosophy of Science*, 21, (1970) pp. 257-68. Even Hooker, who in his paper in *Beyond Reason* seems to have an adequate understanding of Feyerabend's incommensurability, shows, in an earlier paper, a similarly complete misunderstanding: "Such a view [incommensurability] fractures the history of science into a number of unrelated, and unrelatable, episodes ... it becomes impossible to understand how one could decide between rival theories", in "Empiricism, Perception, and Conceptual Change", *Canadian Journal of Philosophy*, 3, (1972) p. 62. For more balanced presentations of incommensurability, see H.I. Brown "Incommensurability", *Inquiry*, 26, (1983) pp. 3-29, G. Couvalis, *Feyerabend's Critique of Foundationalism*, and H. Sankey, *The Incommensurability Thesis*.

<sup>2</sup> See Feyerabend, "Explanation, Reduction, and Empiricism", in H. Feigl and G. Maxwell (eds) *Minnesota Studies in the Philosophy of Science. Vol.III*. pp. 28-97. fn. 1. This article has also been reprinted in P.K. Feyerabend, *Realism, Rationalism and Scientific Method: Philosophical Papers Vol.1*. pp. 44-96.

<sup>3</sup> Feyerabend, "Replies to Criticism: Comments on Smart, Sellars and Putnam" in R.S. Cohen and M.W. Wartofsky (eds) *Boston Studies in the Philosophy of Science. Vol.2*. pp. 223-61, n. 5. This article has also been reprinted in *Phil. Papers. Vol.1*.

<sup>4</sup> See, "Explanation ...", fn. 1. A.N. Perovich, jr. "Incommensurability, Its Varieties and its Ontological Consequences" in Munévar (ed) *Beyond Reason* pp. 313-28, goes some way towards this distinction, but leaves it somewhat unclear.

being at least as fundamental as the rules presupposing R". It is clear that a change of fundamental rules will entail a major change of the theory, or of the language in which they occur. Thus a change in the spatio-temporal ideas of Newton's celestial mechanics makes it necessary to redefine almost every term, and to reformulate every law of the theory, whereas a change of the law of gravitation leaves the concepts, and all the remaining laws, unaltered.<sup>5</sup>

As a first approximation, we could conclude that universal, non-instantial theories, are those theories which are expressed in terms of a set of fundamental rules which do not presuppose any other rules other than the rules within the set. A theory of this sort would certainly be fundamental, but it seems that Feyerabend does not want to limit the idea of universal theories to this characterisation. In the quote above, the most fundamental aspect of Newtonian mechanics is considered to be the spatio-temporal structure of the universe: absolute, Euclidean space with an independent, absolute, temporal dimension. That may be an accurate description; however, Feyerabend contends in many places that Newtonian mechanics, understood as the three laws of motion, is a universal theory.<sup>6</sup> This would seem to suggest that universal theories are not confined to theories at the top of the hierarchy: lower level fundamental rules can be grouped together and classified as a universal theory. This would seem to create a looming problem for Feyerabend: if the incommensurability thesis is to hinge critically around the idea of a universal theory, then that concept should be able to be clearly identified and a line drawn between universal theories and 'particular', non-universal theories.

I think that this problem can be resolved if we concentrate on Feyerabend's contention that universal theories say something about "everything there is". Consider the full quote:

when speaking of *theories* I shall include myths, political ideas, religious systems, and I shall demand that a point of view so named be applicable to at least some aspects of everything there is. The general theory of relativity is a theory in this sense, "all ravens are black" is not.<sup>7</sup>

<sup>5</sup> Feyerabend, "Replies to Criticism", n. 27.

<sup>6</sup> See, for example, "Explanation ..." pp. 52-62, where Feyerabend discusses the impetus theory and Newtonian mechanics.

<sup>7</sup> Feyerabend, "Replies to Criticisms" n. 5. Feyerabend goes on to say that "there are certain similarities between my use of 'theory' and Quine's 'ontology' ... Carnap's 'linguistic framework' ... Wittgenstein's

"All ravens are black" is not a universal theory because it does not say something about everything there is: it says nothing about skyscrapers, people, and so on. Similarly, the kinetic theory of heat, for example, is a limited theory, it does not say anything about the general constitution of matter.<sup>8</sup> On the other hand, Newtonian mechanics brings with it assumptions concerning the spatio-temporal structure of the universe, the nature of matter, and the nature of forces; all of these things are universally applicable. We can conclude that if a particular theory T, can be interpreted as attributing properties universally, then that theory is a universal theory, in Feyerabend's sense.

Let us consider again this quote "I never said ... that *any two* rival theories are incommensurable ... what I *did* say was that *certain* rival theories, so-called 'universal' theories, or 'non-instantial' theories, *if interpreted in a certain way*, could not be compared easily". (AM, p. 114) The main point to be made from this is that incommensurability arises only through particular types of interpretations of universal scientific theories; which implies that different interpretations of universal scientific theories will not eventuate in incommensurability. Even when incommensurability does arise, Feyerabend explicitly states that they "could not be compared easily", not that they could not be compared at all. As a first summary it can be said that, for Feyerabend, incommensurability was never intended to be a categorical claim about the nature of theories as such.

### 3.2.2 The Logical Empiricist Background.

To show that Feyerabend never intended incommensurability to be as Newton-Smith and other philosophers see it, we must first place Feyerabend's ideas in the context from which they arose; that is, in the context of a critique of logical empiricism and the formal notion of theoretical comparison found therein. Feyerabend's original exposition of the incommensurability thesis can be found in his "Explanation, Reduction and

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'language game' ... Pareto's 'theory' ... Whorf's 'metaphysics' ... Kuhn's 'paradigm' ... etc.[!]" Feyerabend never went on to detail these claims and interrelationships. I won't even attempt such a mammoth task.

<sup>8</sup> Of course, logically, 'all ravens are black' says of everything that if it is a raven, then it is black. But we should not let this logical point obscure the intent of Feyerabend's position: the sense is sufficiently unambiguous.

Empiricism" article of 1962. In this article Feyerabend contends "that a formal account of reduction and explanation is impossible for general theories, or non-instantial theories".<sup>9</sup> More generally, Feyerabend intends to show that formal logical analysis of the relations between successive universal scientific theories does not account for the relations that in fact obtain between successive universal scientific theories. Feyerabend is contending that one of the ways of interpreting universal theories which leads to incommensurability are those realist interpretations which see universal scientific theories as formal axiomatic systems, and where the relations between successive universal scientific theories can be explicated as formal logical relations of deducibility and derivability.

Feyerabend explicitly criticises the views of Hempel and Oppenheim, Nagel, and Carnap. But his main attack is upon logical empiricism in general. I think it can be assumed that all logical empiricists adhered to a two language view of scientific theories; that is, there is an identifiable dichotomy between the theoretical terms of a scientific theory and the observational terms of that theory. The observational language in which observations are stated is said to be a pre-existing, neutral language, completely devoid of any theoretical components. Observations are said to be the foundation of knowledge and the repository of information and meaning. The theoretical languages are said to be uninterpreted, abstract, propositional systems which gain an interpretation, and meaning, from being linked with the observational foundation. Feyerabend quotes this passage from Carnap:

there is "no independent interpretation for  $L_T$  [the language in terms of which a certain theory, or a certain world view, is formulated]. The system  $T$  [the axioms of the theory and the rules of derivation] is in itself an uninterpreted postulate system. [Its] terms ... obtain only an indirect and incomplete interpretation by the fact that some of them are connected by the [correspondence] rules  $C$  with observation terms ... ". (AM, p. 279)<sup>10</sup>

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<sup>9</sup> Feyerabend, *Phil. Papers Vol. 1*, p. 44.

<sup>10</sup> Cf. also "Explanation ...", pp. 41-2. For a classic presentation of this sort of view, see R.B. Braithwaite, *Scientific Explanation*, where Braithwaite introduces the 'zipper' metaphor whereby meanings are introduced into scientific systems from the bottom, observational level, upwards into the theoretical levels. Theoretical levels only have indirect meanings given to them by their place in a deductive system and their ultimate connection to observations. This view is what has sometimes been called the 'upward seepage' of meaning, for a discussion of which, see H.I. Brown, *Perception, Theory and Commitment* pp. 46-9.

I think it is safe to say that the above position of Carnap's was a general logical empiricist position.

Feyerabend contends that in the logical empiricist analysis of inter-theoretic relations, such as that of reduction and explanation, there are two assumptions, or demands which must be met. Firstly, that the relations that exist between two theories be one of deducibility. In the case of reduction this amounts to the claim that the reduced theory should be able to be logically deduced from the reducing theory. Similarly, for an explanation to be adequate "the explanandum must be logically deducible from the information contained in the explanans".<sup>11</sup> The second assumption is that, with respect to the processes of both reduction and explanation, meanings are to remain completely invariant. Feyerabend contends that meaning invariance is necessary because without meaning invariance the required relations of deducibility would not be attainable.

I think that Feyerabend is correct in his contention that meaning invariance is a consequence of logical empiricism: it seems to follow naturally from its basic tenets. If all meaning and information is encapsulated in pre-theoretical experience articulated in a neutral observation language, and meaning is given to theoretical terms via the observation language, then the theoretical component of successive scientific theories is compared logically through their correlation and connections to the common observation language. Consequently, provided that the pre-theoretical, theory neutral experience does not change, the logical empiricist conception entails that all theories are able to be compared to any other theory. The very idea of incommensurability is an impossibility in this system.

Feyerabend responds to this logical empiricist position in three steps (i) Feyerabend blocks the instrumentalistic move by contending that there is indeed an objective, independent world. Our theories are meant to be interpreted realistically: they are designed to be accurate descriptions of the processes and things constituting the world. (ii) Given this realism, Feyerabend contends that we must replace the dual language model of scientific discourse and the role that observation plays within that

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<sup>11</sup> Hempel and Oppenheim, in Feyerabend, "Explanation ...", p. 34.

model. He does this by proposing his own pragmatic theory of observation.<sup>12</sup> (iii) Given (i) and (ii), meaning invariance is violated; therefore, formal accounts of inter-theoretic relations fail and incommensurability results.

### 3.2.3 The Pragmatic Theory of Observation.

Feyerabend asserts that he is an empiricist; however, he interprets empiricism to be

a *cosmological hypothesis* concerning the relation between man and the universe ... It is assumed by the hypothesis that there exists a real objective world that contains human observers, and that sensations, but not thoughts, are highly correlated with events in this world.<sup>13</sup>

That is, Feyerabend is a realist concerning theories: the theoretical entities and processes postulated by scientific theories are intended to be realistic descriptions of the nature of the world. Feyerabend contends that if we are realistic about theories, then the perceptions we have should be interpreted in terms of the theory which we hold: the perceptions we have are *caused* by the entities and processes which are posited as existing in the world.

for example, we may admit that the sentence 'this is a raven' uttered by an observer who points at a bird in front of him is an observational sentence and that the observer has produced it because of the impressions, sensations, and perceptions he possesses. We may also admit that he would not have uttered the sentence had he not possessed those impressions. Yet, the sentence is not therefore about impressions; it is about a bird which is neither a sensation nor the behaviour of some sentient being. Similarly, it may be admitted that the observation sentences which a scientific observer produces are prompted by his impressions.

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<sup>12</sup> I will go on throughout the remainder of this chapter to say that it is a realistic interpretation of theories, conjoined with a theory-ladenness thesis of observation, and formalistic accounts of inter-theoretical relations, which leads to incommensurability. Feyerabend has rejected the idea that observations are theory-laden; he contends that his own pragmatic theory of observation relinquishes the very idea of a dichotomy, theory/observation, which the theory-ladenness thesis seems to defend. But for the purposes of Feyerabend's argument, the thesis of the theory-ladenness of observation is sufficient to undermine the idea of meaning invariance. It is therefore sufficient to lead to incommensurability. Thus Feyerabend's incommensurability is not dependent upon acceptance of his pragmatic theory of observation, but is a more generally applicable notion.

<sup>13</sup> Feyerabend, "Problems of Empiricism" in R.G. Colodny (ed) *Beyond the Edge of Certainty: Essays in Contemporary Science and Philosophy*, pp. 215-16.

However, their content will again be determined, not by these impressions, but by the entities allegedly described.<sup>14</sup>

In this situation, observations are not independent sources of meaning, through which theoretical terms are given indirect meanings; rather, it is the theoretical terms which are primary, and through which observational terms are given meaning.

Thus, Feyerabend proposes the pragmatic theory of observation in place of the dual-language semantic theory of observation. The semantic theory of observation is construed as the theory which contends that observational terms can be delineated from theoretical terms by examination of the meaning of the term in question. If a term's meaning can be given solely in relation to perception, then that term is an observational term. In contradistinction to this, the pragmatic theory of observation construes observational terms as those terms an observer will utter when confronted with perceptual stimuli:

a statement will be regarded as observational because of the *causal context* in which it is being uttered, and *not* because of what it means. According to this theory, 'this is red' is an observation sentence, because a well-conditioned individual who is prompted in the appropriate manner in front of an object that has certain physical properties will respond without hesitation with 'this is red'; and this response will occur independently of the *interpretation* he may connect with the statement (he may interpret it as referring to a property of the *surface* of the object, as a property of the *space between* the object and the eye (as did Plato), as a *relation* between the object and a coordinate system in which he himself is at rest). All we need in order to provide a theory with an observational basis are statements satisfying this pragmatic property.<sup>15</sup>

Observations are theoretical, but they can be distinguished in terms of statements uttered in relation to causal, perceptual contexts. Some examples will make this point clearer.

When an observer utters the sentence, 'this is a raven', the semantic theory of observation contends that the sentence is observational only in so far as the meanings of the terms within it can be reduced to perceptual stimuli. The pragmatic theory of observation separates observability and meaning. Imagine two observers, both of whom utter the sentence 'this is a raven', the first observer is a biologist who interprets the term 'raven' in

<sup>14</sup> Feyerabend, "Explanation ...", p. 35.

<sup>15</sup> Feyerabend, "Problems of Empiricism" p. 198.

terms of current biological theory, the second observer, on the other hand, believes in an animistic-mythological world; consequently, the term 'raven' is interpreted by the second observer in terms of animistic spirits portentous of evil happenings. The term 'raven' as uttered by the two observers has completely different meanings, but, nevertheless, both observers can utter the same sentence, and in both utterances the sentence is observational: *a sentence or term is observational if it is produced in response to perceptual stimuli*. "Sensations and perceptions are *indicators*, and they are in this respect on a par with the indications of physical instruments. Like these indications, they are in need of interpretation".<sup>16</sup> Thus, when a scientist points to a line in a cloud chamber and says, 'this is an electron', it is an observational sentence because it is produced in response to perceptual stimuli. Of course, the meaning of the term 'electron' is provided by the complex theories of theoretical physics, but this only goes to show that if we are realistic about theories, then we literally *see* the world as the theory we hold pictures the world to be.<sup>17</sup>

#### 3.2.4 Meaning Variance and the Failure of Formalism.

The third step of Feyerabend's criticism is to say that if we accept the idea that each individual universal theory carries its own ontology, rules and presuppositions, which together give meaning to observational statements, rather than observation statements giving meaning to theoretical statements, then the logical empiricist explications of explanation and reduction fail because the requirement of meaning invariance is violated: different universal theories give fundamentally unique meanings to

<sup>16</sup> *Ibid.*, p. 216.

<sup>17</sup> Cf. Shapere, in his *Reason and the Search for Knowledge*, pp. 215-17 and Ch.16. where he discusses the 'observation' of the centre of the sun through solar neutrino experiments. I think that it can be concluded that scientists use observational idioms in the manner of the pragmatic theory of observation. It is quite ironic that Shapere *criticises* Feyerabend's pragmatic theory of observation, in his *Ibid.*, pp. 75-8, given that Shapere proposes a theory of observation which seems to be identical in spirit to that which Feyerabend was proposing. It must also be said that Feyerabend's pragmatic theory of observation does not lead him back to any 'older empiricism' *Ibid.*, p. 76 which compromises Feyerabend's critical philosophy. See J.G. McEvoy "A 'Revolutionary' Philosophy of Science", pp. 58-62 for this contention. As we have seen, there is no common meaning for alternative universal theories to draw upon through observations and incorporated into observational statements. *Meaning* is provided solely by theoretical presuppositions. *Reference*, on the other hand, is partly determined by the pragmatics of observation. Cf. section 3.3.2 below for a discussion of reference.



the terms in their theory. If there is meaning variance between any two universal theories, then there can be no relations of strict deducibility between any two universal theories.<sup>18</sup>

To make this last point a little clearer, consider Feyerabend's example of the impetus theory of motion and the Newtonian theory of motion. On the impetus theory, the application of constant force is required to sustain inertial motion. According to Newtonian theory, *no* forces are necessary to sustain inertial motion. Consequently, in a Newtonian world, the concept of impetus is incoherent: the two ideas of motion cannot co-exist as descriptions of reality. In this situation, if the impetus theory *could* come out as a logical consequence of Newtonian theory, then the Newtonian theory would have to be internally inconsistent. As Feyerabend contends, it "would imply the demand to derive from correct premises what is false".<sup>19</sup> Needless to say, if the ideal of scientific theories is that scientific theories should be formal axiomatic systems, then the consequence that scientific theories are internally inconsistent is an unacceptable conclusion.

A second example may make the point even clearer. Imagine ourselves in the nineteenth century at the time of the victory of the wave theory of light over the particle theory of light. In this situation, how can a particle theory of light, interpreted realistically, be a logical deduction from a wave theory of light, interpreted realistically? The ontologies postulated by the two theories are mutually exclusive, and a deduction of one from the other an impossibility. The only way to achieve such a deduction is to interpret theories instrumentalistically; and say that the wave theory of light can be used to deduce all the relevant empirical facts. Because the empirical facts are considered to be a stable, independent, theory-neutral domain, then, inasmuch as the particle theory of light could be said to deduce the facts, the particle theory is deducible from the wave theory.

To see where Feyerabend's incommensurability enters into this picture, I will recap Feyerabend's main argument. If we accept a realistic account of theories, with its

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<sup>18</sup> Cf. the very detailed account of meaning invariance in Couvalis, *Feyerabend's Critique of Foundationalism*, pp. 60-80.

<sup>19</sup> Feyerabend, "Explanation ...", p. 68.

concomitant consequence of theory-ladenness of observation, then, for universal theories, purely formal procedures of comparison fail. If, in this situation, we accept a realistic account of theories and the subsequent theory-ladenness of observation, but still wish to retain purely formal procedures of theoretical comparison, then we must accept the conclusion that some theories are incommensurable. This is not to say that Feyerabend himself believes that universal theories are incommensurable.<sup>20</sup>

I think that this point needs stressing. If we read through most of Feyerabend's writings, we find that he often claims that there are incommensurable theories. I would even say that Feyerabend believed that the existence of incommensurable theories is an objective fact.<sup>21</sup> But these claims are meant to be understood in the context of (i) assumptions concerning the nature of theory and observation. And (ii) formal logical methods of theoretical comparison. That is, it is an objective fact about the nature of some theories, interpreted in a particular manner, that they will be incommensurable in relation to certain formal methods of comparison. So, when Feyerabend states in *Against Method* that "Theories can be interpreted in different ways. They will be commensurable in some interpretations, incommensurable in others. Instrumentalism, for example, makes commensurable all those theories which are related to the same observation language and are interpreted on its basis", (AM, p. 279) we can now see more clearly what he means; though the quote is incomplete as it stands, that is, as I said above, it is the combination of an interpretation *and* a formal method of comparison which leads to incommensurability.

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<sup>20</sup> This is one of the few weaknesses of Couvalis', otherwise quite excellent, account of incommensurability in his book *Feyerabend's Critique of Foundationalism*. That is, Couvalis does not fully appreciate that it is a realistic interpretation of universal theories, conjoined with the demand for formalistic notions of inter-theory relations, which leads to incommensurability. Thus Couvalis contends that it is "a necessary condition for two theories to be incommensurable that the later theory *excludes* all of the ontology of the earlier theory" p. 76. This is true, but it must be remembered that this is not a sufficient condition for incommensurability: the two theories in question must also be interpreted realistically. If two theory's ontological claims are mutually exclusive, they are not necessarily incommensurable: if they are interpreted instrumentalistically, then they are commensurable. Couvalis also contends that incommensurability is the idea that no primitive descriptive term, in the one theory, can be defined in terms of the primitive descriptive terms of the other theory. Again, this is true, but only if we interpret universal theories realistically. If we are instrumentalists, then we can define the terms of one universal theory, in terms of another universal theory, without any problems arising.

<sup>21</sup> See, AM, pp. 271-4.

That theories *are* commensurable, has always been a part of Feyerabend's thought; for example, he states that "incommensurability is no difficulty for the sciences ... it is a difficulty only for some very naive philosophical theories".<sup>22</sup> It is no difficulty for the sciences because scientists do not compare theories in the manner advocated by logical empiricists: it is only of secondary importance to scientists whether a particular theory can, or cannot, be logically deduced from another theory.<sup>23</sup>

### 3.2.5 Popperian Incommensurability

The preceding discussion may give the impression that Feyerabend intended his notions to apply solely to logical empiricism: that incommensurability is something that only logical empiricists must deal with. Feyerabend *has* stated that incommensurability is "a puff of hot air extinguishing some burnt-down positivist candles."<sup>24</sup> But, as we saw, logical empiricists can validly avoid any accusation of incommensurability if they remain within the dual-language scheme, and interpret scientific theories as *not* realistic, but simply as fictional instruments for moving from one empirical experience to another in an efficient, short-hand, manner. Feyerabend believes that this instrumentalistic form of empiricism is suspect for two reasons: because of general realist arguments and sentiments; and because of his support for the pragmatic theory of observation. But as we have seen, it is these two conceptions, realism and theory-ladenness of observation, when combined with formalistic concepts of inter-theory relations, which leads to incommensurability. Consequently, it is Popper and his followers, and any others who accept the above combination of factors, who must face the problem of incommensurability.

If we examine the relations that Popper sees as necessary to obtain between two successive scientific theories, then we have to come to the conclusion that they are of a highly formal nature. The requirement that a new theory must meet is that a new theory must have greater empirical content than its predecessor. Or, equivalently, the new

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<sup>22</sup> "Third Dialogue" in *Three Dialogues on Knowledge*, p. 154. See also *Phil. Papers Vol. 1*, p. xi.

<sup>23</sup> How Feyerabend sees comparisons between theories I will address in section 3.4 below.

<sup>24</sup> "Third Dialogue", p. 156.

theory must be more falsifiable than its predecessor. The falsifiability, or empirical content of a theory, is determined by purely logical procedures: it is that class of statements, out of the class of all logically possible statements, which are inconsistent with the theory.<sup>25</sup>

There are three conditions on this requirement, the first two of which are: (i) The ratio of truth-content to falsity-content must be improved. This is achieved by requiring that the false consequences of the previous theory be denied by the new theory. And (ii) The new theory must make new predictions which are falsifiable and testable, thereby potentially increasing the truth-content of the new theory. (CR, p. 246.) This second condition can be explicated as the demand that new theories must be of a higher level of universality than their predecessors; which means, in Popper's system, that the old theory becomes a logically deduced middle-level hypothesis in an axiomatised system.

Popper contends that one of the aims of scientific theorising is to arrive at rigorous axiomatised systems; "this is very necessary; for a severe test of a system presupposes that it is at the time sufficiently definite and final in form to make it impossible for new assumptions to be smuggled in." (LSD, p. 71.) That is, in order for Popper's deductive falsificationist methodology to work, the potential falsifiers of a theory must be clearly ascertainable; the best way to achieve this is to construct the theory as an axiomatic system. According to Popper, in this sort of system, "the axioms are chosen in such a way that all the other statements belonging to the theoretical system can be derived from the axioms by purely logical or mathematical procedures" (LSD, p. 71.) and that "we may distinguish, within a theoretical system, statements belonging to various levels of universality. The statements on the highest level of universality are the axioms; statements on the lower levels can be deduced from them." (LSD, p. 75.) Not only are axiomatic systems preferable because of the precision which they engender, they are also preferable because the more universal a theory is, the more testable (falsifiable) a theory becomes:

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<sup>25</sup> Popper, LSD, Sections 21, 31 and in many other places.

we could now say that our methodological decision ... is to leave nothing unexplained, *i.e.* always try to deduce statements from others of higher universality. The decision is derived from the demand for the highest attainable degree of universality and precision, and it can be reduced to the demand, or rule, that preference should be given to those theories which can be most severely tested. (LSD, p. 123.)

That is, it is a deep consequence of Popper's philosophy that we must search for theories of an ever more universal character: axiomatic systems must be found which are of a higher level of universality, and therefore of a higher level of falsifiability, than the previous system. In fact, "a theory which has been well corroborated can only be superseded by one of a higher level of universality; that is, by a theory which is better testable and which, in addition, *contains* the old, well corroborated theory." (LSD, p. 276.)

This brings us to the third condition which a new theory must meet: a new theory must explain the facts that the preceding theory successfully explained. Once we see what Popper means by explanation, we find that his conception of explanation is virtually identical to that of the logical empiricists. For example, Popper states that "to give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular statements." (LSD, p. 59.) As with the logical empiricists, so too for Popper: to explain is to deduce. Now, it may be said that Popper only applies this idea of explanation as deduction to the explanation of facts. But this is not so: for Popper, all statements are hypothetical in nature, they are simply of more or less degrees of universality. If we look carefully at the quotes from Popper given in the previous paragraph, we see that it is not only 'facts' which are to be explained via logical deduction from higher level universal statements, all statements and hypotheses are explained by their logical deduction from higher level, more universal, hypotheses, "explanation is always the logical reduction of hypotheses to others which are of a higher level of universality." (LSD, p. 63.) If this is the nature of explanation for Popper, then we can see that when Popper claims that "one can work with the idea of hierarchical levels of explanatory hypotheses. There are comparatively low level ones ... higher ones such as Kepler's laws, still higher ones such

as Newton's theory, and, next, Relativity", (CR, p. 173.) the relation between these hypotheses is supposed to be one of logical deducibility.

We can see here that Popper's ideas concerning the formal requirements that a new theory must meet includes, as a necessary condition, the idea that theories must possess relations of logical deducibility between them. As Feyerabend has argued, to have logical deducibility, we must also have meaning invariance. But universal theories, interpreted realistically, are meaning variant. Therefore, universal theories cannot enter into relations where one is a logical deduction of the other. Consequently, universal theories cannot be compared as to empirical content.

This is probably being a little unfair to Popper: though the passages I quoted seem to suggest, quite unequivocally, that the relations between theories is meant to be one of deducibility, Popper has admitted at other places that strict deducibility is not obtainable for some theories. Thus, Popper accepts that Kepler's laws and Newton's theory are, in fact, contradictory, and that they therefore cannot be placed in relations of deducibility, one from the other. (CR, pp. 61-2.) But this development does not mitigate the conclusions so far reached: Popper still retains the *ideal* of the deducibility of theories, but it is now weakened in that, though some unchanged theories cannot be deduced from superseding theories, it is still necessary for an approximation of the previous theory to be able to be deduced from the superseding theory. This requirement, along with the general requirement of content increase for successive theories, presupposes that the theory which is to deduce an approximation of the superseded theory, and the superseded theory itself, are sufficiently similar in content for the process of approximation to be valid; that is, the two theories must share large amounts of background knowledge, including observational theories, for a true statement of the one theory to be able to be conceived as a true statement of the other theory. But this is exactly what does not obtain when we consider two universal theories: it is of the nature

of universal theories that their ontologies, and therefore their content, are completely different. This difference also usually includes a difference of observational theories.<sup>26</sup>

It is interesting to note that Popper did seem to have some sort of inkling as to the problems that could arise if we followed his requirements. For example, in giving his definition of verisimilitude, Popper prefaces his definition with the condition that "the truth-content and the falsity-content of two theories  $t_1$  and  $t_2$  are comparable", (CR, p. 233.) and he also says that "as long as there are no revolutionary changes in our background knowledge, the relative appraisal of our two theories,  $t_1$  and  $t_2$ , will remain stable." (CR, p. 235.) These passages imply that two theories *might not* be comparable, especially if there has been revolutionary changes in background knowledge. Is Popper accepting the idea that incommensurability is possible? Well, I don't think Popper ever accepted that there are incommensurable theories.

Recall that a logical empiricist can avoid the result of the incommensurability of theories, whilst retaining the formal requirements of theory comparison of deducibility and derivability, by relinquishing the idea that scientific theories are to be interpreted realistically, and retaining the idea of the theory-neutral domain of observation. Popper does not have this choice: Popper is a realist who accepts the notion of the theory-ladenness of observation. Consequently, Feyerabend's challenge is to say to Popper that he must either accept the notion that universal theories cannot be compared via formal requirements of empirical content increase, and thereby accept the idea that universal theories are incommensurable, or he must abandon the formal requirements of empirical content increase and look for an alternative account of inter-theoretic relations; one which does not lead to incommensurability.

Feyerabend also contends that Lakatos's methodology of scientific research programmes runs into the obstacle of the incommensurability of scientific theories. Even though Lakatos develops ideas which are remarkably similar to ideas proposed by Feyerabend, in that the character of research programmes is very similar to what

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<sup>26</sup> Cf. the differences in observational theories discussed in chapter 1, *vis a vis* Aristotelian observational theories and the observational theories necessary to support the scientific revolution. For a discussion of Popper on the points mentioned above, see "Explanation ...", pp. 92-4.

Feyerabend meant by universal, cosmological theories; nevertheless, Lakatos still retains in his, elsewhere very much modified Popperianism, the crucial notion of content increase. For example, Lakatos contends that, within a research programme, a theory T is falsified, and another theory T' accepted only if

T' has ... the following characteristics: (1) T' has excess empirical content over T: that is, it predicts *novel* facts, that is, facts improbable in the light of, or even forbidden, by T; (2) T' explains the previous success of T, that is, all the unrefuted content of T is included ... in the content of T'; and (3) some of the excess content of T' is corroborated.<sup>27</sup>

This version of content increase is identical to Popper's notion; therefore, the conclusion should be the same. Lakatos must accept that, given this formal requirement of content increase, then successive theories will be incommensurable.

But the situation is not quite the same as it was for Popper. Recall that Feyerabend contended that it is only the formalistic attempt to compare *universal theories* which fail. If we identify the universal theories of Feyerabend with the research programmes of Lakatos, then the content increase thesis outlined above does not entail incommensurability; the reason being that the theories being compared are internal to a particular research programme.

However, this does not mean that Lakatos escapes. When Lakatos comes to the question of how research programmes can be compared and evaluated, he tries to apply the same conditions of content increase that he applies to theories within research programmes. He states that "an objective reason [to reject a programme] is provided by a rival research programme which explains the previous success of its rival and supersedes it by a further display of *heuristic power*."<sup>28</sup> Heuristic power here means the ability to predict novel facts. That is, the criterion for successive research programmes is the exact same criterion for successive theories within research programmes. But in the discussions which follow this quote, Lakatos himself seems unconvinced as to the

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<sup>27</sup> Lakatos, "Falsification ...", p. 116.

<sup>28</sup> *Ibid.*, p. 155.



veracity of this prescription: there are always valid reasons for continuing to support a degenerating research programme.

As we saw in the previous chapter, Lakatos is quite ambivalent on many issues and his substantive position hard to pin down. But I think we can conclude that if Lakatos stuck wholeheartedly to the idea of relative content increase as an indicator of the preferability of one research programme over another, which seems to be his wish, then Feyerabend can claim that the comparison cannot succeed because research programmes are incommensurable.<sup>29</sup> If, however, Lakatos gives up on the idea that relative content increase is an indicator of the preferability of research programmes, which, as we saw in the previous chapter, seems to be his actual course, then Feyerabend can embrace Lakatos as a 'fellow anarchist'.

### **3.3 Responses to Feyerabend's Incommensurability**

#### **3.3.1 The Strategy of Misinterpretation.**

At this point I will give a short summary of Feyerabend's position. Feyerabend never contended that scientific theories are incapable of comparison: the commensurability of scientific theories is an obvious fact. What Feyerabend did say was that if we are realistic about our theories, then certain very popular and deeply entrenched philosophical ideals of theoretical comparison do not work. If these philosophical ideals are considered to be the *only* way in which theoretical comparison can take place, then it is Feyerabend's contention that we are left with incommensurable theories. To consolidate this conclusion, consider this quote from Feyerabend

scientific discourse *which contains detailed and highly sophisticated discussions concerning the comparative advantages of paradigms* obeys

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<sup>29</sup> A particularly striking example of this situation is the idea that the requirement of content increase is itself an epistemic value and methodological maxim, traceable to a particular world-view: the Aristotelian world-view did not include content increase, as conceived by Popper and Lakatos, as one of its epistemic values, in fact, the very idea is inconsistent with Aristotelianism and, therefore, cannot be made a part of the Aristotelian world-view. Consequently, the use of such a formal criterion of theoretical comparison is question begging when applied to the comparison of Aristotelianism with the "scientific" world-view. Cf. the discussion in Section 2.2.5, and Feyerabend's "In Defence of Aristotle".

laws and standards that have only little to do with the naive models which philosophers of science have designed for that purpose. *There is* comparison, even 'objective' comparison, but it is a much more complex and delicate procedure than is assumed by rationalists. Thus in my first paper on the matter ["Explanation, Reduction and Empiricism"] I claim that 'a formal account of reduction and explanation is impossible for general theories' but show how predictions can still be used for comparing theories: what fails is not the process of theory comparison; it is a rather simpleminded theory of explanation.<sup>30</sup>

In the next section I will elaborate some of Feyerabend's positive notions concerning theoretical comparison. But before I do that, I will present and respond to some of the criticisms levelled at Feyerabend's incommensurability, in the light of the conclusions reached above.

Early in this paper I quoted a passage from Newton-Smith which contended that incommensurability implies that no theory can be compared, in any manner whatsoever, to any other theory. The further implication was drawn from this that incommensurability implies that there can be no rationality in scientific decision-making. Newton-Smith is not alone in this interpretation: the interpretation is the standard interpretation of incommensurability. For example, Laudan states that incommensurability is the thesis "that theories before and after a revolution are so radically different that we cannot even speak meaningfully of any similarities between them",<sup>31</sup> and that "scientists working in different research traditions cannot communicate with, and cannot understand the statements of, their fellow scientists in other traditions ... theories cannot be compared and rationally evaluated".<sup>32</sup> I hope I have shown that this interpretation of Feyerabend's incommensurability is completely false; it may be that others have defended such an interpretation, though Kuhn certainly didn't, but that is irrelevant, *Feyerabend* did not support such a thesis.<sup>33</sup>

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<sup>30</sup> Feyerabend, "More Clothes From the Emperor's Bargain Basement: A Review of Laudan's *Progress and its Problems*", in *Phil. Papers. Vol.2*, p. 238

<sup>31</sup> L. Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth*, p. 141.

<sup>32</sup> *Ibid.*, p. 142.

<sup>33</sup> The source of these misinterpretations is difficult to find. Though I do provide another reason below, a major factor in this misrepresentation seems to follow from an inability to accept, or see, a new paradigm, and a consequent inability to read texts objectively. In that sense it is a vindication of Feyerabend and Kuhn's philosophical position. For example, Feyerabend states, in "Consolations for the Specialist" that "succeeding paradigms can be evaluated only with difficulty and ... they may be altogether incomparable", if we left the quote there we would think that the critics interpretations of

Laudan comes to the conclusion that "the possible incommensurability of theories and research traditions ... does not preclude the existence of comparative appraisals of their acceptability."<sup>34</sup> Apart from the fact that this is what Feyerabend has said all along, it shows the general strategy of critics of incommensurability: examine theories so as to come up with some way, any way, of comparing theories, then pronounce incommensurability defeated.

This strategy of defeating incommensurability by showing that theories *can* be compared, is based upon two false assumptions: (i) That *any* change in meaning of theoretical terms results in incommensurability.<sup>35</sup> Thus, Shapere contends that "we are led to believe that the theory-dependence of meanings is a necessary truth, that since the meaning of *every* term depends on its theoretical context, therefore a change of theory *must* produce a change of meaning of every term in the theory."<sup>36</sup> And (ii) That, for Feyerabend, incommensurable is synonymous with incomparable. The conclusion drawn from these two assumptions is that incommensurability implies that science is completely irrational.

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Feyerabend are correct; but the passage continues, "at least as far as the more familiar standards of comparison are concerned (they may be readily comparable in other respects)" p. 219. Surely, anyone who read this article must have read that passage? There is one aspect of Feyerabend's early writings on incommensurability which could be said to vindicate some of the criticisms of him, that is, in his early writings, Feyerabend did not draw the line sharply between theories which are merely inconsistent, and those which are incommensurable. This may have created the impression that simple inconsistency between two theories was enough to create incommensurability. If that was the case, then every theory would indeed be incommensurable with respect to every other theory. However, Feyerabend's contention that incommensurability only arises when considering realistic interpretations of *universal* theories, should have alerted the critics to the idea that non-universal, lower-level, theories are not incommensurable, though they are certainly inconsistent. At any rate, Feyerabend corrected this source of misunderstanding in later writings, see "Replies to Criticisms" pp. 230-1. What is interesting to note is that the logical empiricist ideal of logical deducibility and derivability between successive scientific theories runs into trouble when it comes to accounting for the relations between merely inconsistent theories: how can two inconsistent theories be put into exhaustive relations of deducibility, one from the other? Shared fundamental ideas will allow near complete deducibility, but a problematic residuum will remain.

<sup>34</sup> Laudan, *Progress and its Problems*, p. 146.

<sup>35</sup> This is the most ubiquitous criticism and the list of philosophers who make it is lengthy: Shapere, *Reason and the Search for Knowledge*, K.P. Parsons, "A Criterion for Meaning Change", *Philosophical Studies*, 28, (1975) pp.367-96. esp. p.370. And I. Szumilewicz, "Incommensurability and the Rationality of the Development of Science", *British Journal for the Philosophy of Science*, 28, (1977) pp. 345-50, are a representative class.

<sup>36</sup> Shapere, *Reason and the Search for Knowledge*, pp. 69-70.

The first assumption, that any change in meaning of theoretical terms results in incommensurability, is a thesis that Feyerabend has always denied. Feyerabend has always contended that it is universal theories which are incommensurable: lower level theories, theories *within* a universal theory, may be meaning variant, but this variance does not result in incommensurability. Theories within universal theories share too many assumptions for them to be incommensurable. Consider these two passages:

it seems reasonable to assume that the customary concept of meaning is closely connected, not with *definitions* ... but with the idea of a *fundamental rule*, or a *fundamental law*. Changes of fundamental laws are regarded as affecting meanings while changes in the upper layers of our theories are regarded as affecting beliefs only. There exists therefore a rather close connexion between meanings and certain parts of theories.<sup>37</sup>

A diagnosis of stability of meaning involves two elements. First, reference is made to rules according to which objects or events are collected into classes. We may say that such rules determine concepts or kinds of objects. Secondly, it is found that the changes brought about by a new point of view occur within the extension of these classes and, therefore, leave the concepts unchanged. Conversely, we shall diagnose a change of meaning either if a new theory entails that all concepts of the preceding theory have zero extension or if it introduces rules which cannot be interpreted as attributing specific properties to objects within already existing classes, but which change the system of classes itself.<sup>38</sup>

These two highly vague and ambiguous passages have been the source of much of the criticism of Feyerabend, especially the second passage. But I think a plausible position can be reconstructed out of the two passages.

Firstly, different universal theories are defined by Feyerabend as theories which have different fundamental laws. If fundamental laws are considered to be the source of meanings, then the terms in different universal theories will have no meaning in common. This is what Feyerabend is trying to say when he contends that there is a change of meaning when "all concepts of the preceding theory have zero extension". But, as the second passage attests to, for Feyerabend, there is a second type of meaning change wherein there is a change in the system of classes. If this second type of meaning change

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<sup>37</sup> "Replies to Criticisms" n. 27.

<sup>38</sup> *Phil. Papers. Vol. I.* p. 98.

is to be different from the first type, then we have to assume that some concepts of the older theory have non-zero extension in the new theory. This is another way of saying that the two theories must share some fundamental laws in common: the two theories are not different universal theories, and they therefore share some meanings in common.

Secondly, Feyerabend is working with some sort of notion of a hierarchical structure of theoretical systems, whereby changes in concepts at one level only effect changes in meanings of terms occurring at that level, or at lower levels. For example, even though both the wave and particle theories of light were intimately related to Newtonian mechanics, the victory of the wave theory of light did not occasion any change in basic Newtonian theory. This can be explained by saying that the hypotheses relating to light were at a lower level in the theoretical hierarchy than the fundamental Newtonian laws; consequently, a change in the meaning of light could only effect changes in concepts at the same level or lower in the hierarchy.

I think that this is a valid interpretation of the two passages above; but some philosophers have only seen paradoxes arising. For example, Shapere contends that "if a new theory entails that *one* concept of the preceding theory has extension zero, apparently no meaning change has taken place" and that "if *all but one* of the classes of the preceding theory have extension zero, again no meaning change has taken place". Finally, for the height of misrepresentation, Shapere contends that "if the extensions of all classes are changed radically, but not so much that the previous extensions are zero, again no meaning change has taken place".<sup>39</sup> All these examples of Shapere's are wrong, in that all three examples can be classified as a "change [in the] system of classes itself", which implies that, for Feyerabend, all three examples qualify as changes of meaning. Of course, the changes of meaning are not of a fundamental nature, and do not, therefore, result in incommensurability.

The second assumption, that the term 'incommensurable' is synonymous with the term 'incomparable', may be what you will find in a dictionary, but it is not how Feyerabend conceived the terms. It has been the point of this chapter to show that, for

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<sup>39</sup> All from Shapere, *Reason and the Search for Knowledge*, n. 78. p. 100.

Feyerabend, incommensurability arises between two theories only when the theories in question are universal theories; when they are interpreted in a particular manner; and when formalistic procedures of comparison and inter-theoretic relations are applied to those theories. Thus, a judgement of incommensurability is a relative judgement, not an absolute judgement. In fact, it could be said that, for Feyerabend, the idea that theories are absolutely incomparable is an impossibility.<sup>40</sup>

### 3.3.2 The Irrelevance of Reference.

One of the most popular forms of attack is to say that theories are commensurable because theories can be brought into relations of referential overlap, that is, putative incommensurable theories are not in fact incommensurable, because the two theories refer to the same things.<sup>41</sup> Even though these theories have been around a long time, Feyerabend has never responded to them at any great depth. It might be said that he hasn't responded to them because he can't. I don't think that this is the case: Feyerabend hasn't responded to the referential overlap theories simply because they are irrelevant.

Sankey has attributed to Feyerabend the belief that incommensurable theories are referentially discontinuous. But Sankey has also said that Feyerabend needs, and implicitly accepts, some sort of referential overlap, if Feyerabend is to call himself a

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<sup>40</sup> It is actually quite amusing to read lengthy articles criticising Feyerabend, at the end of which, means of comparison are proudly produced which Feyerabend had proposed before. D.W. Moberg, in his, "Are There Rival incommensurable Theories" *Philosophy of Science*, 46, (1979) pp. 244-62. esp. pp. 257-60, for example, proposes that, for two incommensurable theories, if one is inconsistent, and the other consistent, then the consistent theory should be preferred over the inconsistent one. In fact, Moberg spends *three pages* in coming to that conclusion. Moberg, *Ibid.*, p. 260, and Devitt, "Against Incommensurability" *Australasian Journal of Philosophy*, 57, (1979) pp. 29-50. esp. p. 47, both contend that some sort of relative measure can be made of how well incommensurable theories fit their own facts, the one that fits its own facts better is preferable. Well Feyerabend proposed both of these techniques, among others, as means of comparing theories, which do not rely on formalist assumptions. The second idea is especially important: it has always been upheld by Feyerabend as a means of comparing and testing realistically interpreted universal theories. See "Replies to Criticisms" pp. 232-4, and AM, pp. 282-4.

<sup>41</sup> This approach has a long history, starting with I. Scheffler, *Science and Subjectivity*, and H. Putnam, see for example his "How Not to Talk About Meaning: Comments on J.J.C. Smart" in his *Language and Reality: Philosophical Papers Vol.2*, pp. 117-131. Then gaining support and elaboration by P. Kitcher, "Theories, Theorists and Theoretical Change", *The Philosophical Review*, 87, (1978) pp. 519-47. K.P. Parsons, "A Criterion for Meaning Change", M. Devitt, "Against Incommensurability", and most recently H. Sankey, *The Incommensurability Thesis*, to name but a few.

realist.<sup>42</sup> The answer to this apparent contradiction is that Feyerabend accepts referential overlap, as long as we are careful as to what we mean by reference. Recall the discussion of Feyerabend's pragmatic theory of observation earlier in this chapter. The pragmatic theory of observation is predicated upon the idea that our theories are meant to be realistic accounts of the nature of the world, and that the independently existing world is the source of the perceptions we receive. In this situation, observation statements are those statements we are predisposed to utter when confronted with perceptual stimuli. This realism of Feyerabend's implies that all theories have common reference: all theories refer to the (one) world. The actual entities and processes which we take to be constitutive of the world may differ radically from theory to theory, but the referent is the independently existing world posited as causing the perceptions we have. Thus, particular common reference can be established, via perceptual stimuli, by ostension. For example, an observer can point to a bird and say 'this is a raven', a second observer can agree that what is pointed to is a bird and that the sentence 'this is a raven' is true. But the two observers may have radically different ideas as to what the raven is: the first observer may think of the raven in terms of the most recent biological theories, while the second observer may think of the raven in terms of an evil portent in an animistic/mythological world view. It could be argued that the two descriptions of the raven are not jointly satisfiable, but the fact that the two descriptions are not jointly satisfiable does not preclude the idea of common reference. A realist simply concludes that one, or both, of the descriptions are wrong, because the common world which both theories refer to can only have one nature. This conclusion is corroborated by the examples which Feyerabend uses when discussing incommensurable theories; for example, when Feyerabend discusses the incommensurable Aristotelian and Galilean world views with respect to their, jointly unsatisfiable, descriptions of the tower argument, the common reference of the tower, the rock, the ground, and the position where the rock hits the ground, is never denied, and couldn't possibly be denied: the

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<sup>42</sup> Sankey, *The Incommensurability Thesis*, for referential discontinuity, pp. 139-52. For Feyerabend's implicit acceptance of referential overlap see pp. 184-7.

common reference to an independently existing world is not the issue between incommensurable theories. The issue concerns the *nature* of that which is commonly referred to. In fact, it would be ludicrous for Feyerabend not to accept some sort of referential overlap; for example, when Feyerabend says that materialistic theories of mind, and dualistic theories of mind, are incommensurable, it would be ridiculous for him to deny that there is referential overlap: how could he deny that both theories refer to humans?<sup>43</sup>

So how does Sankey come to the conclusion that Feyerabend accepts radical referential discontinuity? Firstly, we may say that, for Feyerabend, meaning and reference are intimately connected: "meaning-talk can be replaced, without residue, by theory-talk"<sup>44</sup> and, since theories are to be interpreted realistically, the relations between meaning and reference are very close. This would seem to imply that, in the case of incommensurable theories, where Feyerabend has contended that there exists no common meanings, there would also be no common reference: there would be radical referential discontinuity. Consequently, Sankey, alluding to the second of the two passages quoted above concerning Feyerabend's views on meaning, contends that

if reference is determined by rules, then in order for there to be common reference the same objects must satisfy different systems of rules. But on the first alternative the same objects cannot satisfy both sets of rules: incompatible sets of rules are not jointly satisfiable. While on the second alternative no common objects can belong to both systems of classes. For if the new rules attribute no properties to members of old classes, then no criterial property specified in a new rule can be instantiated by any members of an old class. The system of classes must be completely disjoint. Thus, in both cases there can be no common reference between incommensurable theories, so the transition between such theories is referentially discontinuous.<sup>45</sup>

This argument is valid, *if* we assume that, for Feyerabend, reference is in fact determined by rules, and nothing else. But I don't think that this is the case, though Feyerabend does

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<sup>43</sup> Couvalis comes to a similar conclusion that a form of reference is involved in Feyerabend's talk of incommensurability. See his *Feyerabend's Critique of foundationalism*, pp. 87-126. Of course, Couvalis points out, in great detail, that the notion of reference to be found in Feyerabend is not that of the causal theory of reference, as proposed by Putnam and his intellectual cohorts. With this I totally agree.

<sup>44</sup> "Replies to Criticisms" n. 27.

<sup>45</sup> Sankey, *The Incommensurability Thesis*, p. 147.



seem to contend that reference is intimately connected to meaning, nevertheless, meaning and reference are not interchangeable. In Feyerabend's brand of realism there is always the minimal reference to an independently existing world, which is the cause of the perceptions we receive. Consequently, though it is true that incompatible sets of rules are not jointly satisfiable, this does not imply that there is, therefore, complete referential discontinuity. If we interpret Feyerabend as an idealist, then the above argument of Sankey's is valid. But if we interpret Feyerabend as a realist, then the argument is not valid.<sup>46</sup>

However, the more important considerations are that, (i) the reference of theories and theoretical terms, is not something that can be deduced from a theory: it is something we come to know by using and applying a theory. And (ii) that two incommensurable theories *have* referential overlap, and *where* this referential overlap is, is again something that can only be known through practical familiarity with the two theories, and through empirical investigation of the two theories: not simply through the comparison of logical consequences. The conclusion is that referential overlap theories are irrelevant to criticisms of Feyerabend's incommensurability.

### **3.4 Feyerabend's Commensurability.**

I want now to sketch some of the ways in which Feyerabend sees so-called incommensurable theories as, in fact, commensurable. In the following I will only concentrate on those techniques which Feyerabend presented and argued for with specific reference to incommensurability.

To begin with, we should "remember that the problem of incommensurability arises only when we analyse the change of *comprehensive cosmological points of view*-restricted theories rarely lead to the needed conceptual revisions". (AM, p. 284) What this implies is that the formal accounts of inter-theoretic relations, accepted by logical empiricists and Popperians, can still be applied to successive theories which are within

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<sup>46</sup>A second source for Sankey's charge of referential discontinuity is derived from passages found in SFS, the problems that arise for my interpretation of Feyerabend by these passages will be dealt with in the next chapter.

the same cosmological world-view. For example, the long development of quantum physics from the 20's to the present can be compared using all the Popperian and/or empiricist ideals. Contents can be compared, for successive quantum physical innovations use the same language. Measures of verisimilitude may even be calculable. Comparison of content-classes and refutation can all be calculated and used with every justification.

But, according to Feyerabend, if we want to keep a formalistic account of science, then changes of cosmological theories will become inexplicable: cosmological theories are formalistically incommensurable. Of course, many, if not most, philosophers have objected to this idea and chastised Feyerabend for proposing it. But Feyerabend never believed that theories are incommensurable, in the sense of absolutely incomparable. The fact that formal accounts seemed to lead to incommensurability was good enough reason for Feyerabend to look for an alternative account.

Feyerabend believed that his pragmatic theory of observation provided grounds through which we can understand the way in which universal-cosmological theories can be compared; for example, Feyerabend states that

both [incommensurable] theories may be able to reproduce the 'local grammar' of sentences which are directly connected with observational procedures. In this case the utterance of one of the sentences in question in accordance with the rules of the local grammar ... can be connected with *two* 'theoretical' statements, one of T, and one T' respectively ... We may now say that the empirical content of T' > the empirical content of T, if for every associated statement of T there is an associated statement of T', but not vice versa. And we may also say that T' has been confirmed by the very same evidence that refutes T if there is a local statement S whose associated statement in T' confirms T' while its associated statement in T refutes T.<sup>47</sup>

For Feyerabend "the local grammar of a statement is that part of its rules of usage which is connected with such direct operations as looking, uttering a sentence in accordance with ostensibly taught (*not* defined) rules, etc".<sup>48</sup> Once we get past the technical wording of these passages, the substantive content is very mundane indeed. According to

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<sup>47</sup> "Replies to Criticisms" p. 233.

<sup>48</sup> *Ibid.*, n. 32.

general relativity, light is bent by massively gravitating objects, consequently, during an eclipse, the light from a particular star should be seen in a particular point in the sky. On the other hand, according to classical mechanics light is always propagated rectilinearly, therefore, during an eclipse the light from a particular star should be seen in a particular point in the sky. As it turns out, the two positions are perceptibly different, therefore, an experiment can be conducted. The result of the experiment will confirm, or disconfirm, one or both of the theories in question; but one theory has not therefore refuted the other theory. Rather, what has happened is that one theory has been refuted by its own experience: "Incommensurable theories ... can be *refuted* by reference to their own respective kinds of experience; i.e. by discovering the *internal contradictions* from which they are suffering". (AM, p. 284) The two theories are not placed in relations of logical deducibility, one from the other, nor is there a formal notion of empirical content increase applied. But, nevertheless, the comparison of the theories is uncontroversial and clear.

An objection has been raised against the possibility of comparing incommensurable theories, given Feyerabend's pragmatic theory of observation. Shapere, for example, quotes this passage from Feyerabend: "Observational findings can be reinterpreted, and can perhaps even be made to *lend support* to a point of view that was originally inconsistent with them",<sup>49</sup> and concludes that this passage, and the pragmatic theory of observation generally, precludes the possibility of any sort of comparison. Observation is too malleable and any experience or observation which may be inconsistent with a universal theory can be reinterpreted so that the inconsistency no longer exists. In response we can say that Feyerabend does indeed conceive of observation and experience as inherently malleable; but it is this fact which drives Feyerabend's conception of comparison and test. Feyerabend contends that any one universal theory can be adapted to account for all empirical phenomena. To remedy this

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<sup>49</sup> "Problems of Empiricism" p. 202. Quoted by Shapere in *Reason the Search for Knowledge*, p. 77. Cf also B. Townsend, "Feyerabend's Pragmatic Theory of Observation and the Comparability of Alternative Theories" in R.C. Buck and R.S. Cohen (eds) *Boston Studies in the Philosophy of Science. Vol. VIII* pp. 202-11.

problematic situation, Feyerabend proposes that we should always work with a plurality of inconsistent, incompatible, high-level universal theories: theories which give different accounts of all empirical phenomena, and which, moreover, are essential for discovering new empirical phenomena in need of explanation, or for highlighting the relevance of previously disregarded empirical phenomena. If we are realists, then we must accept that only one, or none, of the extant alternatives can be correct. We are then driven to critically examine the various alternatives. But given that these high-level universal theories can be made to account for all empirical phenomena, the way in which we can create tests is via predictions of previously unknown phenomena, or via predictions of specific phenomena of a certain type, say, in the future. There is no room here for reinterpretation of the phenomena after the fact; *prima facie* evidence is thus provided for the relative strength of one of the alternatives over another, and for the inability of one of the universal theories to explain its 'respective kind of experience' according to its own principles. I think that the pragmatic theory of observation can indeed provide means by which universal, incommensurable, theories can be compared and that these means are uncontroversial and clear.

Feyerabend has stated that "incommensurability is no difficulty for the sciences ... it is a difficulty only for some very naive philosophical theories and, as these theories were regarded as essential ingredients of a certain type of 'rationality', for this type as well".<sup>50</sup> This is an important passage for understanding Feyerabend: for even though formalist theories are adequate for explicating the relations between theories within cosmological theories, the fact that they cannot explain the relations between cosmological theories is a deadly blow, not only to the particular theories, but to the whole programme of analysis. Feyerabend wants to produce an alternative account of rationality, one which can not only account for the relations that exist between theories within a cosmological theory, but also explain the relations that exist between cosmological theories.

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<sup>50</sup> "Third Dialogue" p. 154.

I don't think that Feyerabend ever arrived at a fully satisfactory alternative theory of rationality. But there are bits and pieces, asides, and many clues throughout his writings which seem to point in a certain direction. I don't have time to go into this in any detail, so a very quick sketch will have to suffice.

Feyerabend has said that cosmological theories can be compared in many ways; for example, he sees comparison via mathematical simplicity, coherence, many and daring approximations as opposed to few and safe approximations, number of facts predicted, conformity with basic theory and conformity with metaphysical principles, to name but a few.<sup>51</sup> These notions can be best described as values; they function in science like the virtues of Aristotle function in the eudemonic life. They sometimes conflict, sometimes reinforce each other. They are usually placed in hierarchical relationships, but these hierarchies are not rigid or well defined: they change over time and according to the situation. Mathematical simplicity, for example, is not a necessary aspect of scientific theories, and a theory exhibiting greater mathematical simplicity than another is not necessarily preferable. Science is an activity where skilled judgement is necessary. To acquire this judgement a scientist needs to immerse themselves in the practice of their chosen field.

In a theory such as this, the comparison of scientific theories is achieved through an in-depth study of the values that the theories exhibit and the relationship that is seen to exist between these values. For example, the scientific revolution associated with Copernicus and Galileo can be explicated as a far reaching change in the value-system hierarchy. It is this change in the value-system hierarchy, from Aristotelianism to the new science, which becomes the area for discussion of the relative merits of the alternative world-views: what values remained the same; where in the hierarchy these values were placed; which values were dropped; which values were introduced, and so on, are the sorts of questions which are asked. What is happening here is that debate is being broadened past simple empirical adequacy, apparent falsifications, and formalistic notions of theoretical preferability, which themselves only function as values amongst other

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<sup>51</sup> *Phil. Papers Vol. 1.* p. 16. fn. 39.

values. For example, the acceptance of relativity theory can be seen as being influenced by the values of mathematical simplicity, elegance, and such, just as much by relativity theory's empirical adequacy. In the debate between Einstein and Bohr over the adequacy of quantum mechanics, empirical adequacy was not the point at issue, the debate was conducted over what other sorts of values scientific theories should fulfil.<sup>52</sup>

### **3.5 Conclusion.**

Feyerabend never said that any two theories are incommensurable. For Feyerabend, incommensurability is the result of trying to apply formalistic, 'Rationalistic', notions of inter-theoretic relations to realistically interpreted, high-level, universal theories. Thus the conclusion that some theories turn out to be incommensurable was designed to be a *reductio ad absurdum* of the formalistic notions involved: that theories could be totally incommensurable is an absurd and ridiculous result. Feyerabend's alternative involves working with a plurality of theories, taking a values-oriented approach, and highlighting the reconceptualised importance of predictive empirical experience.

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<sup>52</sup> This sketch of the idea of values-based rationality is taken up again in chapters 5-7.

## 4 Feyerabend's Relativism

### 4.1 Introduction.

In the last chapter I contended that Feyerabend supports a minimal realism: there is a world; it has a nature. And that there is a minimal sense of accessibility to that world, given to humans causally via sense perception. These two postulates are essential for understanding Feyerabend's conception of incommensurability: two incommensurable theories, if interpreted realistically, cannot both be fully realistic accounts of the world; one, or both, must be faulty. Feyerabend's exhortation to actively search for incommensurable alternatives to dominant theories can be seen as a means to encourage deeper investigations as to the nature of the world. This is achieved in two senses: firstly, the realisation that there are incommensurable alternatives to a dominant theory, gives to the individual investigator, or community of investigators, psychological motivation to search for a more comprehensive theoretical standpoint. Secondly, and, for Feyerabend, more importantly, the proposal of incommensurable alternatives is necessary for the discovery of hitherto unthought of empirical facts.<sup>1</sup>

However, these conclusions seem to run counter to the assertions, which can be formulated independently of the issue of incommensurability, that Feyerabend is a relativist. In fact, Feyerabend has described himself as a Protagorean relativist; for example, Feyerabend states that "*Relativism* (in the old and simple sense of Protagoras) gives an adequate account of the situation [the particular relation between reason and practice which Feyerabend endorses] which thus emerges" (SFS, p. 9.)<sup>2</sup> Can these two positions, realism and relativism, be coherently, jointly, entertained? Even if the answer to that question is positive, does Feyerabend support such an account? Or has Feyerabend adopted a position of relativism in his later career in contradistinction to his earlier espousal of realism?

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<sup>1</sup> Cf. for example, Feyerabend's discussion of Brownian motion: the second law of thermodynamics was able to be refuted only after the kinetic theory had been proposed. AM, pp. 38-41.

<sup>2</sup> Cf. also SFS, pp. 79-86. and "Notes on Relativism", in FTR, pp. 19-89.

As a preliminary conclusion, Feyerabend has stated that "relativism is as much of a chimaera as absolutism, its cantankerous twin".<sup>3</sup> Notice here that relativism is opposed to absolutism, not to realism. Notice also that, for Feyerabend, the relativism/absolutism either/or dichotomy is illusory and does not capture any deep philosophical divisions. So too, I want to conclude, the realism/idealism dichotomy is, for Feyerabend, misleading in its mutual exclusiveness. Feyerabend's philosophy cuts across these dichotomies and, as such, contains aspects of all distinctions involved. Nevertheless, it is interesting to track Feyerabend's philosophy in terms of relativism and realism as it gives us insight into Feyerabend's unique metaphysical and epistemological outlook.

#### **4.2 Realism and Relativism.**

There is no single identifiable position which we can name unequivocally as relativism, or one identifiable position which we can unequivocally name realism. Consequently, to say that realism and relativism can be compatible depends on what substantive stance we take on these positions.

There is a very common tendency, identified by Richard Bernstein, to conceive the relativism debate in terms of an either/or dichotomy between 'objectivism' and 'relativism':

By 'objectivism,' I mean the basic conviction that there is or must be some permanent, ahistorical matrix or framework to which we can ultimately appeal in determining the nature of rationality, knowledge, truth, reality, goodness, or rightness

and, conversely:

relativism is the basic conviction that when we turn to the examination of those concepts that philosophers have taken to be the most fundamental ... we are forced to recognize that in the final analysis all such concepts must be understood as relative to a specific conceptual scheme, theoretical framework, paradigm, form of life, society or culture ... For the relativist, there is no substantive overarching framework or single metalanguage by

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<sup>3</sup> Feyerabend, "Concluding Unphilosophical Conversation" p. 515.



which we can rationally adjudicate or univocally evaluate competing claims of alternative paradigms.<sup>4</sup>

Bernstein contends that there has been a tendency to conceive the choice between these alternatives as an either/or situation: there is no middle ground. Once objectivism is rejected then, it is felt, there is no other alternative except the radical relativism adumbrated above. Bernstein believes that this either/or dichotomy is false and needs to be relinquished. This point of Bernstein's is exactly Feyerabend's point. In conducting his *reductio* of what Feyerabend calls 'Rationalist' philosophy, which can be identified with Bernstein's 'objectivism', Feyerabend plays upon the either/or dichotomy: by showing that 'Rationalist' philosophies cannot explicate the rationality of science, and that science seems to work in ways directly counter to the presuppositions of 'Rationalist' philosophy, Feyerabend contends that the 'Rationalist', according to their own philosophy, must accept that science is irrational and relativistic. This conclusion is not meant to be taken literally, it is meant to be understood in the context of a *reductio*; as Feyerabend has pointed out, the employer of a *reductio* argument does not have to accept the premises upon which the *reductio* is conducted. Feyerabend wants to do away with 'Rationalism', and this includes the either/or dichotomy with which it is indelibly linked.<sup>5</sup>

Given this analysis, the variety of relativism as characterised by Bernstein is not the relativism which Feyerabend wants to defend. This may be just as well, for it is this variety of radical relativism which falls victim to the oft-repeated charge of incoherence, that is, that relativism of this sort is self-defeating. The incoherency predicated of relativism rests on a very simple argument: if all truth is relative to system, scheme, paradigm, form of life, then the status of the relativistic claim itself is problematic. If the

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<sup>4</sup> R.J. Bernstein, *Beyond Objectivism and Relativism: Science, Hermeneutics and Praxis*, p. 8. J. Margolis, in his *Pragmatism Without Foundations: Reconciling Realism and Relativism*, pp. 69-73. has taken issue with Bernstein for presenting relativism in such a manner. Relativism, Margolis contends, is not synonymous with the extreme radical relativism which Bernstein portrays. While we may agree with Margolis's sentiments, he has completely missed the point of Bernstein's analysis. Bernstein is presenting a tendency in modern thought, one which he does not necessarily ascribe to himself. This tendency, caught in the either/or dichotomy, places relativism with scepticism, irrationalism, nihilism, and other 'bad' positions. Whether relativism, interpreted correctly, can be rescued from this association, a claim that Margolis (and Feyerabend and Bernstein) makes, is beside the point for the purposes which Bernstein appropriates the term 'relativism' for.

<sup>5</sup> For details, see chapter 1.

relativistic thesis is considered absolutely true, then the thesis is wrong, there are non-relativistic, absolute truths. Alternatively, if the thesis is taken literally, then the relativistic claim itself is true only in relation to a paradigm or form of life, and therefore does not preclude the possibility of absolute truths.<sup>6</sup>

The incoherency argument imputing self-refutation to radical relativism does not have to be questioned: it can be accepted without qualms. But this does not then carry the further implication that any form of relativism is thus refuted. Rather, we can follow Margolis and say that any form of relativism which is susceptible to the charge of incoherence is 'uninteresting' and should be avoided from the outset.<sup>7</sup> The further question to be asked is whether different, non-radical, forms of relativism can be proposed which do not fall victim to self-refutation. Margolis's solution is to say that

relativism can be made formally compatible with the admission of the standard use of 'true' and 'false,' provided only that the set of judgments that take the values 'true' and 'false' be segregated from those that take the weaker values. Any such system, in which the ascription of the weaker values presupposes a domain to which the stronger values apply and depends upon the application to that domain of the stronger values may be termed a robust relativism. For example ... if [in relation to Quine] ... ontology can be segregated from a certain minimal range of empirical findings about behavioral responses to 'non-verbal stimulation' - then, *to that extent*, ontology can be treated relativistically, relative to a body of empirical knowledge or science that is *not* construed relativistically.<sup>8</sup>

This solution is identical in form to my contention that Feyerabend accepts a minimal realism with a minimal, two-way, causal accessibility to that world. It is easy to see how such a conception can obviate the charge of incoherence: the 'all truths are relative'

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<sup>6</sup> Comprehensive presentations of this argument can be found in H. Siegel, *Relativism Refuted: A Critique of Contemporary Epistemological Relativism*. And in J.F. Harris, *Against Relativism: A Philosophical Defense of Method*. It is quite ironic that both authors, in 'refuting' radical relativism, a position they have to push their protagonists into, do not then accept radical, 'vulgar' absolutism. (Siegel pp. 163-4.) The irony consists in the fact that Harris, an absolutist, and Margolis, a relativist, both look to Pierce and pragmatism as a means of providing their respective absolutist/relativist frameworks. This seems to suggest that once we move away from straw-positions and obfuscating, partisan terminology, the substantive positions arrived at meet in the middle, so to speak. It is also interesting to note that Harris's book, as the title suggests, is ostensibly an attack upon Feyerabend: Feyerabend is presented as the arch-relativist. However, apart from out of context quotes and asides, nowhere in the book are Feyerabend's ideas examined in any detail. Such a ready pigeon-holing of Feyerabend for rhetorical purposes (we need demons to fight) is a regrettable tendency.

<sup>7</sup> Margolis, *Pragmatism Without Foundations*, p. 11.

<sup>8</sup> *Ibid.*, p. 19.

formula, with its obvious logical blunder, is not asserted. Rather, many, possibly uncountable, universal, absolute truths, of a particular kind, are acceptable. Consequently, the idea that relativism is presented as true is not self-defeating: a relativist thesis can be an absolute truth, alongside the other absolute truths.

For Feyerabend, the non-relativistic truths are the low-level truths associated with perceptual stimulation. This is to be understood as in no way returning Feyerabend to some sort of positivistic position with regards to empirical experience: sense experience is not completely indubitable, and does not provide the foundations upon which all knowledge is built. Certainly, all knowledge has connections, and makes reference to, sense experience, but the structure of knowledge is not reducible to sense experience. An example may clarify the situation: imagine a westerner and a native walking through the jungle; they do not know each others language and little of each others culture; suddenly, from very close by, a ferocious roar is heard; both westerner and native show obvious signs of fear and assume defensive positions. In this example, it does not matter what the higher level world views, forms of life, paradigms and so on, of the westerner and native are; in fact, we may presume that they are radically different. But their difference of world view in no way obviates the fact that they both hear a loud, threatening sound and react accordingly to it. *The existence of the sound was an objective fact.* It is the *interpretation* of sense experience, our higher level theorising about the world, which is conducive to a relativistic rendering.

One can even state the case for minimal realism, with two-way causal interaction with, and accessibility to, this minimal realist world, in a much stronger form: minimal realism is a necessary condition of any social relativism. This might sound a little drastic, but even David Bloor, one of the defenders of the 'strong programme' in the sociology of knowledge, adopts just this position in order to avoid the charges of incoherence levelled at the strong programme. Bloor contends that

No consistent sociologist could ever present knowledge as a fantasy unconnected with our experiences of the material world around us ... For consider how such a fantasy would have to be transmitted to new members of society. It would depend on education, training, indoctrination, social

influence and pressure. All of these presuppose the reliability of perception and the ability to detect, retain and act upon perceived regularities and discriminations. Human bodies and voices are part of the material world and social learning is part of learning how the material world functions ... Materialism and the reliability of sense experience are thus presupposed by the sociology of knowledge and no retreat from these assumptions is permissible.<sup>9</sup>

The assumption of veridical sense perception, and successful meaningful communication between human beings, are all too obvious requirements for a *social* theory of knowledge. If these assumptions were not met, then the resultant theory could not, by definition, be a social theory; rather, it would be a subjectivistic-solipsistic theory of knowledge.

This minimal realist position can also be used to counter another objection often raised against relativism: the so-called 'bridgehead' argument. Stephen Lukes characterises the bridgehead argument as follows:

in the very identification of beliefs and ... of belief systems we must presuppose commonly shared standards of truth and of inference, and ... we must further presuppose a commonly shared core of beliefs whose content or meaning is fixed by application of the standards.<sup>10</sup>

If this bridgehead is denied, then it is asserted that relativism is incoherent. This argument is crucially dependent upon what is considered necessary to be in the bridgehead, and what not. It seems to me that the minimal realism outlined above provides an adequate bridgehead for the process of understanding and communication to proceed. But the type of bridgehead envisaged by Lukes and others is of a far greater scope and substantiality. Thus Barnes and Bloor deny the existence of a bridgehead, in the sense of the above quote, but as we saw, they support the idea of veridical sense perception as a precondition of social existence. What Barnes and Bloor want to emphasise is that "although we may well all share the same un verbalized environment,

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<sup>9</sup> D. Bloor, *Knowledge and Social Imagery*, 2nd edition. pp. 33-4.

<sup>10</sup> S. Lukes, "Relativism in its Place", in M. Hollis, and S. Lukes (eds) *Rationality and Relativism*, p. 262. See also the papers by Hollis, "The Social Destruction of Reality", who believes that the nature of the bridgehead can be ascertained *a priori*. And Newton-Smith, "Relativism and the Possibility of Interpretation", who believes that the nature of the bridgehead is ascertained *a posteriori*. Both papers in the volume above.

there are any number of equally reasonable ways of speaking of it."<sup>11</sup> This idea, that even though we may all receive the same sense perceptions, we may nevertheless construct different, equally reasonable, interpretations of that experience, is labelled by Lukes "a thoroughgoing relativism of truth and of reason ... applied to the practical beliefs of primary theory no less than to all other beliefs".<sup>12</sup> But this is completely unfair and leads to a dangerously broad conception of what a bridgehead is meant to supply. On this conception, a bridgehead between cultures or forms of life must provide a unique and totally true interpretation of sense experience. Expressed in this way, I'm sure that even Lukes would not accept that requirement. Barnes and Bloor want to say that the Karam people, who classify birds and bats, but not the Cassowary, as 'Yakts', do so for reasons which are not obviously confuted by perceptual experience.<sup>13</sup> Our scientific classification of animals is not something given to us by raw experience; rather, it is the result of centuries of biological research and carries with it huge amounts of theoretical machinery.<sup>14</sup> All that we need to begin the process of understanding and communication is the idea that sense perception is common to all people, any more than that is unnecessary baggage which may hinder the process in question.

It may be replied that this relativistic thesis does not yet escape the charge of incoherency: the relativistic thesis is not a low-level empirical fact, it is a high level theoretical construct, and, as such, is itself a relative truth. This is a valid response, but I will leave further discussion of this point until a later section. Suffice it to say that Feyerabend believes relativism to be an empirical hypothesis: an hypothesis which it is possible to refute. "It is a matter of research and not of philosophical fiat to decide whether the world we inhabit resembles a duck-rabbit picture." (FTR, p. 81.) Be that as it may, it seems that a coherent argument can be presented in support of the compatibility of realism and relativism.

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<sup>11</sup> B. Barnes, and D. Bloor, "Relativism, Rationality and the Sociology of Knowledge" in Hollis and Lukes, *Rationality and Relativism*, p. 40.

<sup>12</sup> Lukes, "Relativism in its Place", p. 266.

<sup>13</sup> Barnes and Bloor, "Relativism ...", pp. 38-40.

<sup>14</sup> Who, on sense perception alone, would have conceived that whales and dolphins are more closely related to humans than are birds and reptiles?

### 4.3 Feyerabend's Changing Attitudes Towards Relativism.

#### 4.3.1 Science in a Free Society.

Relativism, of some sort, could never be denied of Feyerabend's philosophy, but it is not usually the simplistic relativism of the sort which falls victim to charges of incoherence. In fact, Feyerabend is a realist in many respects and a relativist in others.<sup>15</sup> Relativism is not an overarching presupposition of his philosophy. That being said, if we look over his career we find radical realism defended in his early papers, followed by radical agnosticism,<sup>16</sup> followed by radical relativism, and finally, some sort of in-between realism/relativism mix. Thus Feyerabend's position on the relativism issue has seemed to change with every new paper he published, but there does seem to be a general trend which can be delineated, and which presents a coherent version of relativism whilst remaining minimally realist.

Feyerabend's radical relativistic period is best represented in his book, *Science in a Free Society*. Here we find statements which cannot fail to be interpreted in the most extreme relativistic manner. For example, Feyerabend contends that

realism may be interpreted as a *particular theory* about the relation between man and the world, and it may be interpreted as a *presupposition of science* (and knowledge in general). It seems that most philosophical realists adopt the second alternative- they are dogmatists. But even the first alternative can now be criticized and shown to be incorrect. All we need to do is to point out how often the world changed because of a change in basic theory ... we certainly cannot assume that two incommensurable theories deal with one and the same objective state of affairs (to make the assumption we would have to assume that both at least *refer* to the same objective situation. But how can we assert that 'they both' refer to the same situation when 'they both' never make sense together? ... ) Hence, unless we want to assume that they deal with nothing at all we must admit that they deal with different worlds and that the change (from one world to another) has been brought about by a switch from one theory to another ... Speaking in this manner we no longer assume an objective world that remains unaffected by our epistemic

<sup>15</sup> See "Concluding Unphilosophical Conversation" p. 507, "I confess to be a fervent relativist in some senses, I am certainly not a relativist in others".

<sup>16</sup> Actually, I don't know whether it was agnosticism, or whether it was simply silence.

activities ... We concede that our epistemic activities may have a decisive influence even upon the most solid piece of cosmological furniture - they may make gods disappear and replace them by heaps of atoms in empty space. (SFS, p. 70.)

This passage, as it stands, is quite absurd, and seems to run counter to my suggestion that Feyerabend avoided the incoherency charge by supporting a minimal realism. It also quite explicitly vindicates Sankey's claim that Feyerabend believes in referential discontinuity between incommensurable theories. But, somewhat paradoxically, it contains the beginnings of Feyerabend's more coherent account, which he developed over subsequent papers, and which saw him return to a realistic position sufficient to support the account of incommensurability presented in the last chapter. At the time of *Science in a Free Society*, however, a coherent position is very hard to find.

In this book, Feyerabend contends that a central problem to be addressed is the relation between reason and practice. He sees three different manners in which reason and practice might be supposed to interact. Firstly, Feyerabend proposes what he here calls 'idealism'. This position amounts to what he elsewhere calls 'Rationalism', and what Bernstein calls 'objectivism': "*Idealism* assumes that practice ... is crude material to be formed by Reason ... it is the conscious and systematic application of Reason to a partly structured, partly amorphous material that gives us Science" (SFS, p. 7). A second position, which Feyerabend calls 'naturalism',

assumes that history, the law, science are already as perfect as they can be ... the attempt to rearrange science or society with some explicit theories of rationality in mind would disturb the delicate balance of thought, emotion, imagination and the historical conditions under which they are applied and would create chaos, not perfection ... To understand all the many possibilities of Reason ... one has to see it in action, one has to analyse history and its temporal products. (SFS, p. 7)

Feyerabend goes on to say that both idealism and naturalism have disadvantages and that these disadvantages can be removed by "combining naturalism and idealism and postulating an interaction of Reason and Practice". (SFS, p. 8.)<sup>17</sup> According to Feyerabend, interactionism

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<sup>17</sup> Italicised in original.

means that Reason and Practice enter history on equal terms. Reason is no longer an agency that directs other traditions, it is a tradition in its own right with as much (or as little) claim to the centre of the stage as any other tradition. Being a tradition it is neither good nor bad, it simply is. (SFS, p. 8)

I find these passages very confused. The last quote explicitly states that Reason is a separate, independent tradition: just one tradition among many traditions. But if the solution to the problem of the relation between reason and practice is an interactionism between reason and practice, as Feyerabend contends, then this seems to imply that reason is involved in all practices. Interpreted in this way, reason does seem to have pride of place in the centre of the stage, in that it is indispensable to the ongoing practice of all traditions. On the other hand, Feyerabend's comments concerning idealism and naturalism seem to suggest that reason is not an independent tradition in its own right, but that we can have an attitude towards a particular 'reason' which sees it *as* independent from the particular practice to which it is applied. There seems to be two different strands of thought here which Feyerabend runs together; for example, Feyerabend states that "reason and practice are not two different kinds of entities but *parts of a single dialectical process*" (SFS, p. 25), this implies that *within* a practice a process operates whereby abstract formulisations are derived from the ongoing activities of that practice. These abstract formulisations then act as guides to ongoing activity, but can still be changed and affected by the ongoing activity in a negative feed-back loop. But he then goes on to say that

*What is called 'reason' and 'practice' are ... two different types of practice* the difference being that the one clearly exhibits some simple and easily producible formal aspects thus making us forget the complex and hardly understood properties that guarantee the simplicity and producibility while the other drowns the formal aspects under a great variety of accidental properties. But complex and implicit reason is still reason and a practice with simple formal features hovering above a pervasive but unnoticed background of linguistic habits is still a practice. (SFS, p. 26)

This passage assumes a difference, in contradistinction to what Feyerabend seemed to say before, between reason and practice. Rather than a single dialectical process connecting reason and practice, we have reason pragmatically distinguished from any



practice; and practice, though incorporating unarticulated reason, as nevertheless distinct from reason. Feyerabend seems to confuse the distinction between Reason, with a capital R, as something universally applicable to all practices, and reason, with a lower case r, the reason *of a practice*. It seems plausible to assume that a particular practice can arrive at an abstract, formulised, representation of the reason deemed to be implicit within the practice, while not supposing that this reason therefore constitutes an independent tradition.

Another instance of Feyerabend's confusion at this time can be found in his talk of traditions. Feyerabend presents the thesis that "Traditions are neither good nor bad, they simply are". (SFS, p. 27.) And a second thesis that "A tradition assumes desirable or undesirable properties only when compared with some tradition"; (SFS, p. 27.) that is, objective-sounding judgements concerning the merits of traditions only arise from the view-point of a particular tradition which is presupposed in the judgement. Consequently, for Feyerabend, objective-sounding judgements are not, in fact, objective, but are inherently subjective:

The subjectivity is noticed as soon as participants realize that different traditions give rise to different judgements. They will then have to revise the content of their value statements just as physicists revised the content of even the simplest statement concerning length when it was discovered that length depends on reference systems ... those who still cling to absolute lengths cannot pride themselves on forming a special school of especially astute physicists who have overcome relativity. They are just pigheaded, or badly informed, or both. (SFS, p. 28.)

It seems to me that these passages are contradictory: if it is necessary to revise value statements once it is realised that these judgements are subjective and tradition dependent, then there is no sense in which the judgements, for example, true and false, can be applied at all. The revision of value statements pursuant upon the realisation that these statements are subjective, is not a revision, in the sense of a re-ordering of value statements, but a decision to rescind *all* value statements and to cease engaging in any evaluative process whatsoever. Alternatively, the analogy with scientific decision making directly subverts this conclusion. Granted, scientists did, in fact, revise the content of

their theoretical system once it was realised that length is relative to a reference frame. But in this situation the value statement true, once predicated of the concept of absolute length, was replaced by the value statement false. The relativistic conception of length as relative to reference system, was then considered true. The revision which took place was a re-ordering of value statements, not a relinquishment of evaluation itself.

If we take the analogy with scientific decision making even more concretely, then the situation is even worse for the prospects of a coherent account. The classical physicist who adheres to absolute length can be said to be upholding the traditions of classical physics, as such, Feyerabend's evaluation of that physicist as pig-headed is illegitimate: traditions are neither good or bad. The supposedly necessary revision of physics was not, in fact, necessary because relativistic and classical physics are different traditions and the evaluation of their respective merits is therefore subjective.

Feyerabend can reply to this by saying that the analogy with science was intended as just that, an analogy: it was meant to convey the idea that even the most basic of evaluations can be changed, including that most basic of evaluations, that there are tradition independent criteria of evaluation. In the case of the evaluation of competing scientific theories, Feyerabend can say that differential evaluation is possible because the various scientific theories are within the tradition of science. It is only once we attempt to comparatively evaluate whole forms of life that evaluation becomes inapplicable. For example, a comparative evaluation of western scientific culture and, say, Azande culture is, on this account, impossible.

That may be so, but Feyerabend needs to give an account of where the line is between legitimate, intra-traditional evaluation, and illegitimate extra-traditional evaluation. Feyerabend has often contended that classical and relativistic physics are incommensurable theories: they are different paradigms. But Feyerabend has also often contended that incommensurable theories can be legitimately comparatively evaluated. If incommensurable theories can be evaluated, then what distinguishes forms of life such that they cannot be evaluated? Feyerabend has also objected to the idea that there is some monolithic unitary structure called 'science': he sees science as a heterogeneous

collection of disparate paradigms, ideas and practices. If there is no unity in science, then what constitutes the form of life of western scientific culture, as distinct from other non-scientific cultures and forms of life? At this stage, answers to these questions are not forthcoming.

A third example of the confusion evident in *Science in a Free Society* comes when we find that Feyerabend makes a personal confession as to a realist conversion, (SFS, p. 113.) and contends that while he defends a political relativism, where all traditions have equal rights, he nevertheless claims that "*Philosophical relativism ... the doctrine that all traditions, theories, ideas are equally true or equally false or ... that any distribution of truth values over traditions is acceptable ... is nowhere defended in the present book*". (SFS, p. 83.) However, two pages before this passage we find him stating that "classifying traditions as true or false ... means projecting the point of view of other traditions upon them" (SFS, p. 81.) so that there is no such thing as a tradition independent truth: ascriptions of truth are relative to a tradition doing the ascribing. Something which is true from the perspective of one tradition can be equally validly false from the perspective of a different tradition. This idea *is* philosophical relativism, in the usual sense of the term, yet Feyerabend denies that he supports philosophical relativism. It is possible that Feyerabend can avoid a charge of contradiction in this instance by contending that the intuitive interpretation of terms such as true and false, are inherently objectivistically tainted, and that Feyerabend, therefore, rejects the usual interpretation of the terms and, instead, uses the terms as applicable only in relation to a particular tradition. From this perspective the contention that all traditions are equally true or equally false can be consistently denied, for it amounts to the claim that, from the perspective of a particular tradition, science, for example, all traditions are equally true or false. But from the perspective of science, all traditions are not equally true or false: from this perspective, science itself is seen to be true, while magic, witchcraft and astrology are not. Though Feyerabend can avoid the charge of contradiction, his evasion is not very convincing. Regardless of any such manoeuvring, Feyerabend is still left with a version of relativism which it is difficult to reconcile with his professed/confessed

realism. At any rate, the lengthy passage quoted at the beginning of this section explicitly denied the validity of any realistic thesis.

To conclude this section, I want to say that the confusion and inconsistencies that are apparent in SFS, seem to stem from an as yet unsuccessful attempt by Feyerabend to produce a workable, coherent theory which avoids both radical realism and radical relativism. It is my contention that the ideas that are introduced in SFS are developed in subsequent articles and many deficiencies rectified.<sup>18</sup>

#### 4.3.2 Farewell to Reason.

In *Farewell to Reason*, Feyerabend recast the distinction he had previously made between idealistic and naturalistic attitudes towards traditions in terms of theoretical/abstract traditions, on the one hand, and empirical/historical traditions on the other. Theoretical traditions, which Feyerabend sees exemplified by Parmenides, Plato and modern 'Rationalists' is the now familiar position where

The members of theoretical traditions identify knowledge with universality, regard theories as the true bearers of information and try to reason in a standardised or 'logical' way. They want to bring knowledge under the rule of universal laws. Theories, according to them, identify what is permanent in the flux of history and thereby make it unhistorical. (FTR, p. 118.)

The appeal, and much of the justification, of a theoretical/abstract approach, consists in its use of logical proofs; that is, if we want a complete, interconnected, system of knowledge, then we need to be able to unambiguously demonstrate the interconnections between different parts of the system, and the consequences derivable from the system. I think that Feyerabend is right when he stresses that in such a system abstract concepts are a necessity. The main attraction of such a position is that if you follow a given

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<sup>18</sup> In fact, if we look over Feyerabend's entire career, SFS, and other articles of that period, appear as anomalies in the general trend of Feyerabend's philosophy. Without this period, Feyerabend's philosophy is surprisingly, though not totally, consistent. It is also interesting to note that many modern day criticisms of Feyerabend utilise SFS as their main point of attack, then read back from SFS to his other writings, interpreting them from the perspective of SFS. From the above analysis in the text, it is not surprising to then find Feyerabend's views radically incoherent. But I believe that this is the wrong way of conceiving of Feyerabend's philosophy: SFS must be considered to be anomalous, and therefore, though not dispensable, at least secondary in the interpretation of Feyerabend's views.

procedure, then the conclusion or end-product of this procedure will not only be rational, but also, hopefully, unique. Provided, of course, that the procedure followed was the correct procedure. But if the concepts involved in both the procedure itself and the subject matter to which the procedure is applied are ambivalent or ambiguous, then the conclusion or end-products of the procedure will be either ambiguous or multifarious. Different people will interpret the ambiguous concepts differently. The hoped for unique rational end-product will not result. Consequently, to gain better results from proof procedures, concepts must be unambiguously defined: the commonsense multiplicity must be simplified and the core meaning abstracted from the putative accidental features given to concepts in everyday affairs.

But Feyerabend argues that if these procedures, harmless in themselves, and, in fact, a welcome addition to epistemic variety, are regarded as the sole source of true knowledge, then this is an unfounded, question-begging, claim:

We may agree that abstract notions and principles can be connected more easily than practical (empirical) concepts ... But the fact that simple ideas can be connected in simple ways gives the resulting propositions special authority only if everything can be shown to consist of simple things - which was precisely the point on which disagreements arose! (FTR, p. 67.)

If concepts are complex and have different meanings and significance in every different situation in which they are applicable, then the idea of abstract concepts divests concepts of important aspects of their meaning: it creates a situation wherein the applicability and adaptability of concepts to varying situations is greatly curtailed.

I think it is easy to see where Feyerabend is going with this. Concepts should never be tied down to particular definitions, this only gives the impression that they cannot be changed. If we look at the history of any concept we would like to choose, we will find that, in fact, concepts have continually evolved over time. For example, the concept of an atom is still used today, even though this concept has undergone radical changes over the millennia of its use. Something of the infinite Parmenidean 'ones' of the original Greek atomists survived up to Dalton, but the subsequent development of the concept: from indivisibility, to divisibility, through the solar system analogy, to the

current quantum-mechanical models, have left us with a concept of the atom with few similarities with that of the original Greek atomists. If we had been resolved to stick with *the* concept of the atom at any one stage in history, believing that we couldn't possibly mean anything else by the term atom, then the evolution and adaptation of the concept of the atom to the changing circumstances and roles in which it was and has been placed, would have been delayed and obfuscated.

On the other hand, according to Feyerabend, empirical traditions, or, equivalently, common-sense views, or practical traditions,

contain subtly articulated ontologies ... [where] each entity behaves in a complex and characteristic way which, though conforming to a pattern, constantly reveals new and surprising features and thus cannot be captured in a formula; it affects, and is affected by, other entities and processes constituting a rich and varied universe. (FTR, p. 64.)

In a tradition of this sort, knowledge is "a collection of opinions, each of them obtained by procedures appropriate to the domain from which the opinions arose." (FTR, p. 72.)

The members of historical traditions emphasise what is particular ... They rely on lists, stories, and asides, reason by example, analogy, and free association and use 'logical' rules when it suits their purpose. They also emphasise the plurality and, via the plurality, the history dependence of logical standards. (FTR, p. 118.)<sup>19</sup>

Feyerabend sees the forms of knowledge produced by such a tradition as valid knowledge of a specific sort. The juxtaposition of such empirical traditions with

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<sup>19</sup> There is some confusion in Feyerabend's discussion of empirical and theoretical traditions. The distinctions given above seem to imply that theoretical/abstract traditions, in being opposed to historical traditions, are therefore ahistorical. However, Feyerabend contends that "abstract traditions are not alternatives of historical traditions; they are special parts of them ... The dichotomy between historical traditions and abstract traditions ... does not reflect a real difference: *all* traditions are historical traditions" ("Historical Background: Some Observations on the Decay of the Philosophy of Science", in *Phil. Papers Vol. 2*, p. 8. Cf. also FTR pp. 126-7.) That is, all traditions are inevitably historically bound and infused with the psycho-social, cultural and intellectual milieu of the time. It is important to note that this historical relativism is no absolute obstruction to the understanding of different cultures in time and space: Feyerabend himself has examined Galileo and Homeric Greeks and attempted to understand them as they were. This historical relativism only implies that the way in which we study other cultures and previous times, must not be undertaken from the perspective of our own culture, or with preconceived notions as to what is rational and objective. Looked at in this way, apparently ahistorical, absolutistic perspectives are simply disguised relativistic ones where absolutist/objectivist sounding phrases give the impression of objectivity, but only serve to obscure the perspective from which the judgements are made. But this form of relativism is not a very radical one: it is not a relativism which cuts off communication and understanding, and it does not preclude the idea of the acceptability of a minimal realism.

theoretical traditions does not derive simply from the *use* of abstract concepts and abstract generalisations within theoretical traditions: no tradition, theoretical or empirical, can exist without abstractions and generalisations. Rather, it is the attempt to systematically organise all knowledge in the form of abstract concepts and abstract generalisations, and to then make the further implication that knowledge, true, *real* knowledge, is identical with such abstract structures, which Feyerabend sees as the objectionable feature of theoretical traditions. "Problems of reality' arise [only] when the ingredients of complex worlds of ...[the empirical, commonsense] kind are subsumed under abstract concepts and are then evaluated, i.e. declared to be either 'real' or 'unreal' on that basis" (FTR, p. 64.) Feyerabend is claiming that there are many different forms of knowledge, and many different aspects of reality. Theoretical traditions deny this plurality: theoretical traditions are predicated upon the idea of a metaphysical dualism between appearance and reality. The inherent non-universalisability and multifariousness of sense experience, which gives rise to loose and adaptable concepts, is devalued in such a system and labelled merely appearance, while the universalisable and strict concepts derived through abstraction are considered to be indicative of reality. Feyerabend does not deny that abstract/theoretical traditions produce knowledge, of a specific sort, nor does he deny that abstract/theoretical traditions can realistically describe aspects of the universe. What Feyerabend does deny is the contention that a theoretical/abstract approach is an exclusive approach to knowledge, and that the results of its procedures, and its procedures alone, are 'real'.

In this formulation there is no 'rationality' or 'reason', conceived of as an independent tradition, separate from all other traditions. As we saw, this caused trouble in Feyerabend's earlier formulations of the issue. What we have instead is a theoretical/abstract tendency of thought which may arise in any particular tradition:

we arrive at the hypothesis that there exist many different ways of living and of building up knowledge. Each of these ways may give rise to abstract thought which in turn may split into competing abstract theories. Scientific theories [for example] ... branch out in different directions, use

different ... concepts and evaluate events in different ways. (FTR, pp. 74-5.)

What Feyerabend is pointing out is that there can be many different knowledge systems, many different traditions, and that there are two basic attitudes that we can take towards these traditions: we can accept the abundance of facts and particulars given to us by our empirical experience of the world, both social and non-social, and no more; or, we can try to look for abstract generalisations and regularities behind the particulars, thereby 'annihilating' the particulars, as it were.

That Feyerabend is describing tendencies of thought with this theoretical/empirical dichotomy, rather than explicit traditions in themselves, can be illustrated with reference to Feyerabend's ideas on science. Science, as an activity, and a tradition, is exceedingly heterogeneous. Some scientists look for the ultimate abstract generalisations underpinning empirical experience, while other scientists are instrumentalists and are simply concerned with the outcomes of particular experimental situations. The distinction between theoretical and experimental physicists can be roughly interpreted along these lines. If we then look towards the interpretation of science as a whole, we find an analogous situation: philosophers have traditionally looked for the abstract, universal canons of rationality underpinning particular examples of scientific activity. On the other hand, in recent decades historians and sociologists of science have looked towards the idiosyncrasies of particular scientific episodes and denied that a universal abstract characterisation of science can be found. But the point to be emphasised is that science, as a tradition, is neither theoretical or empirical, it contains aspects of both sorts of tendencies of thought, in participants and observers. Feyerabend, of course, has championed the second type of interpretation of science. Even though Feyerabend applauds Aristotle because "Aristotle ... attempted a synthesis of historical and theoretical (abstract) thought",<sup>20</sup> implying that this is the way to go, he nevertheless believes that the fact that science is neither theoretical or empirical, but both, is ultimately a meta-argument in favour of the empirical approach. We can make an

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<sup>20</sup> "Historical Background ...", p. 12.



Aristotelian analogy here: theoretical/abstract and empirical attitudes towards traditions, in their most extreme formulations, constitute the vices to be avoided, virtue is somewhere in between. However, in practical judgement of the right course of action, it is better to lean towards the less pernicious vice.

Given this analysis, what then, for Feyerabend, is real? It would seem that he would want to deny the reality of abstract ordering schemes in favour of the multiplicity of ideas and concepts associated with an empirical attitude towards traditions and experience. The local, practical knowledge intimately tied to sense experience, though multitudinous, as the only reality. This position would seem to place Feyerabend much closer to his traditional logical empiricist opponents than anyone, including Feyerabend, would have thought imaginable. But I don't think that this is the conclusion we should draw. Recall that Feyerabend applauded Aristotle for what he thought was an admirable synthesis of theoretical and empirical tendencies. If Feyerabend were to deny reality to all abstract/theoretical thought, then it would seem that Feyerabend would be making the same mistake he criticised the 'Rationalists' for making when they denied reality to appearances: he would simply be inverting the argument and the same criticism of question-begging could be levelled at the inverted position. Consequently, the position Feyerabend ultimately defends is one in which "Nature herself can be approached in many ways ... and responds accordingly" (FTR, p. 76.) and that "we either call quarks and Gods equally real, but tied to different circumstances, or we altogether cease talking about the 'reality' of things and we use more complex ordering schemes instead." (FTR, p. 89.)<sup>21</sup> Feyerabend goes on to say that

This world is not a static entity populated by thinking ants who, crawling all over its crevices, gradually discover its features without affecting them in any way. It is a dynamic and multifaceted entity which affects and reflects the activity of its explorers. It was once a world full of gods; it then became a drab material world and it will, hopefully, change further into a more peaceful world where matter and life, thought and feelings, innovation and tradition collaborate for the benefit of all. (FTR, p. 89.)

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<sup>21</sup> Cf. also FTR, p. 125.

All this seems to be returning Feyerabend to the radical relativism that he fell victim to in SFS, but there is a subtle difference here: "Note that such an interpretation does not deny the effectiveness of science as a provider of technologies and basic myths; it only denies that scientific objects *and they alone* are 'real'" (FTR, p. 126.) That is, scientific objects are real in their own domain, but in different domains, other, non-scientific, theories and ideas, may be just as real. In order to stem the plausible, though false, assumption that Feyerabend is a radical idealist, contending that our thoughts literally create and shape the world, I will now go off on what seems to be a tangent, but which is essential for understanding Feyerabend's position, and dispelling lingering doubts of madness.

#### **4.4 Quantum Physics and Complementarity.**

After initially being critical of what has been called the Copenhagen Interpretation of quantum physics, Feyerabend came to believe that the Copenhagen Interpretation, or more specifically, Bohr's interpretation of quantum physics, was a physically, mathematically and philosophically coherent account of the micro-physical domain.<sup>22</sup> Though Feyerabend does not present complementarity as a central tenet of his philosophy, Bohr's position occurs again and again as an illustrative example of the points that Feyerabend argues for.<sup>23</sup> It is my contention that a full understanding of Feyerabend's conception of quantum physics is necessary in order to focus Feyerabend's philosophy. Quantum physics and complementarity appears to loom large in the background of much of Feyerabend's philosophy.

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<sup>22</sup> Feyerabend's defense of Bohr's interpretation of quantum physics can be found in "On a Recent Critique of Complementarity: Parts I and II" in *Philosophy of Science* 1968-1969. Reprinted as "Bohr's Philosophy of Science" in Feyerabend's *Phil. Papers Vol. 1*. Feyerabend's initial criticisms of the Copenhagen interpretation of quantum physics mainly revolved around the contention that the Copenhagen interpretation was beyond question; that it was turning into a dogmatic assertion rather than a critically assessed factual/empirical theory. The dangers of such a dogmatism remained foremost in Feyerabend's thought notwithstanding Feyerabend's re-evaluation of Bohr's interpretation of quantum physics.

<sup>23</sup> For example, in the passage from which the lengthy quote from SFS, in which I adduced that Feyerabend, in that book, was supporting a radical, incoherent relativism, there is included a passage, which I left out, where Feyerabend supports his claim with an analogy: "since Bohr's analysis of the case of Einstein, Podolsky and Rosen we know that there are changes which are not the results of a causal interaction between object and observer but of a change of the very conditions that permit us to speak of objects, situations, events. We appeal to changes of the latter kind when saying that a change of universal principles brings about a change of the entire world." p. 70.

It has been said that Niels Bohr's principle of complementarity provides a universal epistemological lesson. Bohr himself states that complementarity should be seen as a contribution to the "general philosophical clarification of the presuppositions underlying human knowledge."<sup>24</sup> To fully understand the import of complementarity, it is essential to juxtapose complementarity with the notions of reality, objectivity and knowledge, underlying the classical physical framework.

Within the classical framework the physical world is considered to have its attributes and existence independently of human observers. We come to know this world via observation and measurement, the results of which are directly predicable of the independent reality. Any effects which observation and measurement may produce are calculable and controllable, so that the theories thus arrived at are ideally a 'mirror' image of the world, where "every relevant element of reality and every relevant physical attribute of these elements has a corresponding counterpart in the theory"<sup>25</sup> Moreover, not only does the theory contain counterparts of all relevant elements of reality, it also exhaustively characterises these elements for all attributes and for all temporal instants.

This last point is crucial, for it is these two conceptions: completeness of description and spatio-temporal continuity, which quantum physics has called into question. The postulation of continuity is especially important in that it is a notion which holds together the entire classical framework and underpins the classical conception of objectivity: if a physical system always has determinate attributes, then, in an interaction of observation or measurement, it is always theoretically possible to distinguish between the observed object and the observing instrument. This is a necessary condition of classical objectivity: we must be able to distinguish between what the object is independently of observation, and what we contribute to the situation when observing an object. If this was not the case, then it is possible that physical systems have determinate attributes only when we interact with them, so that, in a sense, the physical systems

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<sup>24</sup> N. Bohr, "Causality and Complementarity", *Philosophy of Science*, 1937. p. 290. Quoted in H. Folse, *The Philosophy of Niels Bohr: The Framework of Complementarity*, p. 12.

<sup>25</sup> C.A. Hooker, "The Nature of Quantum Mechanical Reality: Einstein Versus Bohr", in R.G. Colodny (ed) *Paradigms and Paradoxes*, p. 70.

would not be independent of human observations: we would 'create' the attributes by observing. This possible situation is avoided by postulating continuity of state: if all physical systems are in a well defined determinate state at all times, then we can say that the physical systems possess the attributes which we ascribe to them, whether we are interacting with the physical system or not.

Thus, classical objectivity relies upon two postulates: that there is an unambiguous distinction between subject and object, and that physical systems possess all their attributes at all times. Bohr denies the validity and applicability of these two postulates. He does so because of his acceptance of the quantum of action in atomic processes, thereby denying the classical ideal of continuity of state: atomic processes are discontinuous processes. This discontinuity of atomic processes implies that an atomic system does not have determinate values of all of its attributes at all times. As we saw previously, the postulation of continuity of state was the reason we could separate subject from object in an interaction: objects are considered to be in a determinate state at all times and are therefore theoretically distinguishable. However, in an interaction of observation or measurement in the atomic domain, it is impossible, because of the quantum of action, to draw a sharp distinction between the object and the observing or measuring instrument. In Bohr's words,

The logical comprehension of hitherto unsuspected fundamental regularities governing atomic phenomena has demanded the recognition that no sharp separation can be made between an independent behaviour of the objects and their interaction with the measuring instruments which define the reference frame.<sup>26</sup>

Consequently, the interaction must be considered as a whole, as an indivisible individual process.

For Bohr, and for Feyerabend, the ultimate source of all of the information we have concerning the nature of the world is phenomenal, sensory experience. This sensory experience, at least in the macro-world of everyday objects, gives us information whereby we can distinguish objects from each other and from ourselves. When we come

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<sup>26</sup> N. Bohr, *Atomic Physics and Human Knowledge*, p. 52.

to the investigation of the atomic domain our sensory information is of an experimental arrangement, which includes both observed object and observing instrument. Consequently, the object from which we derive empirical generalisations is the entire macro-experimental arrangement. This demand of Bohr's to consider the experimental arrangement as a whole produces the famous consequences of complementarity: the contention that 'wave' and 'particle' pictures and kinematic and dynamic properties of atomic systems are complementary aspects of reality.<sup>27</sup> This is because the "experimental arrangement required to realize one of the two alternatives rules out, renders undefinable, the classical concept appropriate to the description of the other alternative".<sup>28</sup> Both are essential for a classically complete description of atomic phenomena, but they are not simultaneously applicable.

We can see here another instance of the divergence from the classical ideal, that is, not all the attributes classically associated with objects are determinable at the one time. There are, in fact, two mutually exclusive classes of concepts which cannot be determined at the same time. If you are a classical realist, then this is an especially difficult concept to accept, surely, if atomic reality is particulate, then the particles must simultaneously possess, for example, both position and momentum. The fact that quantum physics does not allow simultaneous determination of all physical attributes can only be an inadequacy of the theory, not a reflection of the objective situation. On this view quantum physics expresses our ignorance of physical factors and our inability to determine what is in fact determinate.

Bohr's reply is to say that the properties are not determinate at all, in fact, "we are here dealing with the mutually exclusive conditions for the unambiguous use of the very concepts of space and time on the one hand, and of dynamical conservation laws on the other",<sup>29</sup> such that if, for example, we wish to precisely determine the position of a sub-atomic particle, then, in doing so, we are excluding the conditions necessary for the

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<sup>27</sup> For a discussion of the difference between these two complementary relationships, see D. Murdoch, *Niels Bohr's Philosophy of Physics*. ch's. 4 and 5.

<sup>28</sup> Hooker, "The Nature of Quantum Mechanical Reality" p. 145.

<sup>29</sup> Bohr, *Atomic Physics and Human Knowledge*, pp. 72-3.

determination of the particles precise momentum: the concept of momentum is just not applicable to the situation. Feyerabend uses the example of the Moh's scale of hardness:

the concept of hardness as defined by the Moh's scale ceases to be applicable when the temperatures become too high and the same is true of surface tension at low temperatures. There is no need to continue this list which shows quite clearly the existence of nonprobabilistic concepts which characterize experimental setups, are applicable only in certain physical conditions, and change abruptly when the conditions change. Bohr assumes that position, momentum, etc. are concepts of exactly this kind and he specifies the conditions under which they are applicable, and to what degree of precision.<sup>30</sup>

Feyerabend goes on say that "A block of ice may have a certain hardness on the Moh's scale; but when it melts ... its hardness simply ceases to exist".<sup>31</sup> Similarly, once position is determined precisely, the physical situation and conditions preclude the determination of momentum, *because it does not exist*.

At this point it must be remembered that although much of Bohr's philosophy of complementarity is concerned with the conditions for the applicability of concepts and is, therefore, very much a conceptual issue, this does not mean that complementarity is derived from an *a priori* use of a philosophical theory, and that it has no physical underpinning.<sup>32</sup> This could not be further from the truth: the whole structure of complementarity is based upon the physical discovery of the quantum of action, it is not therefore a purely conceptual theory. Rather, complementarity "is a discovery of the *factual absence* of the conditions required for the joint applicability of certain classical concepts".<sup>33</sup> Not only that, but, as Feyerabend states, "the hypothesis of the relational character of all dynamical states is a physical hypothesis as it is an attempt to account for a long series of interesting conjectures and refutations."<sup>34</sup> That is, many classical realist

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<sup>30</sup> "On a Recent Critique of Complementarity. Part II" pp. 93-4.

<sup>31</sup> *Ibid.*, p. 95.

<sup>32</sup> Some philosophers have believed that the philosophy of complementarity incorporates a purely philosophical theory which has illegitimately risen to prominence and obscured the true understanding of quantum physics. See, for example, Feyerabend's discussion of Popper's views in his "On a Recent Critique of Complementarity", and M. Bunge, "Strife About Complementarity: Pts. I and II", *British Journal for the Philosophy of Science*, 6 (1955-1956) for a particularly vitriolic attack upon complementarity.

<sup>33</sup> Hooker, "On the nature of Quantum Mechanical Reality" p. 137.

<sup>34</sup> "On a Recent Critique of Complementarity. Part II" p. 93.

interpretations were proposed but their adequacy was refuted by experiment. We only need to look at the idea that atomic processes are said to exhibit characteristics of both waves and particles. One thing cannot be both a wave, infinitely extended in space, and a discrete, discontinuous particle.<sup>35</sup> Classical realist interpretations have never been able to satisfactorily account for these empirically supported assertions.

Thus, we must come to the conclusion that, strictly speaking, the classical view of the world is false. The realistic ideals of classical physics, derived from the world and designed to 'mirror' reality, do not, in fact, represent the world, and do not, in fact, 'mirror' reality. Bohr contends that "all knowledge presents itself within a conceptual framework adapted to account for previous experience and that any such frame may prove too narrow to comprehend new experience".<sup>36</sup> Consequently, the concepts which we utilise and interpret the world with were adapted to a particular circumscribed part of the world. The part of the world which the classical physical concepts of reality were meant to explain was that which was centred upon the human perceptual range of experience. We might be inclined to conclude that perception is thus inherently misleading and provides us with distorted, false, pictures of the world. But this is not what is asserted by Bohr and Feyerabend. The quantum physical picture of the world, incorporating complementarity, is as equally tied to sense experience as is classical physics. The sense experience caused by the world is the only 'reality' we have, but if we push and prod the world into different circumstances, we will then receive sensory information from the world which we would not normally receive. The epistemological lesson which Bohr wanted to teach revolves around the arbitrariness of the distinction between subject and object. If the distinction is arbitrary, then there really is no distinction, and the "notion of an ultimate subject as well as conceptions like realism and idealism find no place in objective description."<sup>37</sup> If we cannot unarbitrarily make a distinction between subject and object, then questions like, 'is that the way the world

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<sup>35</sup> For a very good discussion of this point, see C.A. Hooker, "The Metaphysics of Science: Atoms Versus Plena" *International Logic Review*, 5, (1974), pp. 111-46.

<sup>36</sup> Bohr, *Atomic Physics and Human Knowledge*, p. 67.

<sup>37</sup> *Ibid.*, p. 79.

really is?' and 'is the world a construction of my ideas?' are not just unanswerable, but meaningless. That which is objective becomes that which we can agree upon; moreover, that which we can agree upon is an objective *description* based upon sensory experience: questions of essential natures are left behind.

#### **4.5 Quantum Physics and the Theoretical/Empirical Dichotomy of Traditions.**

If we now step back from quantum physics and examine the implications of Feyerabend's interpretation of quantum physics for his distinction between theoretical/abstract and empirical traditions, then we will find that Feyerabend's ideas are entirely coherent and give a very interesting and profound account of the inextricable relationship between realism and relativism.

The fact that in our investigations of atomic phenomena we were inevitably drawn to the conclusion that the results of experiments are results pertaining to the entire experimental arrangement, rather than to some putative independent object, gives us reason enough to attempt to generalise these findings to other areas of investigation. Firstly, let us see what implications are inherent in sciences other than physics.

Imagine a biologist studying the transport and incidence of growth hormone in a particular species of bean.<sup>38</sup> The particular bean species is selected from among a variety of alternatives, for a variety of reasons: speed of growth, availability, simplicity of structure, possible economic applicability. The bean seed of the species selected is then placed into a pot, one seed per pot. The soil in every pot is carefully selected and mixed so that each pot, within specified limits, have the same amount and constitution of soil. The pots are then placed into a regulated, weather and pest controlled environment, such that they are all exposed to the same range of specified temperatures and sunlight. Watering of the growing bean plants is done in a carefully monitored way so that each plant receives the same amount of moisture. After a specified period of time the bean plants are removed and analysed as to the transport and incidence of growth hormone.

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<sup>38</sup> This experiment is a crude example of an actual biological experiment told to me in personal communication.



This process is repeated many times to ensure repeatability, and to even out any anomalies that may have escaped the diligence of the biologist. In this particular situation the analysis of successive batches of bean plants is performed at different times in the life-cycle to produce a temporal picture of the incidence and transport of growth hormone.

I would conjecture that this situation has, in many respects, many parallels to the situation found in quantum physics. In conducting this experiment we cannot interpret the results obtained as presenting the true, real, essential nature of the incidence and transport of growth hormone in beans. The results obtained may be true and real, but they are true and real results *of the entire experimental arrangement*: the results are *relative* to the set of circumstances in which the experiment was conducted. Slightly different environmental circumstances will elicit different results, and these results will also be true and real, relative to the slightly different experimental arrangement. The list of possible experimental arrangements is, for all intents and purposes, limitless, and each result obtainable equally true and equally real; as long as the factors which are controlled, and those that are not, are known. We will never arrive at any fundamental knowledge of the incidence and transport of growth hormone in the bean, or any other object of scientific investigation: every single bean plant in the world is exposed to a different causal, interactive environment. Consequently, the incidence and transport of growth hormone will be different in every single bean plant. We cannot have a completely controlled experiment which gives us unadulterated knowledge of the objects as they are in themselves: the environment, or experimental arrangement, is a necessary concomitant of any experiment and the knowledge derived is always a restricted knowledge which must be understood in relation to the entire experimental arrangement.<sup>39</sup>

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<sup>39</sup> Thus, Feyerabend would consider the billions of dollars spent on particle accelerators, with the justification that such expenditure is necessary because the fundamental constituents of matter are thereby discovered, as a fundamentally misguided venture. Granted, the results obtained may be true and real, but the results obtained are definitely *not* indicative of any fundamental reality: the results obtained are relative to the experimental arrangement, which, in this situation, are huge particle accelerators. Equally fundamental knowledge can be obtained by relatively cheap experimental apparatus. This leads us to the question, 'what do we want science to achieve?' the answer is relative to our values and interests. The money spent upon particle accelerators would be better off spent upon pressing problems of survival interest.

These ideas are brought together by Feyerabend in a metaphysical and epistemological vision characterised by the relinquishment of all traditional dichotomies: the dichotomies of realism/idealism, relativism/absolutism, subject/object, are all seen as imaginary, unattainable, polar limits, where reality is always found between the two extremes. If we look at Feyerabend's metaphysics, we can characterise it as either a plentiful realism, or as a moderate idealism:

Scientists, being equipped with a complex organism and embedded in constantly changing physical and social surroundings, used ideas and actions ... to *manufacture*, first, metaphysical atoms, then, crude physical atoms, and, finally, complex systems of elementary particles out of a material that did not contain these elements but could be shaped into them. Scientists, according to this account, are sculptors of reality - but sculptors in a special sense. They not merely *act causally* upon the world ... they also *create semantic conditions* engendering strong inferences from known effects to novel projections and, conversely, from the projections to testable effects.<sup>40</sup>

There is here no question as to the reality of elementary particles: given the experimental arrangement, as specified in sub-atomic experiments, and given the conceptual-semantic system of quantum physics, elementary particles can be justifiably posited. The world is manipulated so as to give specific responses. A better example is provided by the creation of super-conductors: super-conductors do not exist naturally, yet super-conductors can be created under highly specific and difficult to realise conditions. Indeed, the modern scientific conception of the genesis and evolution of the universe can be seen to give support to Feyerabend's ideas: various natural phenomena and natural, physical laws, only emerge once highly specific circumstances are realised. Stars, planets, life, consciousness, all form only under specific environmental boundary conditions. The questions Feyerabend asks are, what other phenomena and laws can we find under other differing circumstances and conditions? And, is there a limit to the range of possible unique circumstances and conditions?

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<sup>40</sup> Feyerabend, "Realism and the Historicity of Knowledge", in *Journal of Philosophy*, 86, 1989, p. 404

For some philosophers the ideas above may be highly unpalatable, in that too much scope is given to the creative capacities of humans to create the world: the position is too idealistic. Feyerabend responds by saying that

I do not assert that any combined causal-semantic action will lead to a well-articulated and livable world. The material humans ... face must be approached in the right way. It *offers resistance*; some constructions ... find no point of attack in it and simply collapse. On the other hand, *this material is more pliable than is commonly assumed*. Molding it in one way ... we get elementary particles; proceeding in another, we get a nature that is alive and full of gods.<sup>41</sup>

That is, the position Feyerabend defends is not completely idealistic: the world does indeed have a nature and unequivocally says 'no' to some attempts at characterising it. The best way to understand this position is to consider the history of science as consisting of a treasure of examples of highly successful concepts and theories which proved to be, nevertheless, unsuccessful as characterisations of the nature of the universe. Experimentation and manipulation of the world according to the assumptions of Newtonian natural science, for example, received highly positive responses: the world could be conceived in a Newtonian manner and empirically validated. Nevertheless, Newtonian science was ultimately proved to be deficient as a characterisation of physical reality.<sup>42</sup>

There may yet be grave reservations about the above quote, expressed by scientifically-minded moderns, concerning the contention that reality may be explicable in terms of gods. Surely, the objection runs, science has progressed to such an extent, giving us reliable and unequivocal knowledge of the independent world in which we live, such that metaphysical suppositions like those of the Greek pantheon cannot now be

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<sup>41</sup> *Ibid.*, p. 405.

<sup>42</sup> Feyerabend's position has many similarities to the position of I. Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, esp. pp. 220-32, where Hacking quite plausibly argues that scientists create phenomena. For example, Hacking contends that "Hall's effect did not exist until, with great ingenuity, he had discovered how to isolate, purify it, create it in the laboratory." p. 226. Hacking does not then infer any idealistic conclusions from this situation: "On the contrary, the creation of phenomena more strongly favours a hard-headed scientific realism". p. 220 The created phenomena are no less real for being created: they simply show how matter acts in particular circumstances. Given the sort of position we find Feyerabend defending, it is interesting to conjecture that Feyerabend's philosophy is, in effect, a process philosophy.

supported or entertained by rational people: the malleability of the world has given way in the face of scientific success. Feyerabend responds to this with five rejoinders. Firstly, the fact that science is successful does not preclude the proposal of alternatives, that is, proliferation of theories is always possible and of utmost value. Secondly, the scientific world-view is not a homogenous, seamless, and systematically interrelated body of knowledge: scientific entities and domains are multitudinous and have not been exhaustively interconnected, or reduced to a set of fundamental physical laws. Thirdly, on the theoretical level, there has been no consilience of the disparate fundamental physical laws that do exist: there has been no synthesis of the world of the very small with the world of the very large. Fourthly, as I have shown, Feyerabend believes that the Bohrian approach to the fundamentals of quantum physics supports his conjectures: given the relinquishment of the subject/object dichotomy, and the realisation that scientific results are only predicable of the entire experimental arrangement, knowledge can be seen as fundamentally contextual and situational specific; therefore, given different conceptual-semantic schemes, and correlative specific investigatory frameworks, different systems of knowledge can find valid empirical support. Fifthly, Feyerabend argues that the specific success of science, and the means that have been designed to achieve that success, reflect the nature of the subject-matter chosen for investigation: the methods pertaining to controlled experiments certainly provide much valuable knowledge; however, the types of subject-matter that are amenable to such a treatment only constitute a limited, circumscribed, portion of reality. Feyerabend contends that the entities and processes postulated in other, non-scientific, metaphysical systems are not necessarily accessible or amenable to the procedures of experimental science; and he further contends that this should not constitute a holistic, dismissive, criticism of these systems of thought. As Feyerabend states, "if Aphrodite exists, and if she has the properties and idiosyncrasies ascribed to her, then she certainly will not sit still for something as silly and demeaning as a test of reproducible effects."<sup>43</sup> What needs to be seen is that suppositions, such as that of the Homeric Greek gods, bring to the

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<sup>43</sup> "Realism ...", p. 398. For the five points above generally, see pp. 400-3.

world their own conceptual-causal-semantic systems, which, by way of their unique 'experimental arrangements', often lead to empirical validation of an extensive character. If we accept the system, we then highlight those aspects of the world sympathetic to explication by our chosen system, and then engage in *creating* further conditions which will provide us with the best possible feedback.<sup>44</sup>

#### 4.6 Conclusion.

If true and real knowledge is as plentiful as Feyerabend believes it is, then dogmatic desires to push one system of thought as the true system of thought is dangerous and unjustified. Any system of abstract thought cannot encompass all particular facts of empirical experience. It is for this reason that Feyerabend supports proliferation of ideas, systems and ways of life, as proposed by J.S.Mill. "According to Mill a plurality of views is ... needed *in the sciences*" for four reasons (i) "a view one may have reason to reject may still be true" that is, we are fallible. (ii) Such a view usually contains some truth, and, since received views rarely contain the whole truth, "it is only by the collision of adverse opinions that the remainder of the truth has any chance of being supplied". (iii) An uncontested view, even if wholly true, will be held in the manner of a prejudice without an understanding of the rational grounds of its acceptance. And (iv) "one will not even understand its meaning ... unless a contrast with other opinions shows wherein this meaning consists." Feyerabend adds a fifth reason, allied to reason two:

that decisive evidence against an opinion can often be articulated and found only with the help of an alternative. To forbid the use of alternatives until contrary evidence turns up while still demanding that theories be confronted with facts, therefore, means putting the cart before the horse. And using 'science' to denigrate and perhaps even to eliminate all

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<sup>44</sup> Consider this passage from Feyerabend, in his "Problems of Empiricism" n.8. p. 220. "We are all aware of thoughts, impulses, feelings that run counter to our conscious intentions. Usually we disregard them, for they do not occur in a very coherent fashion ... It is quite different with a person believing in the existence of demons. He would perceive a meaningful pattern in such occurrences; they would appear to him as the result of the attempts of some demon to corrupt him. Considering the astounding plasticity of the human mind, this belief *could even bring about a more regular display of such alien occurrences* ... Expectation, fantasy, fear, and mental illnesses flowing from them ... would do the rest ... Demons would have become directly observable. *And this has actually happened.*"

alternatives means using a well deserved reputation to sustain a dogmatism contrary to the spirit of those who earned it.<sup>45</sup>

Feyerabend contends that proliferation can be evinced when we come to realise that "[the] complexity of real life (which is a life among particulars) ... keeps our minds flexible and prevents them from being overly impressed by similarities and appearances of lawfulness" (FTR, p. 35.) That is, one of the ways to achieve proliferation is to draw back from abstractions and general statements, thus to see the multifariousness of the world anew. This is the situation which Feyerabend is trying to describe when he talks about empirical traditions: empirical traditions emphasise the particularity and multiplicity of the world. In empirical traditions we have local knowledge, adapted to local circumstances. Local knowledge will be true and real knowledge, but it cannot be applied to all circumstances everywhere in time and space.

On the other hand, the controlled experiment is an excellent example of the tendency of thought associated with theoretical/abstract traditions. It is believed that in order to know what something really is, we must separate all extraneous factors: we must abstract the real from the multiplicity of particulars. Feyerabend has no in-principle objection to this approach: it is a way to see things in new and different ways, thereby increasing the possibilities of knowledge. But the theoretical/abstract approach provides just that, an increase in the possibilities of knowledge, not a replacement of knowledge. Thus, when Socrates protagonists gave many responses to questions such as 'What is virtue?' they were expressing the idea that virtue can be different things in different circumstances. The Platonic Socrates search for the one true meaning, will, following the above analysis, only result in one more answer to the question, applicable in certain circumstances, not applicable in others. If the search is continued relentlessly, the resulting ideas run the risk of becoming empty truisms: true of all circumstances, only because they are true of none.<sup>46</sup>

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<sup>45</sup> This quote, and the previous quotes in this paragraph, all at FTR, p. 34.

<sup>46</sup> With these ideas in mind, we can now see why there has often been obstinate resistance to the application of the methods of natural science, conceived of as abstraction and control, to the social and human sciences: the idea just doesn't make sense. We can agree with Feyerabend when he says that "in the human sciences it would not only be unwise but also immoral and tyrannical to 'annihilate' individual points of view because they do not fit into general frameworks of 'increasing explanatory

These ideas of proliferation are not intended to be confined to science, they are intended as a completely general attitude to life on all levels. Consequently, cultural variety follows from these ideas: we need to be able to juxtapose our conceptions of, for example, justice, with other conceptions of justice, so as to see the underlying abstractions built into our system. Feyerabend believes that such a procedure will lead us to a broader, more comprehensive, picture of reality. Moreover, such a proliferation is not consistent with a radical relativism. Feyerabend denies that "whatever one says is valid only 'within a certain system'" .<sup>47</sup> Firstly, because no system is unambiguous. And secondly, and more importantly, because one can learn new ways of life; "For if one can, then one system is potentially all systems and the restriction 'relative to system A' *while useful for special purposes*, loses its power as a *general characterisation of knowledge*."<sup>48</sup> The learning process leads people away from particular systems which, for example, they may have been brought up with. As long as people are exposed to different traditions they will inevitably absorb aspects of traditions other than the ones that they have been brought up with. As long as a society is not a completely closed society, if there are such things, then the transformation of traditions and ideas is happening with every new generation: "traditions, by their very nature try to reach beyond their boundaries".<sup>49</sup> In Feyerabend's system, the possibility that we can gather greater and greater amounts of knowledge is never denied: the more we see how matter, animals, people, and so on, act in varying circumstances, the more knowledge we will attain. Consequently, we can understand why Feyerabend urges that the proliferation of

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power" (FTR, p. 35.) but we can also go much further in our criticism. It is of the nature of history, social analysis, or psychology, that the attempt to separate out certain factors, thereby supposedly coming to a more fundamental knowledge of the actors, movements and so forth thus separated, *does not give us knowledge of the actual circumstances that obtain*. History, for example, is the sum total of all the various movements and tendencies involved at a particular time, and it is the interactions between them which is of the essence of the situation. True knowledge in such a situation is, ideally, the sum total of influences and interactions: neglect any factors and the history is not complete. If, contrary to possibility, all variables could be 'controlled', in, for example, an analysis of a social movement, then we would not find the true nature of that social movement. Rather, the social movement would vanish: its very nature is relational.

<sup>47</sup> "Third Dialogue", in *Three Dialogues on Knowledge*, p. 151. See also "Concluding Unphilosophical Conversation" p. 503.

<sup>48</sup> "Third Dialogue" p. 152.

<sup>49</sup> "Concluding Unphilosophical Conversation", pp. 508-9.

theories, methods, and traditions is essential for intellectual, scientific, personal, and moral development; and why it is sometimes necessary to break even the most time tested methods and rules to ensure progress. Feyerabend wants free thinkers, people exposed to many traditions, many viewpoints, so that the individual can make a relatively free, informed and unbiased choice as to their life goals and plans. He quotes with approval Kant's definition of enlightenment: "enlightenment is man's release from his self-incurred immaturity. Immaturity is man's inability to make use of his understanding without direction from another."<sup>50</sup>

How exactly all this is achieved, how Feyerabend can delineate between crackpot theories and interesting theories, between theories and ideas that aren't true, even relative to special circumstances, will have to wait until the following chapters. At the moment we can conclude that Feyerabend's position, though a plentiful realism, is still a realism, and that it seems to successfully incorporate the most striking features of relativistic thought, whilst remaining immune to the charges of incoherence. I finish with a final word from Feyerabend:

In A.M and S.F.S I asserted that science was one form of knowledge among many. That can mean at least two things. One: there is a reality which encourages many approaches, science among them. Two: knowledge (truth) are relative notions. In S.F.S I occasionally conflated both versions, in Farewell to Reason I used the first and rejected the second. This I still do.<sup>51</sup>

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<sup>50</sup> Kant., "What is Enlightenment?" p. 11. quoted in Feyerabend, FTR, p. 12.

<sup>51</sup> "Concluding Unphilosophical Conversation", p. 519.



## 5 Feyerabend and Kuhn On Epistemic Values.

### 5.1 Introduction.

John Preston has recently claimed that to understand Feyerabend's philosophy we must place him in the context of a, possibly idiosyncratic, but nonetheless identifiable Popperian framework. According to this account the Feyerabend of the fifties and sixties is ultimately Popperian; the Feyerabend of the seventies and after, with his 'anarchism' and 'relativism', is understandable in terms of a disappointed Popperian.<sup>1</sup>

I want to say that this claim of Preston's, though partially true, is ultimately false. I don't think it can be denied that the early Feyerabend was deeply influenced by Popper: that much of Preston's interpretation is true. However, the further claim concerning the later Feyerabend, that he was a disappointed Popperian, and that this disappointment led him to eschew the possibility of rationality and to embrace relativism and literal anarchism, does not stand up to analysis. Specifically, I will argue that Feyerabend, in his later philosophy, relentlessly pursues some Popperian theses, thereby highlighting internal inconsistencies in Popper's philosophy.

As I will show in the section to follow, Feyerabend's philosophy should be understood in relation to Popper's assertion that methodological norms and standards are ultimately *decisions* to conduct inquiry in a particular manner. These decisions are arrived at via the normative/ethical desideratum of 'achieving the best', or aiming at 'the good'. Feyerabend follows this program of understanding methodology in an ethical/axiological sense, and arrived at conclusions which ultimately indicted Popper's philosophy, despite its rhetoric of openness, as defending a closed, authoritarian, system of value-judgments.<sup>2</sup>

Despite Feyerabend's implicit support for what can be called values-based rationality, Feyerabend never provided a systematic, clear, and unambiguous account of

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<sup>1</sup> J. Preston, *Feyerabend: Philosophy, Science and Society*, *passim*, Cf. also the very similar account given in J. G. McEvoy, "A 'Revolutionary' Philosophy of Science ..."

<sup>2</sup> Preston emphasises the importance of ethical/normative considerations in understanding Feyerabend's philosophy, see *Feyerabend* p. 16. and pp. 20-2. But Preston nowhere links this with Feyerabend's later philosophy; a linkage which is, moreover, of utmost interpretative importance.

such a position. Consequently, in section 5.3 I discuss the views of Kuhn concerning values in science, in order to move towards a clearer conception of what a values-based rationality could be.

## **5.2 Feysabend and Popper on the 'Good' Science.**

### **5.2.1 Popper's Ethical Normativity.**

Karl Popper's philosophy of science crucially revolves around the contention that there must be a criterion of demarcation separating science from pseudo-science, non-science and metaphysics. Popper's answer to the question of demarcation: that "the *falsifiability* of a system is to be taken as a criterion of demarcation", (LSD, p. 40.) or, in other words, that "it must be possible for an empirical scientific system to be refuted by experience", (LSD, p. 41.)<sup>3</sup> contains, along with his anti-foundationalism and fallibilism, the basis of Popper's entire philosophy. Other specific aspects of Popper's position, for example, the exhortation to avoid *ad hoc* hypotheses, to be parsimonious in the proposal of auxiliary hypotheses, and to propose theories of ever greater generality and universality, all follow from Popper's falsifiability criterion of demarcation.

If we now ask of Popper, what reasons do you have for supporting your demarcation criterion? We get the very honest and straightforward reply that

my criterion of demarcation will ... have to be regarded as a *proposal for an agreement or convention*. As to the suitability of any such conventions opinions differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common. The choice of that purpose must, of course, be ultimately a matter of decision, going beyond rational argument. (LSD, p. 37.)

Popper goes on to say that "I freely admit that in arriving at my proposals I have been guided, in the last analysis, by value judgments and predilections." (LSD, p. 38.) If we keep in mind the idea that Popper grounds his more specific methodological rules: avoid *ad hoc* hypotheses, be parsimonious in the proposal of auxiliary hypotheses, propose

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<sup>3</sup> Italicised in original.

theories of ever greater generality, and so on, upon their ability to enhance the aim of science, as given in his demarcation criterion, then we come to the conclusion that Popper's entire system of philosophy is based upon a conventional decision. What reason is there to support such a position?

Popper has explicitly acknowledged that we can conceive of science, and do science, according to the standards of conventionalism. These standards Popper characterises as "the principle of selecting the simplest system - the simplest system of implicit definitions; which of course means in practice the 'classical' system of the day." (LSD, p. 81.) Popper goes on to say that "I regard conventionalism as a system which is self-contained and defensible. Attempts to detect inconsistencies in it are not likely to succeed." (LSD, p. 80.) If both conventionalism and falsificationism, and presumably other conceptions of science, are possible ways of conducting the business of science, and the adoption of any one of these incompatible alternatives is simply a choice, a decision to follow the methodological rules compatible with the preferred aim of science, then the question of which conception of science to choose among the alternatives requires some sort of reasoned answer.

Popper's reply is to say that "My only reason for proposing my criterion of demarcation is that it is fruitful: that a great many points can be clarified and explained with its help." (LSD, p. 55.) Popper goes on to say that "It is only from the consequences of my definition of empirical science, and from the methodological decisions which depend upon this definition, that a scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavours." (LSD, p. 55.) That is, the decision to prefer one way of conceiving of, and doing, science, is based upon the pragmatic, and ultimately ethical consideration, that the science that would result from following Popper's falsificationism will be *better* than the science that would be produced by following the rules of conventionalism, or instrumentalism. According to Popper, though conventionalism is philosophically incontestable, it would result in the retention of the 'classical' theory of the day, despite empirical difficulties: "Whenever the 'classical' system of the day is threatened by the results of new experiments which might be

interpreted as falsifications according to my point of view, the system will appear unshaken to the conventionalist." (LSD, p. 80.) On the other hand, Popper sees his falsificationism as anti-dogmatic, open, and 'better', because of its putative ability to ensure scientific progress through the proposal of ever more general bold conjectures incompatible with their predecessors:

we shall take the greatest interest in the falsifying experiment. We shall hail it as a success, for it has opened up new vistas into a world of new experiences. And we shall hail it even if these new experiences should furnish us with new arguments against our own most recent theories. (LSD, p. 80.)

Popper's argument for falsificationism is not dependent upon refuting alternatives such as conventionalism, or instrumentalism, by showing that they are inconsistent with science, or internally contradictory. And there is no contention that a scientist who was, for example, consciously conventionalist, would not be able to produce good science. Rather, Popper argues that if scientists conducted science according to the standards of falsificationism, then they would, in the long run, produce *better* science than that which would follow from adopting the standards of its rivals: Popper's philosophy is inherently value-driven.

### 5.2.2 Feyerabend's Ethical Normativity.

It can't be denied that Feyerabend's early career can be characterised as following Popper. However, the manner in which Feyerabend appropriated Popper's philosophy should be understood in terms of an acceptance of Popper's ethical normativism, as presented in the previous section. Originally, this was manifested with an acceptance of Popper's more specific doctrines: those pertaining to falsificationism. As we follow Feyerabend through his development, we find that Feyerabend dropped his allegiance to falsificationism, but, I would contend, Feyerabend *never relinquished his adherence to an ethical normativism*: ethical normativism can be predicated of Feyerabend's philosophy throughout his career. Thus, Feyerabend, far from being a disappointed Popperian, was, rather, systematically pursuing the implications of the ethical

normativism found in Popper. This led Feyerabend to indict Popper's specific methodological theories as dogmatic. Strict adherence to Popper's notions *did not necessarily produce the best science*, a desideratum crucial, according to Popper himself, for opting for falsificationism. Consequently, if the norms of falsificationism did not necessarily produce the best science, then it is beholden upon the ethical normativist to search for that norm, or set of norms, which *will* produce the best science. In this situation, refusal to adjust methodological prescriptions in the face of difficulties amounts to dogmatism.

One way of approaching this issue is through what Bartley has called 'comprehensively critical rationalism' (CCR).<sup>4</sup> Bartley sees CCR as the theory of rationality Popper espouses. In this conception of rationality "the rationalist identity might be characterised as that of one who holds *all* his beliefs, including his standards and his basic philosophical position itself, open to criticism". Bartley goes on to say that

The new rationalist identity satisfies its own requirements; without any contradiction or other difficulty the practice of critical argument can be criticized ... a comprehensively critical rationalist, who is not committed to the view that his position is the correct one, could be argued out of rationalism by himself or someone else.<sup>5</sup>

It could be argued that Popper does not support CCR; however, whether Bartley's contention that CCR captures Popper's intentions is correct, does not matter for our purposes. The question raised by Bartley's interpretation of Popper, and embraced by Bartley himself, is the question of the possibility of criticism of Popper's falsificationism. How do we criticise a theory which sees itself *as* criticism?

The early Feyerabend was famous for his espousal of a pluralistic methodology. Though the value of alternatives can be drawn out of Popper's principles, no one emphasised the value of pluralism as much as Feyerabend. Thus, Feyerabend exhorted scientists to work with sets of incompatible general theories, so as to maximise criticism, falsification, and progress: to produce the best science. Moreover, Feyerabend contended

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<sup>4</sup> W.W. Bartley III, "Rationality Versus the Theory of Rationality" in M. Bunge (ed) *The Critical Approach to Science and Philosophy*, pp. 3-31.

<sup>5</sup> Both *Ibid.*, p. 30.

that some potential falsifiers of any one theory could only be found with the help of incompatible alternatives. Once a pluralistic methodology of this sort is in place, it is not hard to make a further step into *methodological* pluralism; especially if we are confronted with the question, as we were in the last paragraph, of the criticism of criticisms. And, indeed, this is the move that Feyerabend makes, and this is what needs to be understood when considering Feyerabend's diatribes against Method, and for epistemological anarchism.

Recall that Popper did not deny, for example, that science could be conducted according to the standards of conventionalism. What Popper denied was that conventionalism will produce the 'best' science. If we look at some of Feyerabend's writings from the sixties, we see a gradual drift away from his initial Popperian position, towards a position critical of Popper, whilst still guided by the idea of the 'best' science.

In his 1964 article "Realism and Instrumentalism: Comments on the Logic of Factual Support,"<sup>6</sup> Feyerabend produces powerful arguments to the effect that local instrumentalism is an unobjectionable move in science. The proposal by Osiander to treat the Copernican hypothesis instrumentalistically enabled the Copernican hypothesis to survive. If the Copernican hypothesis had been treated realistically, then, according to the accepted science of the time, it should have been rejected as utterly falsified. However, treating the Copernican hypothesis as an instrument of prediction saves it from rejection. Similarly, interpreting certain aspects of quantum physics realistically leads to contradictions and absurdities; therefore, quantum physics should be rejected as falsified. However, interpreting those aspects instrumentalistically preserves the impressive empirical power of the theory. In both these cases, according to Feyerabend, there were powerful physical and empirical arguments which militated against realistic interpretations, and for instrumentalistic interpretations. Of course, in this article Feyerabend nevertheless argues that realism is the better option; but this should not blind us to the importance of Feyerabend's arguments for local instrumentalism. In the particular circumstances surrounding the introduction of the Copernican hypothesis, and

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<sup>6</sup> In Bunge, *The Critical Approach*, pp. 280-308.

in the acceptance of the Copenhagen interpretation of quantum physics, the 'best' scientific move to be made may not follow the Popperian model; the 'best' scientific move to be made may be antithetical to the spirit of Popper's falsificationism.

By the time we get to Feyerabend's 1968-69 article, "On a Recent Critique of Complementarity: Pts. I and II", the support for local instrumentalism is not tempered by any vestigial support for realism-as-the-best-no-matter-what. Feyerabend has moved to a position, best exemplified by his later philosophy, wherein the 'best' science, what the best scientific move to make is in any given situation, is not governed by any preconceived notion of the best.

One way of interpreting these conceptions of Feyerabend's is to see the proposed universal norms of science as, instead, defeasible rules-of-thumb: as having legitimate applicability in some circumstances; the applicability being ascertained *in* the situation and determined *by* the situation. Thus, Feyerabend contends that Popper's method of conjecture and refutation can be interpreted in two ways: "as *useful hints* for the scientist [and no more, or] ... as *necessary conditions* of a rational approach".<sup>7</sup> Feyerabend goes on to say that we must "modify the stern rules of falsification ... [and] use them as rules of thumb, or as temporary ingredients of rationality, not as necessary conditions of scientific procedure."<sup>8</sup> In the first sense of useful hint, or rule-of-thumb, Feyerabend finds Popper's conjectures and refutations unobjectionable. However, in the second sense, the sense in which Popper wants his method to be known, Feyerabend finds the idea objectionable and unrealistic. In science, situations requiring decision are highly complex; most often there are conflicting, incompatible courses of action available for consideration by the scientist. In such situations of conflict and divided rational loyalty, the best course of action will only be ascertainable in the particular situation at hand. To say, as Popper does, that we must always take falsifications seriously, and not engage in any conventionalist or instrumentalist thinking, removes from the situation requiring decision options which may be the best thing to do in that particular situation.

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<sup>7</sup> "Historical Background ...", p. 22.

<sup>8</sup> *Ibid.*, p. 24.

However, in one very important sense the example of Popper's conjectures and refutations, and the whole idea of rules-of-thumb, can be a misleading one for a complete interpretation of Feyerabend's thought. Consider this quote:

Separating facts, values and rationality is of course an artifice. Facts are constituted by procedures that contain values, values change under the impact of facts and principles of reasoning assume a certain world order (the law of non-contradiction is absurd in an absurd world.). (FTR, n.6. p. 25.)

If we add to this Feyerabend's idea that world-views, in part, constitute the facts, then the circle of facts, values, rationality and metaphysics is complete. These ideas follow from Feyerabend's conception of the relation between theory and observation: there is no boundary dividing statements into two mutually exclusive categories, the theoretical and the observational. The distinction between the theoretical and the observational is a temporary distinction, made within particular experiential contexts. In these contexts,

Nobody will deny that the sentences of science can be classified into long sentences and short sentences, or that its statements can be classified into those which are intuitively obvious and others which are not. Nobody will deny that such distinctions *can be made*. But nobody will put great weight on them. (AM, p. 168)

This is because the distinction is only one which is applicable within the situation in which the distinction was made: it is not an absolute distinction. What was once theoretical and problematic can become the observational and unproblematic, and vice versa.<sup>9</sup> When a scientist is working with a universal theory, such as Newton's theory, the facts of the world are explicable in terms of the metaphysical world-order implicit and explicit in Newtonian theory. The way that the scientist reasons will assume the existence of the Newtonian world. The values that the scientist employs will be related to possible solutions of problems within the spirit of Newtonian ideals. Subsequent facts may bring

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<sup>9</sup> Feyerabend continues the quote above by saying that nobody will put great weight on them "for they do not now play any decisive role in the business of science". (AM, p. 168, italicised in the original) I do not think that this is correct, even though the distinction between theoretical and observational is not an absolute, but a pragmatic distinction, this is still an important distinction within the context of scientific theorising and experimentation. When scientists said that they observed the centre of the sun via neutrino experiments, the observational was identified with the unproblematic, the theoretical was identified with that which was still problematic and hypothetical. It seems to me that this is the role of the observational and the theoretical, but more of this in the next chapter. For the observation of the centre of the sun, see Shapere, *Reason and the Search for Knowledge*. pp. 215-17. And Ch. 16.



the values of Newtonian science in question, and may even lead to the questioning of the Newtonian world-view itself. In this sort of situation, there is nothing inherently external to the metaphysical, theoretical, factual circumstances within which the scientist works, and from which the values and principles of reasoning are obtained. The idea of rules-of-thumb may give the impression that these rules-of-thumb, though defeasible, and legitimately applicable only according to the particular circumstances of a problematic situation, are nevertheless *external* in their content; that is, their content is not connected with any particular world-view. Thus, falsification, even as a rule-of-thumb, may be considered as a possible guide in any situation requiring rational deliberation. This is not how Feyerabend sees the situation: there is not a grab-bag of assorted rules-of-thumb, which may be applicable to a particular problematic situation. The rules-of-thumb are inherently related to the particular world-view, and the particular corpus of unproblematic background information. The impression that some principles have a status which is genuinely universal, even in a contextual, defeasible manner, is a product of the level of entrenchment of a particular world-view.

The sorts of criteria, rules-of-thumb, that Feyerabend sees as being more deeply entrenched in the scientific world-view, and therefore appearing to be more 'universal', are many and varied:

There are *formal criteria*: a linear theory is preferable to a non linear one ... Or: a 'coherent' theory is preferable to a non coherent one ... A theory using many and daring approximations ... may be less likeable than a theory that uses only a few, and safe approximations. Number of facts predicted may be another criteria. *Nonformal criteria* usually demand conformity with basic theory ... or with metaphysical principles. (SFS, fn.119.)

This list can be extended to encompass more traditional criteria such as simplicity, empirical adequacy, explanatory power, and fruitfulness. But the point to be made about such criteria is that

these 'other methods' ... though reasonable ... are arbitrary, or 'subjective', in the sense that it is very difficult to find wish-independent arguments for their acceptability. Also, these 'other methods' most of the time give conflicting results: a theory may be preferable because it makes numerous

predictions, but the predictions may be based on rather daring approximations. On the other hand a theory may seem attractive because of its coherence but this 'inner harmony' may make it impossible to apply it to results in widely different domains. (SFS, pp. 68-9.)

The wish dependent nature of these criteria is another way of saying that they are ends, or goals, to be attained: they function in a teleological manner and constitute the values of science. The fact that these values provide differing results, which lead to different prospective courses of action, is indicative of the fact that they are only definitively, or exclusively applicable within the confines of a particular problematic situation. As we saw above, once the various values are given specific content, derived from the problematic situation and the substantive theories under consideration, pertinent deliberation can begin and be brought to a tentative solution: a solution involving action as to what is to be done.

Given these ideas, we must object when Stove, for example, contends that Feyerabend "reject[s] the distinction between description and prescription".<sup>10</sup>

Feyerabend was always looking for ways in which the 'best' science could be achieved: Feyerabend's philosophy was always prescriptive and normative, but this normativity became contextualised. Feyerabend states that

we must of course make sure that our prescriptions have a *point of attack* in the historical material, and we must also make sure that their determined application leads to desirable results. We make sure by considering (historical, sociological, physical, psychological, etc.) *tendencies and laws* which tell us what is possible and what is not possible under the given circumstances and thus separate feasible prescriptions from those which are going to lead into dead ends ... progress can be made only if the distinction between the *ought* and the *is* is regarded as a temporary device rather than as a fundamental boundary line. (AM, p. 167)

That is, there are no fundamental, unchanging oughts. The ought/is distinction is temporarily created according to the exigencies of the situation. Once the situation has passed, the same oughts may never be assembled in the same manner again. An example will make Feyerabend's point clearer. Imagine a Newtonian scientist around the turn of the century who is confronted with a problematic situation. In this particular situation the

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<sup>10</sup> Stove, *Popper and After*, p. 4.

scientist will make a distinction between the *is* of the situation and the *ought*. The descriptive 'is', the facts of the situation - that which must be taken into account and treated as the given - will consist of, for example, Newton's three laws of motion and law of gravitation; that body of unproblematic science specifically relevant in the problematic situation at hand, and so on. What the scientist ought to do will be constructed out of the given facts of the situation, in this example, the scientist cannot transgress Newton's laws. And the scientist must take into account the specific nature of the problematic situation. The resultant ought will constitute a possible solution to the problem at hand. If the prescribed course of action led to success, then that result may then become the 'is' of a new problematic situation. If the prescribed course of action did not lead to success, then the situation must be reconsidered, with, at least, some more negative information as to the given, and a *new* 'ought' constructed.<sup>11</sup>

Feyerabend does not reject the distinction between the descriptive and the prescriptive, he does not want to say, 'this is science, this is how it goes, therefore, it is as it should be'. Prescriptive accounts of science are necessary, but for Feyerabend, prescription is something limited and constrained to the actual problematic, historical situation in which a scientist is obliged to make decisions. These decisions are plans for action: they are possible resolutions of the problematic situation. The prescriptivity which Feyerabend objects to is the notion of norms which are explicated independently of any particular problem situation, but which are intended to universally apply, *ceteris paribus*, to all problematic situations. Thus, someone defending a more traditional account of the normativity of science, may accept that in the past there have been cases of good science which have not adhered to the norms their theories of scientific rationality recommended, just as in the future such cases may well occur again. But the defender of traditional accounts of the normativity of science will not take this admission as in any way impugning the ongoing applicability of the norms in question: the norms are, as it were, the default mode of scientific reasoning, exceptional circumstances may

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<sup>11</sup> Lakatos's hypothetical Newtonian history, in his "Falsification ..." pp. 100-1, provides a very good example of the process I have in mind.

engender a temporary relaxing of the norms, but this relaxation is only for the duration of the exceptional circumstances, once these circumstances have passed, the norms again assume pride of place.

It is as evidence against this traditional interpretation that Feyerabend's case studies achieve their greatest importance. That is, the importance of Feyerabend's historical case studies is not simply that they provide counter-examples to prominent 'Rationalist' accounts of science; or that the adoption of the norms of 'Rationalist' philosophies within the historical episodes in question would have retarded the rational progress of science. Though these considerations do raise serious questions, the aspect of greatest importance in Feyerabend's case studies consists in the idea that the examination of the moves made in the debates at the time indicates that they were responses inherently related to the problematic situation present at that time. Careful examination of protagonists normative schemes, between different research groups, and across time, discipline, and theoretical upheaval, provides evidence that the ought structures of science change with research group, time, discipline and theoretical upheaval.

Though we can bring forth these ideas from Feyerabend's philosophy, and say that Feyerabend is proposing that we consider rationality in terms of a contextual, ethical normativity, Feyerabend never systematically explored the implications of such an approach to rationality. For example, the remarks quoted above concerning the many criteria of rational decision-making, which I interpreted as being a presentation of a values-based approach, were given by Feyerabend in a decidedly off-hand manner. There are many statements of Feyerabend's which cry out for a grounding within a values-based conception of rationality, but which are left dangling in obscurity and ambiguity. For example, we saw in the last chapter that Feyerabend believes that abstract, generalised thought has no intrinsic advantage over what he called empirical thought. Thus, the move to work with ideal abstraction and generalisation is a *decision*, which is then objectivised by saying that this procedure leads to reality, whilst practical-empirical concepts are illusory and unreal:

rational standards and arguments supporting them are visible parts of special traditions consisting of clear and explicit principles and an unnoticed and largely unknown but absolutely necessary background of dispositions for action and judgement ... we have further seen that there are other traditions that also lead to judgements though not on the basis of explicit standards and principles. These value judgements have a more 'immediate' character, but they are still evaluations, just like those of the rationalist. (SFS, p. 27.)

That is, the preference for abstract, universalised thought constitutes a value, and, for Feyerabend, "Speaking of *values* is a roundabout way of describing the kind of life one wants to lead or thinks one should lead" (FTR, p. 24.) Feyerabend has also contended that "values affect not only the *application* of knowledge but are essential ingredients of *knowledge itself*"; (FTR, p. 28.) that "important changes of abstract thought are qualitative changes", (FTR, p. 154.) and that "the qualitative elements of the sciences ... [are equivalent to] the fundamental ideas of a certain branch of knowledge" (FTR, p. 156.) We see here that there is a series of identifications and implications: fundamental ideas-qualitative elements-abstract thought-values-the life one thinks one should lead, which are all brought together in a manner in great need of explication and clarification. But the answers to these doubts concerning details will not be found in Feyerabend's writings and, as such, Feyerabend's philosophy is lacking in systematic, detailed explication. Therefore, Feyerabend's philosophy is incomplete. This is not to say that Feyerabend's philosophy should therefore be ignored; rather, the task before us is to take Feyerabend's philosophy, with the many clues scattered throughout it, and try to provide those aspects which would constitute a completion of his philosophy: a completion which is consistent with the ideas thus far presented in chapters 1-4.<sup>12</sup>

In order to do this, I will leave explicit discussion of Feyerabend to introduce and critically discuss the idea of values in rationality: its place, its role, its relation to other aspects of epistemic activity, and so on. There have been, at least, three prominent figures in philosophy who have defended the idea that values are essential for

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<sup>12</sup> This is not to say that Feyerabend would have agreed that what is to follow is consistent with his philosophy, or in the spirit of his philosophy. It sometimes appears that *any* more systematic philosophy would come under Feyerabend's critical gaze: his general sceptical and fallibilist position seems to lead him to abhor any settled theoretical structure, especially if he was in a dadaist mood.

understanding science and rationality in general: Kuhn, Laudan and Dewey. In the remainder of this chapter I will discuss Kuhn and Laudan. In the next chapter I will continue the discussion of values with particular reference to John Dewey.

### **5.3 Kuhn on Paradigms and Values**

#### **5.3.1 The Trouble with Paradigms.**

At this point in the discussion I would like to bring in some aspects of the debate surrounding the philosophy of Thomas Kuhn and his book, *The Structure of Scientific Revolutions* (SSR). Originally, Kuhn proposed that science is characterised by the adoption of a paradigm by a community of scientists. Paradigms are complex structures of cognitive commitment. They are comprised of symbolic generalisations, metaphysical models, and exemplary scientific achievements. Kuhn contends that paradigms structure the conceptual world in which scientists work and give them heuristic guidance as to their current, and future, research programs. Paradigms provide the standards of acceptable scientific activity; they delineate what sorts of objects exist in the world; how scientists are to investigate the nature and behaviour of those objects; and the criteria of whether any one scientific result is successful or unsuccessful. Even more than this, paradigms provide the language of science: they define the meanings of scientific terms.

The ubiquitous nature which Kuhn attributes to paradigms leads him to contend that, to a large extent, scientists who are committed to different paradigms live in different worlds. Kuhn's central argument for this is derived from his rejection of the theory/observation dichotomy. If a neutral observation language is rejected, then the theories which a person is committed to will affect the observations that that person performs. If we then consider the overarching nature and depth of paradigms in structuring the theoretical understanding of the world of scientists, we are led to the conclusion that scientists in different, sufficiently large-scale paradigms, literally *see* a different world, and act as if they lived in different worlds.

According to Kuhn, the fact that scientists live in different worlds makes it difficult to account for paradigm change, or scientific revolution. Members of alternative paradigms which are in conflict cannot directly communicate to each other. Their languages, the meanings of the terms they use, and their theoretical presuppositions differ. They often talk past each other and don't accept the claims made by the other party. The standards of evaluation of the different paradigms may differ markedly. In this situation the ideal means of resolving the conflict would be that of appeal to an impartial arbiter, or objective standard, to which all paradigms could be compared. But the fact that there are no impartial arbiters or objective standards to which appeal can be made is the central claim of Kuhn's philosophy: standards are intra-paradigmatic.

Kuhn's original formulations of how science progresses through scientific revolutions was laden with terms implying that the transition from paradigm to paradigm was ultimately mysterious. Kuhn talked of 'conversion' and 'gestalt-switches': suddenly scientists see a duck, when they had previously seen a rabbit. These formulations led many philosophers to say that Kuhn conceived science as inherently irrational.<sup>13</sup> I think that many of the criticisms were unfair to Kuhn: Kuhn never wanted to deny that science was a rational enterprise, and there are many passages in SSR which attest to this. What Kuhn wanted to say is that *conclusive proof* of one paradigm being better than another paradigm cannot be attained. Reasons can be adduced and powerful arguments put forward for the acceptance of one paradigm over another, but this is never enough to ensure the superiority of the one paradigm over the other. Consequently, the empirical fact that scientists *do* change their commitment to paradigms needs to be explained on grounds other than proof.

However, I don't think it can be denied that Kuhn's original position is inadequate. I think we can grant the fact that there is something other than proof involved in the evaluation of competing paradigms, but Kuhn, at that time, had not arrived at anything which would fill the gap in an appropriate manner. Consequently, if Kuhn is to preserve the insights gained by seeing science as a paradigm governed

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<sup>13</sup> See I. Lakatos and A. Musgrave., *Criticism and the Growth of Knowledge*.

activity, whilst at the same time preserving the idea that science, as a whole, is rational, then he must provide an alternate account of rationality applicable when paradigms are in conflict and a choice between paradigms is required.

### 5.3.2 Kuhn, Values and Rationality.

The way Kuhn attempts to preserve the rationality of scientific decision-making is one of the few, if not the only, alternatives open to someone who eschews 'Rationalist' versions of rationality. Building upon a minor theme in his SSR, Kuhn contends that scientific, or epistemic values, are the relatively objective and independent criteria which provide the stable background against which debates over paradigm commitment can be conducted.<sup>14</sup> Kuhn contends that values

are more widely shared among different communities than either symbolic generalizations or models ... Though they function at all times, their particular importance emerges when the members of a particular community must identify crisis or, later, choose between incompatible ways of practicing their discipline.<sup>15</sup>

Some of the values Kuhn sees as operative in science are: accuracy, puzzle-formulation and solution, consistency, scope, simplicity and fruitfulness. As the quote above indicates, Kuhn sees these values as being stable across paradigms; as some sort of inherited higher-order scientific commitment. However, this is not to say that values are concrete and fixed.

values [such] as accuracy, scope, and fruitfulness are permanent attributes of science. But little knowledge of history is required to suggest that both the application of these values and ... the relative weights attached to them have varied markedly with time and also with the field of application.

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<sup>14</sup> Kuhn's reappraisal of his position concerning epistemic values can be found in the "postscript-1969" to SSR; in "Reflections on my Critics" in Lakatos and Musgrave, *Criticism and the Growth of Knowledge*, pp. 231-78. And in "Objectivity, Value Judgement, and Theory Choice" in T.Kuhn., *The Essential Tension: Selected Studies in Scientific Tradition and Change*, pp. 320-39. The fact that Kuhn always considered epistemic values to be essential to scientific decision-making can be seen in SSR pp. 152-8. And in "The Function of Measurement in Modern Physical Science", in *The Essential Tension*, pp. 178-224. (originally published 1961).

<sup>15</sup> SSR (2nd. ed) p. 184.



Furthermore, many of these variations in value have been associated with particular changes in scientific theory.<sup>16</sup>

This last sentence may seem to obviate the putative stable and independent nature of values. If theory change brings with it value change, then values cannot play a role in the justification of theory choice. We would be back where we started. But the values which Kuhn proposes as being operative in science are relatively stable; for example, Kuhn states that

I know of no case in the development of science which exhibits a loss of quantitative accuracy as a consequence of the transition from an earlier to a later theory ... whatever the price in redefinitions of science, its methods, and its goals, scientists have shown themselves consistently unwilling to compromise the numerical success of their theories.<sup>17</sup>

We can see here that for Kuhn the value of quantitative accuracy has been a permanent fixture of science. But it could not be denied that the level of quantitative accuracy, in its application to actual scientific episodes, has become ever more stringent as science has progressed. What was once considered to be accurate results are now scorned as totally inadequate. It is also apparent that the value of quantitative accuracy has become ever more important in a continuously widening circle of disciplines: it was originally only applicable to astronomy, but then mechanics, chemistry, electricity, heat, biology and so on became quantitative.<sup>18</sup> I think we can grant Kuhn the contention that

for the function that I have here ascribed to values, such relative stability provides a sufficient basis. The existence of a feedback loop through which theory change affects the values which led to that change does not make the decision process circular in any damaging sense.<sup>19</sup>

But there is another aspect of values which makes their role in decision-making uncertain, that is, though values may be shared by a community of scientists, the

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<sup>16</sup> "Objectivity, Value Judgement, and Theory Choice" p. 335.

<sup>17</sup> "The Function of Measurement in Modern Physical Science" pp. 212-13.

<sup>18</sup> See "Objectivity..." p. 335. At this point, the question which Feyerabend would feel compelled to ask concerns the validity, or applicability, of quantitative accuracy itself. Far from quantitative accuracy being a permanent fixture of science, Feyerabend would argue that quantitative accuracy is a recent phenomenon, a value which came to be of the utmost importance only with the advent of 'modern' science. The value of quantitative accuracy, and more generally, mathematiseability, is seen to lay in the opportunities which are thus presented for the control and manipulation of nature for technological purposes. The value of this, the form-of-life underpinning such a conception, is certainly challengeable from the perspective of different forms-of-life.

<sup>19</sup> *Ibid.*, p. 336.

application of those values may differ from scientist to scientist. What one scientist sees as simplicity, for example, might not be regarded as such by another scientist. The Copernican hypothesis was regarded in some circles as being significantly simpler than the Ptolemaic astronomy. But defenders of the Ptolemaic astronomy could point to the fact that acceptance of a heliocentric system brought in huge complications for the generally accepted cosmological world-view of Aristotelianism. Because Copernicanism carried implications which transgressed the narrow field of astronomy, its simplicity could be validly challenged.

Another problem for values-based decision-making is that "different values, taken alone, would often dictate different choices. One theory may be more accurate but less consistent or plausible than another."<sup>20</sup> So how does the recourse to values help us explicate the rationality of paradigm choice? Individual values can be interpreted differently by different scientists, and different values can often come into conflict. We seem to be left in a situation only marginally better than before.

The way in which Kuhn tries to resolve the situation is one which, though not systematically developed by Kuhn, is very promising. Kuhn contends that

What one must understand ... is the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of a group will ultimately find one set of arguments rather than another decisive.<sup>21</sup>

This is indeed what needs to be understood, but Kuhn goes on to say that "that process is persuasion".<sup>22</sup> I don't think it can be denied that persuasion plays a large role in scientific decision-making. But I think that more can be said and a clue can be found in the passage above. Kuhn states that one of the factors involved is the "set of shared values", it is this which I feel is the important point. It doesn't need to be denied that individual values can be interpreted differently by individual scientists, or that judgements made on the basis of individual values may conflict with judgements made on the basis

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<sup>20</sup> "Postscript", p. 185.

<sup>21</sup> *Ibid.*, p. 200.

<sup>22</sup> *Ibid.*

of other values. Those points can be accepted without qualms. But what can be denied is that scientific conflict is ever decided on the basis of one or two values alone. If the whole set of shared scientific values is brought into the decision-making process, then it may be hoped that this more complicated situation will lead to greater perspicuity in paradigm comparison.

The set of shared values can alleviate the problems of individual interpretation of values, and conflict between values, by supposing that the set of shared values are ranked hierarchically. Some values would be more highly prized, while others would be of lesser importance. For example, predictive accuracy seems to carry much more weight than simplicity in evaluating theories. This position seems to be one which Kuhn implicitly supported; we saw in the quote earlier that Kuhn regards quantitative accuracy as something prized by scientists above many other considerations. Kuhn also contends that "the demonstrated ability to set up and to solve puzzles presented by nature is, in case of value conflict, the dominant criterion for most members of a scientific group".<sup>23</sup>

The idea that the set of shared values are usually ranked hierarchically can be linked to the central Kuhnian notion of the exemplary scientific achievement. That is, though Kuhn has said that "The paradigm as shared example is the central element of what I now take to be the most novel and least understood aspect of this book",<sup>24</sup> it seems that Kuhn has not seen the full significance of the idea of the scientific exemplar. It is plausible to assume that it is the concrete scientific achievement which gives the scientist indications as to what the current hierarchy of values is, and how the various values should be interpreted. The *values are inherent within the concrete scientific achievement*, and the concrete scientific achievement functions as a temporary crystallisation of the hierarchy of values. For example, when Newton first proposed his theory, Cartesians found the postulation of action at a distance mysterious and occult: it was directly contrary to the ideal of mechanistic explanation. As it turned out, the

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<sup>23</sup> *Ibid.*, p. 205. This passage seems to imply that puzzle-formulation and puzzle-resolution is something different from values. But in other passages Kuhn implies that puzzle formulation and solution is an epistemic value. Puzzle-formulation and resolution would have to be considered to be a value, which, of course strengthens the argument for the existence of a hierarchy of values.

<sup>24</sup> SSR, p. 187.

explanatory power, and the empirical, predictive and quantitative accuracy of Newton's theory won the day. The importance, the value, of mechanistic explanation was eroded with the advent of Newtonianism. Though mechanistic explanation was still of utmost importance, as is evident in Newton's three laws of motion, mechanistic explanation could be waived if explanatory power and accuracy could be demonstrated, as is evident in Newton's law of gravitation. Scientists were thus guided in the construction of new theories by the values inherent in the Newtonian exemplar.

In cases of theory conflict within paradigms, the exemplary scientific achievement provides the scientists with implicit guidance as to theory preference. Suppose, for example, that the exemplary achievement exhibits great explanatory power, but is quite complex. In this situation, the scientific community would place greater importance upon the value of explanatory power: it would be higher in the hierarchy than the value of simplicity. Suppose also that there are two competing theories vying for the allegiance of the scientific community. One theory has great explanatory power but is very complex. The other theory has great simplicity but little explanatory power. Then, in this situation, if all other values are equal, the theory which will be adopted is the theory which exhibits the greatest explanatory power.

For Kuhn, this temporarily fixed hierarchy of values is one of the defining features of normal science. We can now see that the defining feature of 'crisis', of extraordinary science, is the *relinquishment* of commitment to any particular, agreed upon, hierarchy of values. In times of crisis, not only do scientists propose theories with different metaphysics and models, these theories also exhibit value structures at odds with the previous paradigmatic value hierarchy. The important point to keep in mind is that even though in times of crisis there is no agreed upon hierarchy of values, the set of values remain. Kuhn's resolution of the problems associated with paradigm choice in his early formulations consists in the idea that the debate over paradigm succession can be conducted according to known epistemic values. For example, explanatory power has often been presented, along with predictive accuracy, as the prime scientific value, but it can be argued that with the crisis in modern physics associated with the advent of

quantum physics, explanatory power took a back seat to empirical and predictive accuracy. And as we saw in Section 4.4 above, many other values of science, such as objectivity and coherence, were radically reconceptualised at the same time. It has been contended that it is precisely this aspect of quantum physics which Einstein found to be objectionable.<sup>25</sup> But the famous debates between Einstein and Bohr were conducted with the tacit agreement that certain sets of considerations were relevant. What was contested was the relative weights and importance given to the various values: what was contested was the hierarchy of values deemed appropriate for the running of science.

Kuhn contends that the variability associated with the application of values, once crisis has commenced and the previously fixed value-hierarchy is relinquished, constitutes a positive contribution to science:

The points at which values must be applied are invariably also those at which risks must be taken ... In matters like these the resort to shared values rather than to shared rules governing individual choice may be the community's way of distributing risk and assuring the long-term success of its enterprise.<sup>26</sup>

This is very reminiscent of Feyerabend's position *vis a vis* empirical traditions and proliferation. That is, Kuhn is saying that the unencumbered application of values produces a proliferation of theories. This is seen as good because a wide range of alternatives are proposed which subsequently ensures that one of them will be successful and be chosen as the next paradigm. This situation highlights one of the few major differences between Feyerabend and Kuhn. Feyerabend would like to see a proliferation of theories at all times, not just in times of crisis. Kuhn, on the other hand, sees proliferation as anathema to the normal running of science: proliferation is needed only in times of crisis where the existing paradigm has run into trouble and an alternative is needed.

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<sup>25</sup> See, for example, C.A. Hooker, "The Nature of Quantum Mechanical Reality" and his "Projection, Physical Intelligibility, Objectivity and Completeness: The Divergent Ideals of Bohr and Einstein" *British Journal for the Philosophy of Science*, 42, (1991) pp. 491-511.

<sup>26</sup> SSR, p. 186.

### 5.3.3 Kuhn Versus Laudan on Values.

Larry Laudan has criticised Kuhn's theory of values on two points: (i) Laudan sees Kuhn as committing what Laudan calls the 'covariance fallacy', and (ii) Laudan contends that Kuhn supports 'cognitive egalitarianism'. I argue below that both Laudan's claims are false. In the course of these rebuttals the character of Kuhn's value theory, as presented in the previous section, will become clearer.

Firstly, to the charge that Kuhn commits the 'covariance fallacy': the claim that theoretical/ontological claims are necessarily covariant with axiological/cognitive goals. "Each form of the fallacy assumes that the presence or absence of consensus with respect to factual claims can be used to infer the existence of agreement or disagreement with respect to cognitive aims".<sup>27</sup> Thus disagreements over cognitive goals cannot be rationally adjudicated for there is no higher court of appeal; any appeal to theory or ontology is also out of consideration because question-begging and self-referring. Kuhnian paradigms are assumed by Laudan to be covariant structures of just this type: they are all-or-nothing in character. "So far as I [Laudan] know, Kuhn never imagines that there might be fundamental ontological or theoretical differences between scientists who share the same cognitive goals".<sup>28</sup>

In reply to this objection of Laudan's, consider this quote from Kuhn:

I have implicitly assumed that, whatever their initial source, the criteria or values deployed in theory choice are fixed once and for all, unaffected by their participation in transitions from one theory to another. Roughly speaking, but only very roughly, I take that to be the case. If the list of relevant values is kept short ... and if their specification is left vague, then such values as accuracy, scope, and fruitfulness are permanent attributes of science.<sup>29</sup>

Here Kuhn does not take differences at the level of basic theory as evidence for *fundamental* differences of aim or value. Rather, for Kuhn, differences at the basic

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<sup>27</sup> L. Laudan, *Science and Values: The Aims of Science and Their Role in Scientific Debate*. p. 43.

<sup>28</sup> *Ibid.*, p. 44.

<sup>29</sup> Kuhn, "Objectivity ..." p. 335. More correctly, I feel, Kuhn should have said modern science, rather than the unqualified 'science'. It can be contended that pre-Galilean science did not have a place for fruitfulness in their value inventory; especially if fruitfulness is conceived in its association with content increase and novelty. See the discussion in chapter 2 above.

theoretical level will indicate *some* sort of difference in aims, goals or values, but the basic categories of valuation: puzzle-solving capability, accuracy, consistency, coherence, scope, simplicity and fruitfulness, survive as permanent fixtures of science. Thus, for Kuhn, scientists who do indeed support and share the same cognitive aims, values and goals, may yet differ in supporting fundamentally different theories. For example, the value structures of Newtonians and Einsteinians did not differ markedly, except possibly in greater emphasis being placed upon theoretical and mathematical coherence.

Kuhn has emphasised that theory change can evince value change, consequently, a further point against the idea that Kuhn commits the covariance fallacy can be gathered from the following passage:

if ... value changes had occurred as rapidly or been as complete as the theory changes to which they related, then theory choice would be value choice, and neither could provide justification for the other. But, historically, value change is ordinarily a *belated* and largely unconscious concomitant of theory choice, and the former's magnitude is regularly smaller than the latter's. For the function I have here ascribed to values, such relative stability provides a sufficient basis.<sup>30</sup>

Obviously, if value change, according to Kuhn, is a belated change, then the putative attribution to Kuhn of the covariance thesis is false. Laudan takes great pains to show that value change does not necessarily covary with theoretical/ontological change by presenting the example of the shift from the explicit value of pure observation, to the explicit acceptance of the idea of the postulation of unobservables in a hypothetico-deductive manner.<sup>31</sup> Even though scientists had, in fact, been working with unobservable, hypothetical entities for a considerable period of time, it was only in the 1830's that recognition of this fact was made explicit in the 'official' axiology of science. Laudan thus concludes that axiological standards do not covary with theory and ontology. It is obvious that this situation is exactly what Kuhn had in mind when he said that value

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<sup>30</sup> *Ibid.*, p. 336. my emphasis

<sup>31</sup> Laudan, *Science and Values*. pp. 55-60.

change is ordinarily a belated change with respect to the factual/theoretical changes which drive it.<sup>32</sup>

Laudan's second objection to Kuhn is the charge that Kuhn supports 'cognitive egalitarianism': "the thesis that all beliefs are epistemically or evidentially equal in terms of their support".<sup>33</sup> Laudan tries to temper the charge by saying that Kuhn presents the thesis in "a less extreme and more carefully articulated form",<sup>34</sup> but, nevertheless, Laudan contends that

He [Kuhn] says ... that because we have no rules that would unambiguously pick out a single theory *to the exclusion of all other possible theories* about the relevant domain, it is inevitable that the choice between any two theories could always go one way or the other, given any set of values or norms about what we expect our theories to achieve. Kuhn is not denying that rules play a role in the choice of scientific theories, but he is insisting that their intrinsic ambiguity precludes the possibility of decisive preferences *ever* being justified on the basis of shared methodological rules.<sup>35</sup>

Laudan goes on to give examples of theoretical situations where specific values would have given unambiguous preference for one theory over another: Newton's theory was empirically more accurate than Aristotle's; Newton's theory was of greater generality than Kepler's, and he concludes that "neither Kuhn nor anyone else has shown that most (let alone all) theory choice situations exhibit the impotence of 'shared criteria' to determine a preference".<sup>36</sup> What Laudan plays upon is a shifting in the use of the term 'theory'. Kuhn, in his article "Objectivity, Value Judgment and Theory Choice", uses the term 'theory' throughout the article, where it is plain in the context that it is not *theory*

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<sup>32</sup> The whole question of the dubiousness of covariance is belied by Laudan's own admission and delineation of the explicit/implicit dichotomy of axiological/value commitment. It can be argued that Galileo's theories were indelibly suffused with the postulation of unobservable processes: Galileo's analytic method, which was taken up enthusiastically by Hobbes and Descartes, sees observable processes as composed out of unobservable processes. Once this method is seen as presenting metaphysical insights, the road to atomism and the postulation of other unobservable entities is clearly indicated. Consequently, covariance, in fact, happened. The fact that the explicit admission of unobservable entities and processes took much longer to arrive does not impugn the fact that covariance was in operation. At that time the stress upon empirical values can be explained as part of the rhetoric of modern science, begun with Galileo, in order to stymie the objections of empirically minded Aristotelians.

<sup>33</sup> Laudan, *Science and Values* p. 30.

<sup>34</sup> *Ibid.*

<sup>35</sup> *Ibid.*, p. 31. my emphasis.

<sup>36</sup> *Ibid.*, p. 32.



choice which Kuhn is addressing, but *paradigm* choice. As Kuhn has stated in a different article, "though they [values] function at all times, their particular importance emerges when the members of a particular community must identify crisis or, later, choose between incompatible ways of practicing their discipline".<sup>37</sup> That is, the situation where the shared values of a community of scientists exhibit underdetermination of interpretation and weighting, are situations of paradigm choice. For Kuhn, theory choice *within* paradigms is relatively unproblematic, hence the entire structure of normal science. It is normal science because the moves to be made in the ongoing puzzle-solving and articulation of the paradigm, and the assessment of what is appropriate and what is not appropriate research, is settled and unproblematic, and this is so *because of the settled and unproblematic temporary interpretation and weighting of the shared value hierarchy*.

Laudan's examples are also highly misleading. Kuhn stated that it was in times of crisis and paradigm choice that values became especially important and where value interpretation and weighting becomes ambiguous. Consequently, to give the unambiguous preference of Newton's theory over Aristotle's by the late seventeenth century as an example of unambiguous value determination of preference, is neither here nor there in relation to Kuhn's theory of values. Of course it is the case that eventually paradigm choice via fulfilment of shared values is unambiguously determinable. The questions and issues that Kuhn wants to highlight are those surrounding the preference for Aristotle or Galileo, at the time of Galileo; exactly the time when the new paradigm was in its fledgling stage. This is an especially important time in that it was the interpretation of empirical accuracy, qualitative versus quantitative, which was one of the central issues over which the debates were conducted. By the time of Newton, empirical accuracy was deemed to be quantitative accuracy, rather than qualitative accuracy, so it is not very surprising that Newton's theory was better than Aristotle's in this area. If we turn to the preference for Newton's theory over Kepler's in relation to generality, then the ready assent as to Newton's preferability over Kepler's is highly uncontroversial for

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<sup>37</sup> Kuhn, "Postscript" in SSR, pp. 184-5.

two reasons. Firstly, Kuhn could contend that both Kepler and Newton were working within the paradigmatic tradition started by Galileo, hence Newton's theory could be considered to be normal science, where the interpretation and weighting of values had been settled. Secondly, nowhere in Kuhn's system is it denied that, *all other values being equal*, the theory which has, in this instance, greater generality, should be preferred unambiguously. The situations Kuhn has in mind are situations where different theories have strengths and weaknesses in relation to different values, or where it is the interpretation of the value in question which is in debate. For example,

The oxygen theory ... was universally acknowledged to account for observed weight relations in chemical reactions, something the phlogiston theory had previously scarcely attempted to do. But the phlogiston theory, unlike its rival, could account for the metals being much more alike than the ores from which they were formed. One theory thus matched experience better in one area, the other in another. To choose between them on the basis of accuracy, a scientist would need to decide the area in which accuracy was more significant.<sup>38</sup>

Or consider the situation with respect to Galileo and the Aristotelian world-view: Galileo's ideas could certainly be seen as having advantages in many areas, but the Aristotelian world-view, as just that, a world-view, had advantages in different areas such as generality. In both these situations competing theories had competing strengths, and that is all that Kuhn wanted to point out in saying that theory choice, in being value-driven, is not algorithmic.

Even in situations of paradigm choice, Kuhn's values are not completely impotent in helping scientists to come to a preference. Specifically, it is not the situation, contra Laudan, that Kuhn's values cannot lead to preferences amongst the set of all possible theories. Kuhn states that

Values like accuracy, consistency, and scope may prove ambiguous in application, both individually and collectively; they may, that is, be an insufficient basis for a *shared* algorithm of choice. But they do specify a great deal: what each scientist must consider in reaching a decision, what he may and may not consider relevant, and what he can legitimately be required to report as the basis for the choice he has made. Change the list, for example by adding social utility as a criterion, and some particular

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<sup>38</sup> Kuhn, "Objectivity ... " p. 323.

choices will be different, more like those one expects from an engineer. Subtract accuracy of fit to nature from the list, and the enterprise that results may not resemble science at all, but perhaps philosophy instead.<sup>39</sup>

That is, even in the case of paradigm choice, the set of possible candidates will be heavily circumscribed in their nature. Of course, within those limits there may be considerable scope for theoretical variation, and within that variation, unambiguous choice will not be immediately attainable, but that certainly does not lead to the conclusion of cognitive egalitarianism.

Laudan presents what he calls the reticulated model of scientific rationality and justification, as opposed to the hierarchical model:

Where the reticulational picture differs most fundamentally from the hierarchical one is in the insistence that there is a complex process of mutual adjustment and mutual justification going on among all three levels of scientific commitment. Justification flows upward as well as downward in the hierarchy, linking aims, methods, and factual claims. No longer should we regard any one of these levels as privileged or primary or more fundamental than the others. Axiology, methodology, and factual claims are inevitably intertwined in relations of mutual dependency.<sup>40</sup>

The fact of the matter is that Kuhn, far from advocating the hierarchical model, as Laudan insinuates, is advocating just such a model as Laudan presents. It is quite plain that Kuhn advocates the idea that theory/paradigm change can alter the structure and interpretation of the individual values, and the value hierarchy as a whole. It is equally obvious that ongoing research in normal science is governed and informed by the value structure. For Kuhn, values and theories are involved in a complex feedback loop of mutual adjustment and refinement.

#### **5.4 Conclusion: Problems and Projects.**

If we bring to Feyerabend's rather sketchy account of the details of values-based rationality, the sort of explication that we find in Kuhn, then the prospects of producing a coherent and plausible notion of rationality, based upon values, becomes ever more possible. However, there are, as yet, a number of problems facing an approach such as

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<sup>39</sup> *Ibid.*, p. 331.

<sup>40</sup> Laudan, *Science and Values*, pp. 62-3.

this. Firstly, the mechanism which triggers a rearrangement of the value hierarchy cannot itself be based upon values. Kuhn suggested that paradigms and values are involved in a feedback loop; values may shape the structure and the adoption of theories, but theories, as exemplars, define the status of values. This seems plausible enough, but some of the big questions are not adequately answered in this way. The deepest, most far reaching scientific revolutions involve the radical upheaval of the value system of science. In fact, it could be said that the degree of upheaval in the value system is directly correlated with the depth of the revolution.<sup>41</sup> For example, it has been said by a number of commentators that the scientific revolution associated with Copernicus *et al*, *The Scientific Revolution*, can be identified as the then unprecedented marriage of explanatory power and predictive, quantitative accuracy.<sup>42</sup> The Aristotelian system, including the values inherent in that system, had no place for such a union. The transition from Aristotelian science to modern science did not constitute a change in the hierarchy of values; rather, the transition constituted a change in the set of values itself. The idea that theory change can elicit value change seems to be an adequate account when considering small scale value change, and, conversely, the idea that values shape the direction of theory change is also plausible when considering theory change within paradigms. But when such large-scale theoretical and value disruptions such as that of the Copernican revolution are considered, it becomes a problematic account. It can be convincingly argued that Galileo was working within the new value system *before* he proposed his theories, and that it was necessary for Galileo to do this so as to make it possible for the new system to arise. How is this notion possible according to the idea that exemplars and values are indelibly linked?

A second problem arises out of the first and brings us back to questions raised by Feyerabend. If it could be argued that, apart from minor qualifications, changes in application, and reshuffling, science as a whole has shown a great stability in its value

<sup>41</sup> This point has been made by E. McMullin, "Rationality and Paradigm Change in Science" in P. Horwich (ed) *World Changes: Thomas Kuhn and the Nature of Science*.

<sup>42</sup> See McMullin *Ibid.*, and S. Amsterdamski, *Between History and Method: Disputes About the Rationality of Science*. Amsterdamski probably captures the marriage better when he characterises it as the marriage of *episteme* and *techne*.

system, then it becomes plausible to assume that science itself is a tradition. But if science as a whole is a tradition, then the whole question of rationality can be asked again. The particular structure of values-based rationality may be something only applicable to science: like all traditions, rationality is an intra-traditional notion. We are left with three options here: we can say that science has its own rationality, other large-scale traditions have their own rationality, and never the twain shall meet. Or, we could contend that we would have to repeat the process all over again, identifying supra-traditional values on a very large scale. Or, we could say that there is an identifiable continuity in values between large-scale traditions of various sorts.

If there is continuity of values between large-scale traditions of various sorts, how is this continuity characterised? What is it, in the fine-textured structure of the process of valuation, which could be the key to understanding rationality, wherever and whenever it is, without becoming embroiled anew in 'Rationalistic' philosophies? These questions will be addressed in the next two chapters.

## **6 Dewey's Pragmatic Naturalism and Values-Based Rationality**

### **6.1 Introduction.**

We have seen that Feyerabend seems to be supporting a values-based conception of rationality as an alternative to 'Rationalist' accounts of rationality. Moreover, we have also seen that Kuhn explicitly proposed a conception of scientific rationality revolving around the idea of epistemic values. But, as we saw in section 5.4, there remain doubts as to the general applicability of a values-based conception of rationality. Most important among these doubts concerns the worry that different, large-scale, traditions of thought, with their very different value-structures, may not be able to be comparatively rationally assessed.

It is my contention that such worries surrounding the idea of values-based rationality are ultimately unfounded. Specifically, if we look to the philosophy of John Dewey, with its very detailed treatment of values and rationality, then we can find resources in the idea of values-based rationality which can overcome residual doubts. If Feyerabend had included within his philosophy some of the insights of Dewey's philosophy, then he would have been able to present a viable, alternative account of rationality.

Dewey was a prolific writer, both in volume and scope; consequently, systematic exposition of Dewey's philosophy is not possible here. In section 6.2 I will give a short background to Dewey's philosophy, highlighting the problematic situation which led Dewey to propose his ideas concerning values and rationality. This is followed, in section 6.3, by a presentation of Dewey's ideas concerning values. In section 6.4 will be presented those ideas of Dewey's which can be fruitfully appropriated so as to complete Feyerabend's project of producing an alternative conception of rationality.

### **6.2 Dewey's Problematic.**

For Dewey, all existence is historical: it is marked by beginnings, mediation, and ending. All inquiry is also historical, in that it is a temporal process marked by

problematic situations, mediation, and resolution. Moreover, anything which is historical is also individual and unique and cannot be made to be merely an instance of a generalisation:

Generalized facts, when they are taken to be individual events complete in themselves, lead to a picture of the universe in which occurrences are exactly like one another. There is repetition but no development; mechanical production but no cumulative movement toward an integrated consequence ... When, however, events are viewed in their connections ... nature is seen to be marked by histories. (QC, p. 247.)

This idea of the unique, historical and individual character of existence, does not imply that we cannot have any generalisations or abstractions. Rather, for Dewey, "Nature *has* a mechanism sufficiently constant to permit of calculation, inference and foresight." (QC, p. 248.) But this mechanism, this physical lawfulness, is always of the nature of statistical generalisations: they express possibilities. As such, "They are not descriptions of the exact structure and behavior of any *individual* thing". (QC, p. 248.) This leads Dewey to contend that "No mechanically exact science of an individual is possible. An individual is a history unique in character." (QC, p. 249.) Existence is always individual and unique: it has its own non-repeating quality. Abstractions and generalisations are tools which inform us of the range of possible outcomes of these unique temporal processes. Once we know what outcomes are possible, we can then manipulate and control existence so as to bring about those states of affairs which we would prefer to have happen: "Standardizations, formulae, generalizations, principles, universals, have their place, but the place is that of being instrumental to better approximation to what is unique and unrepeatable." (EN, p. 117.) These unrepeatable states or ends must, of course, be of some sort or kind, but they will nevertheless bring with them unique aspects and qualities. Moreover, these unique aspects and qualities, once attained, may show the way to new end-objects, new historical culminations, of a kind not previously attained.

In science, for example, if we want to know some particular fact, say, why certain rocks exist in a particular mountain range, we bring to bear generalised facts, principles and laws: tectonic plate movements, the distinctions associated with rock formation such

as sedimentary, metamorphic, and so on. These generalisations do not provide us with the knowledge of the particular rocks from the particular mountain range in question. Rather, these generalisations function as boundary conditions as to the range of possible influences and histories: they are tools and instruments and their purpose is to place the particular rocks within a context of limited possibility. The identification of the particularity of the rocks under scrutiny involves a further process of determination and limitation of possibilities. Moreover, this *further* limitation will produce a unique set of circumstances inapplicable as a description of any other geological feature in other mountain ranges.

This conception of existence as temporal and historical in character, and therefore as constantly unique and non-repeatable, is seen by Dewey as a characterisation of *all* existence, at whatever level we care to think about: physical, social, ethical, aesthetic, and so on. It is meant to be constitutive of existence *qua* existence; consequently, all aspects of human endeavour is to be understood as falling within the scope of temporal-historical understanding. Most especially, Dewey sees the process of inquiry, and therefore of rationality, as being essentially temporal and historical. Consequently, we can conclude that, for Dewey, all rational, right-thinking must be contextual. All rational thinking must be contextual because each moment in time, each time-slice of a multitude of interacting and non-interacting histories, is ultimately individual, unique and novel. If we are to know what the rational thing to do in any situation is, then we must be aware of the peculiarities of the specific situation at hand.

However, the contention of historicity and contextuality creates a double-edged situation: context "gives point to everything said ... Context is incorporated in what is said and forms the arbiter of the value of every utterance".<sup>1</sup> That is, nothing can be said, by anyone, and be completely understood, without a knowledge of the presuppositions built into the context within which a speaker speaks.

A background is implicit in some form and to some degree in all thinking, although as background it does not come into explicit purview; that is, it

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<sup>1</sup> Dewey, "Context and Thought", in Bernstein (ed) *On Experience, Nature and Freedom*, p. 92.



does not form a portion of the subject matter which is consciously attended to ... Surrounding, bathing, saturating, the things of which we are explicitly aware is some inclusive situation which does not enter into the direct material of reflection ... There is always that which continues to be taken for granted, which is tacit, being 'understood'.<sup>2</sup>

That is, we are always immersed within a context which provides the implicit, unstated, meanings of which we are at most times unaware. The background, the context, provides the superstructure around which explicit meanings are contoured. This context is seen by Dewey as all pervasive: it affects everything from the highest culture and art, to the mundane everyday activities of normal existence.

There exists at any period a body of beliefs and of institutions and practices allied to them. In these beliefs there are implicit broad interpretations of life and the world. These interpretations have consequences, often profoundly important. In their actual currency, however, the implications of origin, nature, and consequences are not examined and formulated ... this cultural context of beliefs and allied institutions is irretrievably there; reference to it is taken for granted and not made explicit.<sup>3</sup>

We have here an admission by Dewey of the deeply contextual, implicit, nature of much of our behaviour and thought. But if this is the case, how are we able to transcend the constraints embodied in our practices and institutions? If the context is implicit, tacit, 'understood', then how are we able to step back from the context in which we are immersed and explicitly examine and test the presuppositions involved?

This is an especially important question for Dewey to answer; Dewey's entire philosophy revolved around the ways and means by which we can avoid the spurious quest for certainty:

exaltation of pure intellect and its activity above practical affairs is fundamentally connected with the quest for certainty which shall be absolute and unshakeable ... Through thought ... it has seemed that men might escape from the perils of uncertainty ... The intellect ... according to the traditional doctrine, may grasp universal Being, and Being which is universal is fixed and immutable. (QC, pp. 6-7.)

Moreover, this quest for certainty has resulted in the notion that reason

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<sup>2</sup> *Ibid.*, pp. 98-9.

<sup>3</sup> *Ibid.*, p. 106.

designates both an inherent immutable order of nature, superempirical in character, and the organ of mind by which this universal order is grasped. In both respects, reason is with respect to changing things the ultimate fixed standard - the law physical phenomena obey, the norm human action should obey. For the marks of 'reason' in its traditional sense are necessity, universality, superiority to change, domination of the occurrence and the understanding of change. (QC, p. 212.)

In contrast, Dewey sees practical activity, doing, as the area to which we must direct our attentions. It is practical activity which highlights the need for adaptive response to unique situations.

Intelligence ... is associated with *judgment*, that is, with selection and arrangement of means to effect consequences and with choice of what we take as our ends. A man is intelligent not in virtue of having reason which grasps first and indemonstrable truths about fixed principles, in order to reason deductively from them to the particulars which they govern, but in virtue of his capacity to estimate the possibilities of a situation and to act in accordance with his estimate ... Whenever intelligence operates, things are judged in their capacity of signs of other things. (QC, p. 213.)<sup>4</sup>

Consequently, if we are to be intelligently adaptable and responsive to the demands of ever new unique, non-repeatable, situations, and not simply responding to new situations as we have done before in mechanical, habitual fashion, or in manners which presuppose an immutable fixed reality, then the context within which we find ourselves, no matter how entrenched, must be available for revaluation and revision. Consider this quote from Dewey:

Our life has no background of sanctified categories upon which we may fall back; we rely upon precedent as authority only to our own undoing - for with us there is such a continuously novel situation that final reliance upon precedent entails some class interest guiding us by the nose whither it will.<sup>5</sup>

Thus, the inescapable context of thought and behaviour, which must always be kept in mind if fallacious conclusions are to be avoided, is also something which must be constantly transcended so that illegitimate a priorisms and class, power interests are to be avoided: context is both the "arbiter of the value of every utterance" and that which must be transcended. The question must be asked, how is this balance, this 'essential tension'

<sup>4</sup> Cf. LTI, pp. 9-10.

<sup>5</sup> Dewey, "The Need for a Recovery of Philosophy" in Bernstein (ed) *On Experience, Nature and Freedom*, p. 68.

between tradition and innovation, to be achieved? What processes can ensure that we are always open to criticism of even the most deeply entrenched conceptions of life and the world?

### 6.3 Values and Rationality.

#### 6.3.1 The Process of Valuation.

As will become clearer as we proceed, the idea of values is a crucial notion for understanding Dewey's philosophy: valuation *is* inquiry; valuation *is* criticism.

we suppose that when we are reflecting upon or inquiring in to the good or value of some act or object, we are dealing with something as simple, as self-inclosed, as the simple act of immediate prizing or welcoming or cherishing performed without rhyme or reason, from instinct or habit. In truth ... *to be* good means precisely ceasing to look at it as a direct, self-sufficient thing and considering it in its consequences ... a man may not only enjoy a thing, but he may judge the thing enjoyed to be good, to be a value. But in so doing he is going beyond the thing immediately present and making an inference to other things, which, he implies, are connected with it. (EEL, pp. 356-7.)

The act of valuation is an act of inquiry and criticism: to say something is better than other things, apart from expressing immediate enjoyment, is to have discriminated, weighed and judged the alternatives as to their efficacy in producing consequences which will in the future be appreciated and enjoyed.<sup>6</sup>

This position implies that valuation is not an arbitrary, subjective expression of preference. If valuation is simply an expression of liking and disliking, then there is no sense in the contention that values are involved in discriminating judgment, rather, they just are.

They exist; they are given. But as given they are *not* determinate values. They are not *objects* of valuation; they are *data for* a valuation ... were they already definite values, they would not be estimated; they would be stimuli to direct response ... A value ... means a *consideration*, and a

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<sup>6</sup> This conception is identical to Kuhn's response to criticisms imputing subjectivity to his idea of values in science, see his "Objectivity, Value Judgment and Theory Choice" pp. 336-8.

consideration does not mean an existence merely, but an existence having a claim upon judgment. (EEL, pp. 363-4.)

Nor is valuation an alignment of ourselves to, or a realisation of, a pre-existing independent value structure. Thus,

it is frequently assumed ... that valuation is a process of applying some fixed or determinate value to the various competing goods of a situation; that valuation implies a prior standard of value and consists in comparing various goods with the standard as the supreme value. (EEL, p. 370.)

This situation is unacceptable for Dewey, for it implies that ends can be discriminated from means in a categorical manner. Dewey does not accept any inherent separateness between means and ends: "only by a judgment of means - things having value in the carrying of an indeterminate situation to a completion - is the end determinately made out in judgment." (EEL, p. 371.) To conceive of means as isolated from intelligent choice of ends to be reached, is to see the world as completely deterministic. But, for Dewey, this conclusion is belied by the empirical facts of experience: in formulating ends and acting upon them, we actively control and manipulate existence so as to bring about states of affairs which would not have existed if we had not actively striven to bring them into existence.

The subject-matter [of practical judgment] implies that it makes a difference how the given is terminated: that one outcome is better than another, and that the proposition is to be a factor in securing (as far as may be) the better. In other words, there is something objectively at stake in the forming of the proposition. (EEL, p. 339.)

The subject-matter of practical judgment concerns a particular problematic situation; a formulation of an end to be attained which will be better than the original situation, and which, moreover, it is possible to achieve, and which, in addition, would not be achieved without the formulation of the practical judgment in question. Practical judgments imply active participation to bring the desired end product to fruition. Thus practical judgments are existentially formative and constructive: when acted upon they change the external world so as to produce, in experience, a better world.

A practical proposition is binary. It is a judgment that the given is to be treated in a specified way; it is also a judgment that the given admits of such treatment, that it admits of a specified objective termination. It is a

judgment, at the same stroke, of end - the result to be brought about - and of means. (EEL, p. 340.)

The utopian, romantic, idealistic conception, which sees ends as pre-existing ends-in-themselves, also takes away from human endeavour the active, constructive aspects which Dewey sees as necessary to have in a changing universe. A fixed and final structure of ends will only retard ongoing valuation and judgment: pre-existing values, designed for particular purposes, will act as constraints to possible solutions of problematic situations. Action will thus be forced into the pre-given moulds and adequate, adaptive response consequently ruled out. Allegiance to such a pre-given end-value structure, in the face of problematic situations requiring *reevaluation*, will bespeak of an authoritarian attitude advanced in the interests of a privileged class, or culture. Moreover, if values were determinate and pre-given, then situations would not require valuation, they would simply be stimulus for response according to the dictates of the value system. Thus, for Dewey, *whenever we truly value*, we are reconstructing our values according to the exigencies of the particular problematic situation at hand: "to judge value is to engage in instituting a determinate value where none is given." (EEL, p. 368.)

Some may find this position of Dewey's untenable, surely, they may say, the value of honesty, for example, is something which can be supported and upheld as a pre-given, sufficiently determinate value, such as to guide conduct in situations requiring valuation. Dewey's reply would be to say that values such as honesty are *not* determinate with respect to particular problematic situations. Every problematic situation requiring valuation is unique and individual; the fact that the situation is problematic implies that if honesty is a consideration, then it is not known how it is applicable. Different situations require individual responses, and these responses are dependent upon the details of the particular situation: there are as many permutations of being honest as there are

problematic situations.<sup>7</sup> Of course, it is obvious that values such as honesty are often involved in valuation, but

the more completely the notions of the model is formed outside and irrespective of the specific conditions which the situation of action presents, the less intelligent is the act. Most men have their ideals of the model changed somewhat in the face of the actual offering ... The man who is not accessible to such change ... [has] become a reacting machine. In short, the standard of valuation is formed in the process of practical judgment or valuation. It is not something taken from outside and applied within it - such application means there is no judgment. (EEL, pp. 373-4.)<sup>8</sup>

That is, "if the standard is already given, all that remains is its mechanical application to the case in hand." (EEL, p. 374.)<sup>9</sup> It is also obvious that there are many situations in which honesty is not the best policy. Consequently, to uphold honesty as something to be followed in all situations is either to stymie adaptive response because of a too rigid interpretation of what honesty is, or to leave one with an empty and vacuous maxim, irrelevant to successful conduct because it can give no concrete, practical guidance as to purpose and action.<sup>10</sup>

In contradistinction to the views that valuation is liking or disliking, or reference to pre-existing value structures, Dewey's position implies that

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<sup>7</sup> Thus, Dewey contends that we must "abandon the notion of a predetermined limited number of ends inherently arranged in an order of increasing comprehensiveness and finality ... [for] natural termini are as infinitely numerous and varied as are the individual systems of action they delimit" EN, p. 395.

<sup>8</sup> We have here an example of Dewey's anticipation of post-positivist philosophy. Specifically, this quote mirrors the sentiments and contentions of H.I. Brown, in his *Perception, Theory and Commitment and Rationality*, wherein he criticises the algorithmic, classical model of rationality. Cf. my discussion of these points in chapter 1.

<sup>9</sup> Thus, Dewey does not deny that we often have and utilise pre-existing, ordered, value systems. Since situations often overlap, without being identical, this is often unequivocally justifiable. But their status as pre-existing values is always hypothetical and revisable. See, for example, LTI, p. 168. Moreover, in these situations the value system does not function as values, rather, they function as facts. Values only exist in those situations wherein an as yet indeterminate situation needs to be resolved and determined through valuation and judgment. Confusions regarding the through and through *functional* nature of all distinctions in Dewey's philosophy has retarded understanding of his philosophy. Thus, the idea that a particular statement could function as a value in one problematic situation, and as a fact in another, has been thought to be an impossibility. See, for example, K. Neilsen, "Dewey's Concept of Philosophy" in Tiles (ed) *John Dewey: Critical Assessments Vol. IV. Nature, Knowledge and Naturalism* esp. Section. IV. And M. White, "Value and Obligation in Dewey and Lewis" in Tiles (ed) *John Dewey: Critical Assessments Vol. III. Value, Conduct and Art*. Without going into detail, it seems to me that these writers haven't fully appreciated the functional nature of Dewey's philosophy; consequently, they have implicitly relied upon theories of value which exclusively dichotomise values and facts.

<sup>10</sup> Cf. QC, pp. 265-6.

a judgment of value is simply a case of a practical judgment, a judgment about the doing of something ... It asserts that value-objects mean simply objects as judged to possess a certain *force* within a situation temporally developing toward a determinate result. To *find* a thing good is ... to attribute or impute nothing to it ... But to consider *whether* it is good and how good it is, is to ask how it, *as if acted upon*, will operate in promoting a course of action. (EEL, 358-9.)

Thus, judgment is valuation,

as long as I judge, *value* is indeterminate. The question is not what a thing will do - I may be quite clear about that: it is whether to perform the act which will actualize its potentiality. What will I have the situation *become* as between alternatives? And that means what force shall the thing as means be given? ... When its status in these respects is determined, its value is determined; judgment ceases, action goes on. (EEL, p. 361.)

Dewey offers the process of buying a suit as an example of valuation and judgment. When buying a suit, we have a particular purpose in mind for which the suit is required; various aspects of suits can then be delineated as being value-objects:

Relative, or comparative, durability, cheapness, suitability, style, aesthetic attractiveness constitute value traits [of suits]. They are traits of objects not *per se*, but *as entering into a possible and foreseen completing of the situation*. Their value is their force in precisely this function. The decision of better and worse is the determination of their respective capacities and intensities *in this regard*. Apart from their status in this office, they have no traits of value for knowledge. (EEL, p. 362.)

Moreover,

Value is not determined by comparing various suits with an ideal model, but by comparing various suits with respect to cheapness, durability, adaptability *with one another* - involving, of course, reference also to length of purse, suits already possessed, etc., and other specific elements in the situation which demands that something be done. (EEL, p. 373.)

That is, in every situation requiring valuation and judgment we are creating new values pertaining to the specific circumstances at hand, we are not relying upon pre-existent ideals or values.

It is essential that we are clear as to what Dewey is saying here. Dewey's stress upon means-ends relationships is not meant to be understood in terms of rational choice, decision theory type analyses of means-ends relationships. The idea that rationality consists in the best choice of means to procure a given end is anathema to Dewey's

analysis. Rather, rationality and valuation is indelibly linked to the idea that the end is not something given, such that we only have to decide which means will best achieve that end. The end is itself determined in valuation amongst a set of alternative ends, and it is determined in association with the means available to achieve the various competing ends, and in relation to sets of possible consequences perceived as resulting from the adoption of any one end as the conclusion of judgment. The chosen end is *then* additionally valued because of its perceived superiority, if attained, of being a means to the end of resolving the problematic situation which evoked the necessity for judgment and valuation. For Dewey, valuation is essentially linked to possibility:

the relation between objects as known and objects with respect to value is that between the actual and the possible. 'The actual' consists of given conditions; 'the possible' denotes ends or consequences not now existing but which the actual may through its use bring into existence. (QC, p. 299.)

In as much as rational decision theory sees calculation of means as involving the selection of action from amongst sets of possibilities, Dewey would agree that some rationality is involved. But the idea that ends are not involved in the calculation of possibilities, that ends are simply what they are, given, means that Dewey would reject rational decision theory approaches as lacking an essential component of what it is to be rational.<sup>11</sup>

### 6.3.2 Science, Kuhn and Dewey.

The example of buying a suit, being as it were an example of practical reasoning, may not be considered relevant to the theoretical reasoning which we find in science. This is completely contrary to Dewey's conception of inquiry: all inquiry can be conceptualised in the same manner.

evaluative judgments ... enter into the formation of *all* final judgments ... The scientific worker has continually to appraise the information he gathers ... he has to appraise its bearing upon what problems to undertake and what activities of observation, experimentation and calculation to carry out. While he 'knows' ... systems of conceptual materials, including

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<sup>11</sup> For a discussion of some of the problems associated with rational decision theory, see B. Hindess, *Choice, Rationality, and Social Theory*.



laws, he has to estimate their relevancy and force as conditions of the particular inquiry undertaken. (LTI, p. 174.)

Whenever a scientist must make a choice, that scientist is engaged in valuation and judgment.

We can see here that the values which, for example, Kuhn proposes as those which are operative in science: empirical quantitative accuracy, simplicity, fruitfulness, comprehensiveness, and so on, fulfil the role of value traits in science; just as the value traits above were those applicable to suits. Problematic situations arise in science, various alternatives are proposed, these alternatives are then considered and weighed with respect to the various value traits considered relevant in the case at hand. The alternatives are not compared to some pre-existing model of what should be the case, nor are they compared and adjudged according to some fixed antecedent maxim of rationality. Rather, the alternatives are compared with each other in respect to the various value traits deemed to be relevant in producing a resolution of the problematic situation.

If we compare Dewey with Kuhn, we can see why Kuhn could contend both that scientific values have remained stable over the history of science, and that the content of these values has evolved over the history of science. We can also see how values may differ according to the requirements of differing, particular situations. As an example, consider quantitative accuracy, which, as we saw, was considered by Kuhn to be a necessary concomitant of scientific advance. In Dewey's conception, quantitative accuracy may function as a value trait in scientific research, but as such, there is no prior model, of any specificity or determinateness, as to what quantitative accuracy will be in any particular research-inquiry situation, *apart from what is present in the concrete scientific alternatives at hand*. Thus, in a particular research situation, all other relevant factors being equal, the theory, concept or technique which possesses, or potentially possesses, the greater quantitative accuracy over the other alternatives will be preferred in that situation. It is not hard to then imagine a hypothetical history of science, which would mirror the actual history of science, wherein the level of quantitative accuracy is

always preserved or bettered in ongoing research. A particular theory, concept or technique becomes an exemplar of what it means to possess quantitative accuracy; there is thus produced a constantly evolving value, being redetermined and revalued at intermittent intervals.

It is also easy to see that values will be interpreted differently according to the specific nature of the research situation. Quantitative accuracy may be a value ubiquitous in science, but even within one research field the precise level of quantitative accuracy may radically vary; for example, certain problems in physics may require accuracy of a very high order, but in other physical problems, possibly less developed, or indicative of the nature of the subject matter, accuracy may be of a significantly lesser degree. Quantitative accuracy may also be indispensable for, say, meteorology, but a physicist may find the level of accuracy laughable. What matters is that the level of quantitative accuracy, in whatever field it is relevant, is discriminatory for that field.

Striking examples of the culmination of the change in value systems provide us with the best examples of Kuhnian normal science. It is interesting to note that Dewey would have agreed with Feyerabend's criticisms of normal science. For Dewey, the fact that we may arrive at a point wherein science becomes paradigm governed, 'normal' science, is an indication that valuation, and therefore criticism, is absent from the deeper levels of science. The exemplary scientific achievement, and the value hierarchy encapsulated within it, instead of being treated as provisional and hypothetical, are being treated as facts and presuppositions. This Dewey sees as the greatest danger to science:

directing conceptions tend to be taken for granted after they have once come into general currency ... Failure to examine the conceptual structures and frames of reference which are unconsciously implicated in even the seemingly most innocent factual inquiries is the greatest single defect that can be found in any field of inquiry ... Failure to encourage fertility and flexibility in formation of hypotheses as frames of reference is closer to a death warrant of a science than any other one thing. (LTI, pp. 507-8.)<sup>12</sup>

Of course, Dewey would also agree that the full explication and the drawing out of the consequences and implications of any particular idea is of the greatest importance in

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<sup>12</sup> Cf. also EEL, p. 439.

localising the strengths and weaknesses of that idea. But this articulation of a paradigm should not blind scientists to alternative possibilities not present within the conceptual structure of particular paradigms, to the extent that only those alternatives offered within a paradigm are considered worthy of pursuit.

At the end of the last chapter I raised the question of whether Kuhn's conception of the role of values in science could account for large-scale paradigm change. Specifically, Kuhn sees theory and value as involved in a feedback loop: theory change, of a sufficiently large kind, can evince value change, and values then direct theory formulation. Thus, important scientific achievements become the concrete exemplars of the values to be followed in subsequent research. However, it can be argued, for example, that Galileo must have been working within a new value system *before* he produced his concrete scientific results. This problem can be partly alleviated if we work with the concept of valuation which Dewey espouses. Because valuation must always be related to an individual problematic situation, initial small adjustments in the existing value system may be introduced in relation to the resolution of a specific problem. The implications, consequences and possible comprehensiveness of this initial exemplar are then explored. Each successive step in attempting to resolve the burgeoning problems and tensions involved in the new ideas proposed, being the result of further re-valuations, will push more and more adjustments to the pre-existing value system. Consequently, we must suppose that paradigm change is necessarily an extended historical process. If we identify paradigm change with the striking syntheses, the consummatory fulfilments, of the historical process, such as that given by Newton, then we will miss the crucially important formative genesis of the ideas involved. To say that Galileo was working within a new value system before he produced his results has aspects of both truth and falsity. Certainly some aspects were in place before Galileo's research: mathematical knowledge had advanced; the Reformation had changed the culture with respect to attitudes towards authority; and Copernicus had proposed his heliocentric system. But other aspects of the new value system only developed along with Galileo's ongoing research.

This idea of paradigmatic drift, though only implicit in Dewey, has been explicitly defended by Laudan under the rubric of the 'evolution of research traditions'.<sup>13</sup> Laudan contends that

There is much continuity in an evolving research tradition. *From one stage to the next*, there is a preservation of most of the crucial assumptions of the research traditions ... But the emphasis here must be on *relative* continuity between *successive* stages in the evolutionary process. If a research tradition has undergone numerous evolutions in the course of time, there will probably be many discrepancies between the methodology and ontology of its *earliest* and its *latest* formulation.<sup>14</sup>

For example, what was called Newtonian physics in the late 1800's was a very different science to that of its namesake of two hundred years before. Given this evolution of traditions/paradigms, it is easy to see that continual evolution can result in the formulation of a new tradition/paradigm. Just as in biological evolution genetic drift can eventually produce different species from a common ancestral gene pool, so too new paradigms can evolve out of old paradigms, notwithstanding radical departures in deep-level assumptions.

Nevertheless, there still remain some residual uncertainties with such a view: though the idea of paradigmatic drift can explain much of the history of science, there still remains, in the largest paradigm changes, such as that of the Copernican revolution, leaps in paradigmatic structure. These leaps are much smaller than that which was suggested by the early Kuhn, but, nevertheless, they can be quite dramatic. For example, through hindsight we can see that there had been much evolution in Aristotelian thought towards ideas encapsulated within the scientific revolution; nevertheless, the work of Galileo constituted a dramatic leap contrary to many of the basic Aristotelian assumptions. In situations such as this, defenders of the old regime can simply dig their heels in and contend that the new ideas are just not rational: there has been a punctuation in the continual evolution of ideas big enough to create ideological division.

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<sup>13</sup> Laudan, *Progress and its Problems*, pp. 95-100.

<sup>14</sup> *Ibid.*, pp. 98-9.

This uncertainty is not a problem for Dewey's concept of valuation, for Dewey has resources which can cope with this last hitch. Specifically, all the processes of valuation discussed in this section are meant to be understood in the context of what may be called general values of inquiry. These general values of inquiry can be seen to form a bridge between the support of values given by Feyerabend and Kuhn, and the presentation of a viable, alternative, concept of rationality.

## **6.4 Four General Values of Inquiry.**

### **6.4.1 Introduction.**

In Dewey's philosophical system we can identify four overarching values of inquiry: (i) the necessity of empirical experience (ii) the idea of fruitfulness (iii) the drive for comprehensiveness, and (iv) the necessity for experimental testing. These four ideas are, in Deweyan philosophy, general values of all inquiry. This is a very important point: all knowledge, wherever it is to be found, if it is to be valid and rational knowledge, must be characterised by these four values. If a claimant to knowledge cannot be connected to empirical experience, then it does not qualify as a knowledge claim. If claimants to knowledge cannot act as guides to action and resolution of problematic situations, then they are worthless. If claimants to knowledge are only ever fragmentary and isolated, then they are not knowledge, they do not go beyond immediate experience to that which is not immediately present. If claimants to knowledge are not experimentally testable, whether by decision or internal structure, then they are again worthless as knowledge claims until we decide to subject them to test, or until they are construed such as to admit of test.

These four general values of inquiry are the means by which debate across paradigmatic gaps, in any one historical process, can proceed. They are the means whereby alternate claims to rational allegiance; for example, Western medical science and Chinese traditional medicine, can be brought together and the beginnings of rational comparison can proceed.

#### 6.4.2 Experience.

As outlined in Section 6.2 above, Dewey sees all existence as inherently historical and temporal; this is no less the case when we analyse empirical, perceptual experience. In fact, Dewey sees our perceptual experience as the source of all our notions of temporality: perception is identified with the 'now' of history; and it exposes us to the unique and non-repeatable aspects of ongoing temporal existence. However, in a similar fashion to the discussion in Section 6.2, perception also carries with it the residues of previous lived experience: presuppositions concerning the nature of the world fused into the structure of empirical experience. It is this recurrent tension in experience which leads Dewey to his ideas concerning value, as discussed in Section 6.3 above, and which ultimately leads to the importance which Dewey places upon testability and experimentation, to be discussed in the following section.

Dewey conceives of experience as *primarily* non-cognitive, non-reflective and non-intellectual. In such experience, "factors and qualities hang together; there is a great variety of them but they are saturated with a pervasive quality." (EEL, p. 5) In immediate experience there is a non-logical unity of subject and object, person and world, which is taken for granted and simply lived.

It may most easily be described from a negative point of view: it is a type of experience which cannot be called a knowledge experience without doing violence to the term 'knowledge' and to experience. It may contain knowledge resulting from prior inquiries; it may include thinking within itself; but not so that they dominate the situation and give it its peculiar flavor. Positively, anyone recognizes the difference between an experience of quenching thirst where the perception of water is a mere incident, and an experience of water where knowledge of what water is, is the controlling interest; or between the enjoyment of social converse among friends and a study deliberately made of the character of one of the participants. (EEL, p. 2.)

This idea is directly contrary to the idea that experience is a primarily cognitive affair: where our everyday interactions with the world are characterised by relations of knower and known. A person who believes that experience is through and through cognitive in character

generally forms a habit of supposing that no qualities or things at all are present in experience except as objects of some kind of apprehension or awareness. Overlooking ... that things and qualities are present to most men most of the time as things and qualities in situations of prizing and aversion, of seeking and finding, of converse, enjoyment and suffering, of production and employment, of manipulation and destruction, he thinks of things as either totally absent from experience or else there as objects of 'consciousness' or knowing. (EEL, p. 3.)

We see here that Dewey identifies consciousness with knowing, with cognition. The main point to be extracted from these quotes is the idea that experience is not primarily cognitive; rather

'Consciousness' ... is only a very small and shifting portion of experience. The scope and content of the focused apperception have immediate dynamic connections with portions of experience not at the time obvious. The word I have just written is momentarily focal; around it there shade off into vagueness my typewriter, the desk, the room, the building, the campus, the town, and so on. *In* the experience, and in it in such a way as to *qualify* even what is shinningly apparent, are all the physical features of the environment extending out into space no one can say how far, and all the habits and interests extending backward and forward in time, of the organism which uses the typewriter and which notes the written form of the word only as temporary focus in a vast and changing scene. (EEL, p. 6)

In this situation consciousness and perception are indelibly linked to practice, to action. We focus upon, become conscious of, perceive, those aspects of our multifarious experience which are relevant to the actions and deeds currently being undertaken. Dewey focuses upon the words on the paper in his typewriter so as to successfully complete the task he has before him. He knows what the words are that he is typing because he has selectively emphasised those aspects of his experience. He does not know that the light in his study is changing gradually, because this aspect of his experience is not the focus of his perception. The changing light could *become* the focus of his perception and he would then know, for example, that the light was fading.

reflection ... arises because of the appearance of incompatible factors within the empirical situation just pointed to ... Then opposed responses are provoked which cannot be taken simultaneously in overt action, and which accordingly can be dealt with ... only after they have been brought into a plan of organized action by means of analytic resolution and synthetic imaginative conspectus ... In other words, reflection appears as a

dominant trait of a situation when there is something seriously the matter, some trouble, due to active discordance, dissentiency, conflict among the factors of a prior non-intellectual experience; when a situation becomes tensional. (EEL, pp. 9-11)

That is, in everyday situations cognition is usually a secondary, fleeting phenomenon; however, in conflicting situations reflection and cognition become the dominant factors. Dewey is typing away, something is wrong, amiss. At this point reflection begins and possible solutions are proffered. The factors in these solutions are then analytically separated from the general context of experience and adjudged as to their capability of resolving the problematic situation: there has been a typographical error; a word has been omitted; words have been incorrectly transposed; the light is fading such that the words on the paper are becoming hard to read, and so on. Eventually, Dewey judges the light to be too dim and engages in overt actions to rectify the situation. In this situation, all aspects of the proposed solutions were in experience, but they were not known to be in experience until they were made the focus of consciousness and perception. As each alternative is considered the relevant factors in each alternative are discriminated, analysed, out of experience and judged as to their problem solving capacity.

Some may not find the above example convincing: in situations as described above a person does not seem to explicitly and methodically go through the various alternative solutions; prognosis and action are often virtually instantaneous. But for Dewey this is a case of illusion brought about by familiarity and consequent habit: in normal perceptual situations the significance and meaning of various aspects of experience are well entrenched in relation to everyday actions and activities.

In the situation which follows upon reflection, meanings are intrinsic; they have no instrumental or subservient office, because they have no office at all. They are as much qualities of the objects in the situation as are red and black, hard and soft, square and round. And every reflective experience adds new shades of such intrinsic qualities. In other words, while reflective knowing is instrumental to gaining control in a troubled situation (and thus has a practical or utilitarian force), it is also instrumental to the enrichment of the immediate significance of subsequent experiences. (EEL, p. 17)

Proposed solutions are often habitual: in previous experience it has been found that various problematic situations of certain types have certain circumscribed standard



solutions. Consequently, the process of inquiry is thereby truncated and automatic: we simply 'see' the problem and solution and act accordingly. However, if the usual, habitual solutions do not resolve the problematic situation, the process of inquiry becomes overt and explicit.

This idea of Dewey's of the incorporation, the 'funding', of the results of previous reflection and inquiry into the structure of experience, explains his assertion that Dewey's experience whilst sitting in his study typing extends indefinitely outwards in time and space. Our experience has enfolded within it the results, the residues, the accumulated inferential reflections of our life experience. All the implications and inferences we have available to us are potentially present in our ongoing experience. All that is needed to call out this latent potentiality is a problematic situation requiring reflection and inquiry. If something new is arrived at via the process of inquiry, this new signification, or meaning, is subsequently incorporated into our experience as an additional, possible, means to the future resolution of problematic situations: "these logical fixations become of the greatest assistance to subsequent inquiries; they are its working means. In such further uses, they get further tested, defined, and elaborated" (EEL, p. 19)

Empirical, perceptual experience is thus not only our access to an independent, evolving, inherently historical world, but it also constitutes our own evolving identity; our own being in the world. Consequently, returning to the particularity of empirical experience is a necessary condition of rationality. Given Feyerabend's historical-contextual approach to philosophy, and the importance which he places upon empirical experience, as shown in chapter's 3 and 4 above, this is a conclusion to which Feyerabend should readily give consent.

#### 6.4.3 Experiment and Testability.

In situations where reflection and inference are the dominant factor, the usually non-cognitive nature of experience is transformed into an explicitly cognitive, intellectual affair: "the material is entering into a new environment, and has been subjected to changes which will make it useful and effective in that environment." (EEL, p. 40) That

is, the data of inference has to be extracted and analysed out of non-reflective experience *before* it can fulfil its role in valid inference and inquiry. This abstraction is governed by the demands of the particular problematic situation, and always involves a previous generalisation: "A *generalization* in the form of a *hypothesis* is a *prerequisite* condition of selection and ordering of material *as facts*." (LTI, p. 498.)

In inquiry it is indeed the case that we make distinctions between fact and idea, data and theory, and so on. But these distinctions should not lead us to conclude that the 'facts' of a particular inquiry are indubitable: an unquestionable foundation of knowledge from which to procure more indubitable facts. Rather,

The conflicting situation inevitably polarizes or dichotomizes itself. There is somewhat which is untouched in the contention of incompatibles. There is something which remains secure, unquestioned. On the other hand, there are elements which are doubtful and precarious. This gives the framework of the general distribution of the field into 'facts,' the given, the presented, the Datum; and ideas, the *Quaesitum*, the conceived, the Inferential. (EEL, p. 137.)

Dewey goes on, commenting upon the unquestioned in any problematic situation, "this very element may be precarious, the ideal, and possibly fanciful of some other situation". (EEL, fn.1. p. 137.) That is, the polarisation, the dichotomisation, of experience into fact and theory is a contextual, pragmatic distinction, valid within the particular problematic situation where the distinctions are made, but not valid as an ontological characterisation of the universe.

In the logical process, the datum is not just external existence, and the idea mere psychological existence. Both are modes of existence- one of *given* existence, the other of *possible*, of inferred existence. And if the later is regarded, from the standpoint of the unified experience aimed at, as having only *possible* existence, the datum also is regarded as incomplete and unassured ... datum and ideatum are divisions of labor, co-operative instrumentalities, for economical dealing with the problem of the maintenance of the integrity of experience. (EEL pp. 139-40.)

The ideational, the theoretical, is that which is to be constructed, to be arrived at, it is inherently future oriented, and as such, because it is as yet not actualised, it is only possible, tentative, that which is not obvious or assured. Once some inference is validated or corroborated, through experimental testing, as being capable of returning

fractured experience into a unified whole, it functions in further explicit inquiry as the factual, the unproblematic. Of course, this provides no guarantee that the inference will remain as part of the unproblematic; a new situation may arise in which it may become questioned as capable of resolving some particular problem, it is then ideational and theoretical once again. Time may also place once bed-rock concepts into the pile of illusory concepts; for example, the apparent movement of the sun across the sky was once thought of as the unquestioned fact of the movement of the sun around the earth. With the advent of heliocentric models of the solar system this unproblematic fact was transformed into a theoretical, problematic idea; one in which the status of its validity was to be ascertained by experimental adjudication. After the acceptance of the heliocentric model of the solar system, the 'fact' of the movement of the sun across the sky still remained, but it was now a fact of the illusory affects that relative motion can produce in observers.<sup>15</sup>

This characterisation of the nature of, and the relation between, theory and fact, is reminiscent of Feyerabend's pragmatic theory of observation which was discussed in chapter 3. For Feyerabend, observation statements are those statements which an observer would unhesitatingly make in a situation: they are the unproblematic. Theoretical statements are those statements which are not obvious or apparent in the immediate situation. Thus it can be seen that Feyerabend's 'pragmatic' theory of observation is pragmatic in more than one sense. Dewey's account of theory and fact is also one which anticipates Shapere's account of observation.<sup>16</sup> Shapere contends that when scientists reported that they had observed the centre of the sun via neutrino experiments, they were using 'observed' in a functional sense, as that which was unproblematic in their experiments, as opposed to the interpretation of their results which were as yet 'theoretical', that is, problematic and under debate. It is obvious that this account is identical to Dewey's account. Post-positivist philosophy of science, instead of being new and original, is thus seen as being a return to ideas which were a

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<sup>15</sup> For the ideas in this section, see also LTI, pp. 105-111.

<sup>16</sup> See his *Reason and the Search for Knowledge*. For the solar neutrino experiments, see pp. 215-17, and Ch. 16.

common-place in pragmatic circles. Dewey supports what came to be known as the thesis of the theory-ladenness of observation; while taking the implications further in that knowledge, given the theory-ladenness of observation, becomes a product of mediated judgment: it is the end-product of inquiry rather than that with which we begin the inquiry.

The functional dichotomisation of non-cognitive experience, in situations where the inferential, reflective factors are dominant, into fact and theory, is not brought to a conclusion until action has taken place. One of the major themes of pragmatism is that ideas and theories must be connected to practice: ideas and theories have no import unless they are direct guides to action. Thus Dewey sees pragmatism as being a philosophy modelled around the idea of the experimental method: every action undertaken to achieve an end is conceived as an experiment as to the validity of the particular end chosen.

If by acting in accordance with the experimental definition of facts, viz., as obstacles and conditions, and the experimental definition of the end or intent, viz., as plan and method of action, a harmonized situation effectually presents itself, we have the adequate and the only conceivable verification of the intellectual factors. If the action indicated be carried out and the disordered or disturbed situation persists, then we have not merely confuted the tentative positions of intelligence, but we have in the very process of acting introduced new data and eliminated some of the old ones, and thus afforded an opportunity for the resurvey of the facts and the revision of the plan of action ... the rectification of intellectual content through acting upon it in good faith is the 'absolute' of knowledge, loyalty to which is the religion of intellect. (EEL, p. 241.)

We can see that the structure of inquiry which Dewey describes indelibly links experience, value, and experimentation. Ideas, concepts, theories, all act as conditions *to be* attained; they act as directives to action, goals to be achieved, and as such they possess a normative, purposive, teleological status. Concepts and theories are values, and are valued, because of their prospective, action-guiding nature as possible resolutions of problematic situations. Moreover, all proposed solutions must be tested in practice; action must follow; ends-in-view must be striven for. A possible solution is

worthless unless it guides concrete activity, and unless action to actualise the potential resolution is successful (or unsuccessful).

#### 6.4.4 Fruitfulness.

Recall that for Dewey inquiry is an inherently future oriented enterprise: alternative courses of action are proposed, they are then valued and judged and a particular end-in-view settled upon with respect to its comparative prospective ability to resolve the problematic situation. Thus, amongst other values, there is the overarching idea that a possible solution is just that, a *possible* solution: something which is as yet indeterminate as to its concrete ability to resolve the problematic situation, but which it is considered to be the best out of the alternatives available for guiding action, inference and research. That is, the end-in-view selected is selected upon the basis of its fruitfulness.<sup>17</sup> "The justification of ... [scientific] hypotheses has lain in their power to direct new orders of experimental observation and to open up new problems and new fields of subject-matter." (LTI, p. 519.)

This is not to say that science alone furnishes us with knowledge; that science alone is experimentally testable and fruitful.

There is no kind of inquiry which has a monopoly of the honorable title of knowledge. The engineer, the artist, the historian, the man of affairs attain knowledge in the degree they employ methods that enable them to solve the problems which develop in the subject-matter they are concerned with ... philosophy framed upon the pattern of experimental inquiry ... eliminates all invidious monopolies of the idea of science. (QC, p. 220.)

There are no invidious monopolies in that if our ideas and theories are put to test, and if our ideas and theories fruitfully go beyond that which is immediately present to that which is not immediately present, which for Dewey is the definition of inference, then the result attained is valid knowledge in the same manner in which science gives us knowledge.

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<sup>17</sup> With respect to this contention, Dewey would certainly applaud the sentiment of Chalmers position *vis a vis* his support of 'fertility'. See A.F. Chalmers, *What is This Thing Called Science?*, 2nd ed, pp. 125-32.

What distinguishes, for example, common-sense from science, is not that there is a different *type* of knowledge to be found in the two areas. Rather, what distinguishes common-sense from science is the relative fruitfulness of the ideas and concepts involved. Perceptual experience provides us with a large array of qualitatively dissimilar existents. In common-sense activities we abstract and generalise the perceptual, qualitative, dissimilarities into the everyday concepts of tables and chairs and so on. In so doing we are going beyond that which is immediately present, *this* unique perceptual experience, and inferring that which is not present, the general concept of 'table'. When we see dark clouds on the horizon, we may validly infer that there is a high probability of rain in the near future. We have moved inferentially from that which is given, the clouds on the horizon, to that which is not given, rain in the near future. Dewey would see such inference as being of a type identical to that of the scientist; however, the fruitfulness of these common-sense inferences are usually very weak. In common-sense inference the inferential chains are usually quite short, and, therefore, usually quite isolated. Science, on the other hand, is seen by Dewey as an exemplification of the abstraction from the immediately given, to a position where existence can be related in an ever more homogeneous and comprehensive manner. Water remains water in our qualitative experience; but the number of inferences we can draw from water, once the transition to  $H_2O$  has been effected, is enormous. As  $H_2O$ , the substance we know, qualitatively, as water, *is brought into systematic relationships with all other substances* via the chemical table of elements and sub-atomic theory. (QC, pp. 158-9.) We can see that with modern science we have moved further away from drawing distinctions upon the basis of qualitative differences. If a relatively small number of ideas can fruitfully be applied to the point where every substance can be connected to all other substances, then we can make a dizzying multitude of inferences as to action and behaviour, and thus further manipulate and control existence according to our values. Moreover, this fruitfulness of inference makes the possible testability of our ideas more and more ubiquitous.

#### 6.4.5 Comprehensiveness.

Dewey believes that reflection and inferential activity are dominant in problematic situations. These problematic situations are resolved by effecting a unified, unproblematic situation. As we saw in the previous section, pursuant upon this unity is the natural and inevitable extrapolation of successful unification to other situations. The ability of some particular result to be transferable, and successful, in other problematic situations is a constant driving force behind all inquiry. This is the defining characteristic which Dewey sees as exemplified in modern science: the heterogeneous and inherently unique, individual, nature of lived qualitative experience, is transformed in physical science, through mathematical analysis, into homogenous measures and instruments whereby inter-translatability of physical phenomena is effected. The qualitative aspects of water are replaced in science by the chemical formula of  $H_2O$ ; thus water, instead of being unique, unlike any other thing, becomes connected with all other physical phenomena by way of the chemical table of elements and the quantum theory.

Dewey adds to this idea of fruitfulness by pushing the idea of fruitfulness to its furthest extent: the ultimate fruitfulness of an idea or theory is that it can be developed into a comprehensive account of the universe; it can be developed into a fully-fledged world-view. For example, if we have two conceptions of ethics and human conduct, equally adequate as an account of ethics and behaviour, then the way in which we can test these ideas is to extrapolate the ideas in question into other areas of inquiry; that is, what consequences are produced when the ideas are applied to questions of justice, or of society in general? If the ideas in question can provide adequate accounts of these other areas of inquiry, or if they lead to problems when so extrapolated, we are then provided with additional information as to the worth of the ideas under investigation. This idea Dewey sees as exhaustively applicable: there are no boundaries where we stop the inferential processes; for example,

Just as the validity of a proposition in discourse, or of conceptual material generally, cannot be determined short of the consequences to which its functional use gives rise, so the sufficient warrant of a judgment as a claimant to knowledge ... cannot be determined apart from connection

with a widening circle of consequences ... The point involved comes out clearly when the social consequences of scientific conclusions invoke intensification of social conflicts. For these conflicts provide presumptive evidence of the insufficiency, or partiality, and incompleteness of conclusions as they stand. (LTI, p. 490.)

That is, if there is no ontological gulf between physical and social subject-matters, and all knowledge is concerned with consequences, then the fact that physical science produces social consequences is not some accidental by-product having no real connection with the subject-matter of physical science. The potential comprehensiveness of any intellectual tool cannot be known in thought: it must be continually applied to ever new circumstances so as to test its generality and define its valid scope and limitations.

in order that he [the scientist] keep his own balance, it is needed that his findings be everywhere applied. The more their application is confined within his own special calling, the less meaning do the conceptions possess, and the more exposed they are to error. The widest possible range of application is the means of the deepest verification. (EEL, pp. 441-2.)

And, one might add, of the deepest falsification and realisation of limitations.

Consequently, the social consequences of scientific theories constitutes a further indication of the suitability of the conceptions involved: they are a further test of the validity, the comprehensiveness, of the conceptions involved in the physical sciences.

Dewey sees knowledge as coming in many guises and forms: physical science has no monopoly on knowledge, and if it does contain knowledge, then it is justifiably testable and examinable via the consequences which it produces in ethical, social, legal, common-sense and any other spheres of existence experienceable and experienced by human beings. Conversely, knowledge derived from areas of human existence and experience, other than that of physical science, can be validly extrapolated into physical science, where it is tested according to the consequences which it implies for physical existence. A hypothetical example may make this point clearer. It can be cogently argued that the modern world-view arose as just that, an interconnected vision of the universe, man, and man's place in the universe. The atomic, corpuscular, and analytic science of Galileo and Newton was mirrored by the emergence of the modern liberal conception of the individual, and the subsequent conception of society commensurate with the nature



of liberal individuality. It is possible that someone at the time, dissatisfied with the individualist conception of society, and believing that humans are inherently interconnected, may have consequently considered the atomic conception of physical existence, providing as it does implicit support to liberal conceptions of the individual, a defective conception of physical reality. Instead, this individual may have been led to suppose, given their support for social interconnectedness, that physical existence is also inherently interconnected. The proposal of a wave theory of existence may have provided just such a basis for an alternative conception of existence commensurable with a social theory of interconnectedness, and antithetical to the individualist presuppositions of modern liberalism.<sup>18</sup>

In saying that we are always driven towards attempting to produce a comprehensive conception of the universe, Dewey does not believe that we will, in fact, arrive at such a conception. Complete comprehensiveness of world-view, and exhaustive translatability and transferability of results between situations, is something which Dewey considers to be inherently impossible. The temporal, developing, transforming nature of the world, and the inexpugnable individuality of qualitative lived experience, cannot be overcome.

in any object of primary experience there are always potentialities which are not explicit; any object that is overt is changed with possible consequences that are hidden; the most overt act has factors which are not explicit. Strain thought as far as we may and not all consequences can be foreseen or made an express or known part of reflection and decision.  
(EN, pp. 20-1.)

In this we can see an agreement with Kuhn. Kuhn contended that the articulation of a paradigm, produced via normal science, is the process whereby the limitations and weaknesses of a paradigm are able to be brought to light. I believe that Dewey would have agreed with this: it is only by searching out and testing the various consequences of

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<sup>18</sup> Personally, I wonder whether something like this was indeed the case: where did the idea of a wave theory come from? It is interesting to speculate that the wave conception evolved from a residual Aristotelianism which survived the scientific revolution. In this respect, not only is Aristotle's physics different from that of modern science, Aristotle's conceptions of the individual and society is radically at odds with the modern liberal conception.

an idea that enables us to ascertain the scope and limitation of that idea, thereby indicating where we can validly use those ideas in our ongoing inferential activities. The push for comprehensive world-views enables the widest possible testability, and, if successful, produces ideas and theories of the widest possible inferential validity.

### **6.5 Conclusion.**

In saying that in all inquiry these four aspects should be present, Dewey is not thereby endorsing a pre-existing value system. It is easy to see that these values need to be explicated in terms of a particular discipline before they can be validly applied. For example, the idea that all knowledge, to be valid knowledge, must, of necessity, be experimentally testable, differs in meaning according to discipline. The idea of experiment and test in physics is different to that which we find in economics, which differs again from the idea of test and experiment of moral/ethical ideas. Deliberately controlled experiments, of necessity in physical science, are evaluated in a wholly different manner in moral-ethical-social spheres: they are often unethical, immoral and socially destructive. What Dewey has in mind when he proposes that ethical phenomena should be experimentally testable, is simply the idea that ethical maxims should be considered to be hypotheses and judged, revised and revalued according to the consequences which ensue, rather than proposing some *a priori* ethical system.<sup>19</sup> Moreover, the overarching values are filled out by the discipline specific values that arise through specialised inquiry. In science, for example, the value of quantitative accuracy holds a position of great importance within the general value of testability, which it does not possess in other fields of inquiry: there is no place for quantitative accuracy in ethical inquiry. What the discipline specific values are will be dictated by the nature of the subject matter of the discipline in question, and by the results of ongoing inquiry.

For Dewey, it is a part of what it is to be rational for us to be led by pre-existing, accepted and successful ideas and conceptions, embodied in the traditions and forms-of-life in which we are located. It is also a part of what it is to be rational to be constantly

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<sup>19</sup> Cf. QC, pp. 277-8.

aware of the plethora of possibilities around us which may, if pursued, provide successful inference, prove fruitful and possibly comprehensive in application, and pass tests which such application engenders. The rationality of any assertion or position is only ascertainable within the context in which it is made: is the assertion given wide collateral support through connected ideas within the disciplinary matrix or practice of which it is part? Or, is the assertion capable of being developed into a far-ranging conception of reality? In both cases the assertion is to be checked against the specific experiential basis from which it is derived.

We have seen that Feyerabend can be interpreted as presenting a values-based, alternative, account of rationality, in contradistinction to what he saw as the dominant 'Rationalist' account of rationality. Feyerabend will have nothing to do with universalised, atemporal, context-independent, rules, methods or procedures. It is inherent in every situation requiring rational response that the response has, and should have, its own unique character. Consequently, Feyerabend sees science as "a complex medium containing surprising and unforeseen developments" and as such "demands complex procedures and defies analysis on the basis of rules which have been set up in advance and without regard to the ever-changing conditions of history". (AM, p. 18) According to Feyerabend an examination of the history of science shows us that

the business of science meets considerable difficulty when confronted with the results of historical research. We find then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. (AM, p. 23)

Points like this are not meant as absolute denials of method, but simply as denials of absolute method: no simple universal formula can do justice to actual science. One needs instead to look for better, more complex and historically informed accounts.

Feyerabend's alternative account is one in which the 'oughts' of scientific activity are derived from the particular situation in which a scientist is working: there are no universal, trans-situational prescriptions for the conduct of science. Every situation is a new situation where norms are not taken for granted, they are examined anew and decided upon *according to the exigencies of the particular situation*. Moreover, this

situation should be interpreted in terms of reflective equilibrium achieved amongst a set of, often conflicting, values.

It is my contention that if Feyerabend had followed this insight, then he should have supported a conception of general, overarching values of inquiry, like that which Dewey proposed. That Feyerabend did not present such an account can only be explained by supposing that Feyerabend felt any *general* account of rationality ran the danger of becoming yet another version of 'Rationalism'. But it seems to me that such fears are unwarranted. We saw above that these general values only receive specific content *within* the ongoing context of inquiry. But, more importantly, these overarching values can also be in conflict with each other; for example, if we reconstruct Feyerabend's Galilean case-study with an eye to Dewey's general values, then we can see that the fruitfulness of the concepts associated with the scientific revolution overrode the Aristotelian, comprehensive, considerations. Also, the testability of many of the assumptions of the new science were also in question: the general particulate cosmology was in many ways only testable in a promissory sense. All this is contrary to Popper's notions of the supreme importance of testability. Though Popper emphasised the importance of proposing highly general theories, which bring with them high degrees of falsifiability, this requirement was always subordinate to the dictates of testability. *Ad hoc* and auxiliary hypotheses were denigrated by Popper as reducing the testability of theories. But, as we saw, it was *ad hoc* hypotheses that preserved the *potential* comprehensiveness of the new science from being precipitously rejected. Therefore, questions of fruitfulness and comprehensiveness can, given the details of the specific problematic situation, override the value of testability. This is the sort of situation which was alluded to in section 5.2 concerning Feyerabend's values-based rationality; consequently, I think we can conclude that the overarching value structure implicit in Dewey's idea of inquiry and rationality, is, nevertheless, contextual and non-authoritarian, and should, therefore, be acceptable to Feyerabend.

## **7 Conclusion: Feyerabend and Values-Based Rationality.**

### **7.1 Introduction.**

After this detour through some of the philosophy of Dewey, it is time to return to Feyerabend and bring together the various strands of positive philosophy which we can find in Feyerabend. The project which follows is programmatic and sketchy, rather than completed and systematic; but there are three reasons for not regarding this as a weakness of the project. Firstly, this sketchiness reflects the nature of the philosophy in question: what systematicity I give to Feyerabend's ideas already goes beyond that which we find in Feyerabend. This is not to say that we cannot find most of these ideas in Feyerabend's writings: there are scattered here and there throughout his writings a number of references to metaphysical/cosmological assumptions, values and forms-of-life. However, these scattered references, along with lengthy passages concerning traditions and empirical experience, are not brought together by Feyerabend in any coherent manner. For example, Feyerabend's conception of 'traditions' is highly ambiguous, in that 'traditions' is used by Feyerabend as a generic term: it is applicable to any ongoing practice; the highest level explication being forms-of-life. Consequently, at one extreme, we have high level traditions, and on the other extreme, we have empirical experience. In Feyerabend's writings, the middle ground which connects these extremes is left obscure and ambiguously explicated. Thus, the concept of values and valuation fills this middle ground and brings the extremes together. I would want to go further than this and say that Dewey's high-level values of inquiry: the necessity to connect our ideas to empirical experience, fruitfulness, comprehensiveness and empirical testability, would give further credence and detail to Feyerabend's conceptions. The empirical thesis and testability, I believe, would have been acceptable to Feyerabend; however, fruitfulness and comprehensiveness may have seen him balk, in that they seem to encourage the abstract/theoretical attitude towards traditions, in preference to the empirical attitude.<sup>1</sup> I

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<sup>1</sup> It may also be said that acceptance of Dewey's values is leading us back to conceptions of novelty and content increase as proposed by Popper, Lakatos and others. But an important difference here is that

would want to say that this is not the case, and that Dewey's values can be seen as providing the synthesis of abstract/theoretical and historical/empirical attitudes which Feyerabend finds so agreeable about Aristotle. Nevertheless, I would not want to impute to Feyerabend more than was necessary to give his alternative conception of rationality credence and plausibility. Thus, the emphasis I place upon values, imported from Kuhn and Dewey, is not to be found in Feyerabend. But I think that this interpolation can be justified in that Feyerabend did make reference to values. Moreover, I think that this interpolation is the minimum necessary addition to Feyerabend's philosophy in order to present a plausible conception of rationality.

The second reason for the sketchiness of the positive account is that it is of the nature of this contextual approach that details cannot be given independently of the particularities of individual disciplines, practices and histories. This approach constitutes a framework from which to embark on extensive case studies of rationality, wherever and whenever rationality is thought to play a part.<sup>2</sup> It is thus a prospective research program, and too many details may rigidify it prematurely; any further explication should only be given *with* the results of in-depth case studies.

The third reason for the sketchiness of the positive account is that it is all that is necessary for the purposes of this thesis: my thesis is that Feyerabend is not an irrationalist. Rather, Feyerabend can be seen as moving towards an alternative conception of rationality. To achieve this purpose, I have tried to show that the alternative conception of rationality which we can find in his writings is feasible and plausible; I have not tried to give an exhaustive account of this alternative conception, or tried to answer all possible objections and criticisms.

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Dewey would have had no time for the semi-technical explication that these terms acquired in Popperianism, and which skewed the interpretation of the terms in a particular direction.

<sup>2</sup> It thus bears a family resemblance to the program Toulmin extolled in his *Uses of Argument*.

## 7.2 The Values-Based Alternative.

Recall that the general project of Feyerabend's philosophy was to move away from what Feyerabend called 'Rationalist' philosophy: positions which look for atemporal, universal, necessary, objective, rules of reason, which only need to be correctly applied to ensure that whatever we do or think is rational. Feyerabend believes that any such position can be reduced to absurdity, in that episodes in the history of science which 'Rationalists' label as rational, do not, in fact, exhibit a structure commensurate with the dictates of their own 'Rationalist' theses. Furthermore, Feyerabend argues that following the precepts of 'Rationalist' philosophies, in the scientific episodes in question, would have hindered the acceptance of important scientific theories, if not completely smothered their development. In this respect, much of Feyerabend's language of irrationalism, propaganda, subjective wishes, and the like, is of a purely rhetorical nature: this language forms part of the *reductio* of 'Rationalist' philosophies.<sup>3</sup> 'Rationalist' philosophies set up an either/or dichotomy: either science is rational and objective, or it is irrational and subjective. Given that Feyerabend believes that he has shown the inadequacy of 'Rationalist' theses to account for the rationality of seminal scientific episodes, the 'Rationalist' must accept that science is therefore irrational.<sup>4</sup>

If we can conclude that Feyerabend was not, in fact, supporting any form of irrationalism, then it can be further concluded that Feyerabend was proposing that the idea of what it is to be rational must be reconceptualised. I think this conclusion can be upheld; specifically, the vision of rationality which Feyerabend is led to is one which revolves around the idea of values.<sup>5</sup>

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<sup>3</sup> If one looks up 'rhetoric' in the index of AM, you will find that, along with the usual page references, the main reference is to pp. 1-309. That is, *the entire book* is referred to as rhetoric!

<sup>4</sup> The details of Feyerabend's *reductio* are given in chapters 1 and 2.

<sup>5</sup> I discussed Feyerabend on values in chapter 5. The further implications of what a values-based rationality requires was discussed in connection with Dewey's conception of values and valuation in chapter 6.

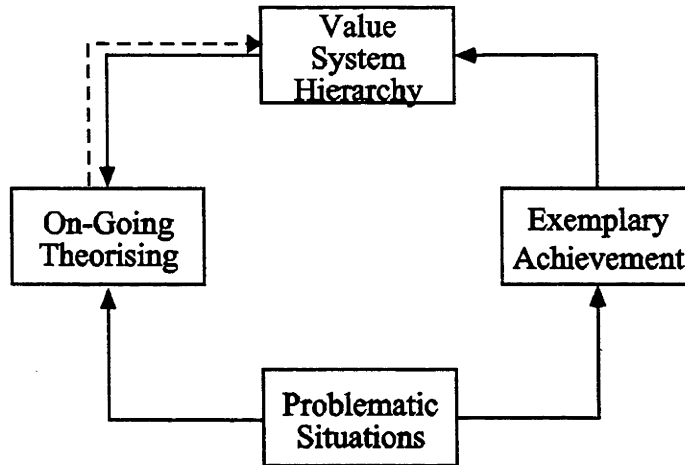


Fig. 7.1 Feyerabend's Rationality I: Values

The above diagram is meant to show that all situations requiring rational decision involve the application, balancing and re-valuation of values: a problematic situation arises, one which needs to be resolved, various alternative future resolutions of the problematic situation are proposed and these possible solutions are valued against each other, and with respect to a set of value traits deemed relevant to the situation at hand. The solution judged to be the best in the situation, the solution which it is believed will remove the tensions involved in the problematic situation, then acts as the goal to be achieved in the situation. If successful resolution of tensions and problems follows, then it is these ongoing exemplary achievements in any discipline or practice which, in their internal structure, embody and give content to the current values of that discipline or practice.

There are a number of factors which recommend such an approach to Feyerabend. Firstly, there are no independent, fixed values: values arise in contexts and have no application outside those contexts. Values are indelibly linked with the concrete exemplifications in which they are embodied. This does not mean that values are inherently transitory: various values can survive over lengthy periods of time. But it also does not mean that values remain the same: values are constantly evolving and can and do change with changing circumstances. Thus at any one time a hierarchy of values may



be delineable, but this hierarchy is not fixed and is adaptable in three ways. Firstly, specific circumstances may indicate that a temporary adjustment of the hierarchy may be necessary, without giving grounds for permanent adjustment. Secondly, a particular resolution of a problematic situation may give grounds for the permanent re-valuation of the value-system hierarchy. And thirdly, a particular resolution of a problematic situation may introduce wholly new values and/or drop other values from the value-system hierarchy. Consequently, a values-based approach to rationality is an inherently contextual, historical and dynamic vision of what it is to be rational. It alerts us to the situational specific character of rational decision-making, but also highlights the constantly moving, overcoming and transcending character of those decision-making processes.

But there remain some highly important factors which have to be taken into account. Feyerabend believes that it is imperative to take into account, in any theory of rationality, the metaphysical/cosmological background which informs the rules, principles and values to be found in such theories.

### **7.3 The Metaphysical/Cosmological Background.**

We saw in chapter 3 that Feyerabend's concept of incommensurability crucially revolves around the notion of a universal, non-instantial, or cosmological theory. Such a theory is one which contains a set of fundamental assumptions which (i) cannot be derived from any other assumptions in the theory. And (ii) says something about 'everything there is'. This second point emphasises that at the centre of the highest level scientific theories there lies a metaphysical world-view: a deep and far-ranging metaphysical picture of the nature of all existence.

In chapter 2 I also briefly discussed the idea that the rules and principles which we find in any particular theory of rationality are invariably based upon metaphysical/cosmological assumptions. In particular, the rational principles which we find in Lakatos's MSRP: content increase and the associated concept of novelty, are supportable *given* the metaphysical assumption of an infinite universe. Conversely,

content increase and novelty are not found among the rational principles of Aristotelianism: these ideas had no place within the finite metaphysical structure of the Aristotelian world-view. This idea Feyerabend sees as generally applicable:

every methodological rule is associated with cosmological assumptions, so that using the rule we take it for granted that the assumptions are correct. Naive falsificationism takes it for granted that the laws of nature are manifest and not hidden beneath disturbances of considerable magnitude. Empiricism takes it for granted that sense experience is a better mirror of the world than pure thought. Praise of argument takes it for granted that the artifices of Reason give better results than the unchecked play of our emotions. (AM, pp. 295-6.)

I think that this is a plausible position; for example, if we look at the values suggested as being operative in science: simplicity, quantitative accuracy, comprehensiveness and so on, there is certainly the metaphysical/cosmological assumption that these ideas are applicable to reality, because the world itself has such attributes. I don't think that Feyerabend is here denying that these values are supported because of their empirical success. But whatever empirical success they may have garnered, it is never enough to prove that the world is indeed as the values portray it to be. The status of simplicity is a case in point: why should the deep structure of the world be governed by as few laws and principles as possible? Granted, there have been many striking instances of parsimonious scientific theories; but there has also been just as many highly complex and profligate scientific theories. It could be argued that the simplicity of a scientific theory in one domain is balanced by the complexity of linking this theory with theories in other scientific domains. Be that as it may, the point to be made is that the empirical success of the value of simplicity is a moot point, and, consequently, the acceptability of simplicity can be questioned. We can see here why values such as simplicity are values: they are heuristic, prospective promises of success; they are the goals to be attained. As such they are only forceful if scientists are *committed* to them and guide their research accordingly; regardless of setbacks and difficulties.

Feyerabend's position can be schematically summarised as follows:

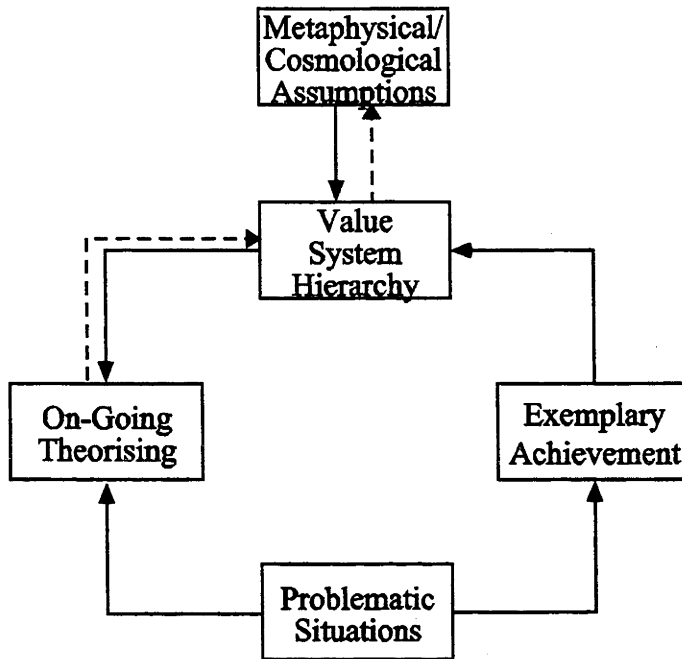


Fig. 7.2 Feyerabend's Rationality II: Metaphysics

Thus we can see that it is the largely tacit, covert, metaphysical/cosmological assumptions which ultimately give context to values and which connect the values of any one discipline or practice with the values of other disciplines and practices. In saying this I am not contradicting what was said before concerning the idea that values have no independent existence outside of the concrete embodiment of those same values in exemplary achievements. Metaphysical/cosmological assumptions are just as much embodied in concrete exemplary achievements as are the lower level values. In fact, the best way to conceive of these assumptions is as the highest level, most deeply entrenched values: they are the most stable and enduring aspects of the system. As such, they do not escape the feedback loop and can be transformed by significant exemplary achievements, as the arrow from the value-system to the metaphysical/cosmological assumptions signifies in the diagram.

#### **7.4 Realism, Observation and Rationality.**

At this point in the discussion I want to reintroduce some of the conclusions reached in chapters 3 and 4 concerning Feyerabend's pragmatic theory of observation and his attitude towards experience and perception. Feyerabend believes that in all situations we take the world to be as our theoretical commitments and beliefs say it is: we do not question this assumption, we simply live it. Our perceptions are thus theory-laden, in that we take our perceptions as being caused by, and as indications of, the world as we conceive it: conception and perception are inextricably intertwined. In this situation, given that all statements are theoretical, the distinction between observation statements and theoretical statements is a pragmatic distinction. Observation statements are those statements we are disposed to utter in direct response to perceptual stimuli; they are delineated in terms of possible ostensive reference. I concluded in chapter 3 that this pragmatic theory of observation is, at least, minimally realist: the world has a nature and it would have a nature even if humans were not there to perceive it. In addition, I concluded that Feyerabend's pragmatic theory of observation implies that there is, at least, a minimal connectivity between humans and the world via perception: the world, or parts of the world, are the cause and source of the perceptions which we have.

The importance of the preceding discussion can be brought out if we consider figure 7.3 below. There are two points to be gathered from the diagram. Firstly, the 'theory' involved in the theory-ladenness of observation is comprised of the entire system of commitment and belief. Secondly, and more importantly, experience and perception are the driving force behind Feyerabend's conceptions of belief change and rationality. It is by attending to the multiplicity of experience and perception, which thus places our preconceptions and theories in juxtaposition to experience, that forces conflict and problematic situations, which itself drives the engine of valuation, judgment and reflection.

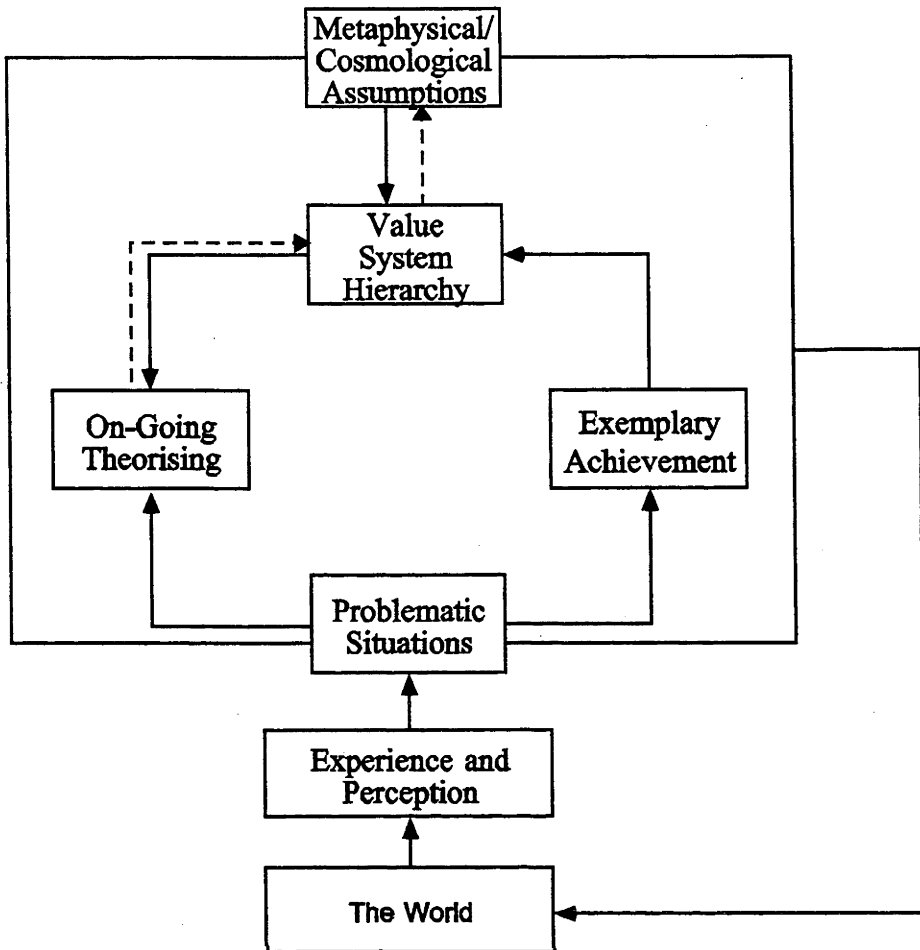


Fig. 7.3 Feyerabend's Rationality III: Realism and Perception.

In chapter 4 we saw that Feyerabend criticised what he called theoretical/abstract traditions and extolled the virtues of what he called empirical traditions. Empirical traditions emphasise the particularity, multiplicity and situational dependent character of experience and knowledge; whilst theoretical/abstract traditions are engaged in the attempt to reduce this abundance of empirical perspectives in favour of generalised, universalised, accounts of reality-as-a-whole. Recall that Feyerabend has no in-principle objection to abstract traditions: the results obtained in such traditions give valid knowledge. But the knowledge obtained in such traditions is not universal knowledge: it is knowledge of particular circumstances, just as the knowledge derived through empirical traditions is knowledge of particular circumstances. Now the question may be

asked, if we are to turn to the particularity of experience and perception, so as to provide us with more knowledge, are we then returning to the idea that observation is not theory-laden and that unadulterated perception is the source and ground of all our knowledge? And is this then a contradiction in Feyerabend's writings? I think we can conclude that Feyerabend is indeed an empiricist: perception *is* the source of all knowledge. But there is no denial here that observation is always theory-laden, we cannot break out of that particular circle, and we cannot see things from a neutral, pure position. But the fact that we cannot do so in no way implies that we cannot see discrepancies, disparities and differences. Feyerabend's point is that it is by attending to the situational specific characteristics of our experience and perception, that enables us to uncover the discrepancies, disparities and differences denied in abstract, generalised notions. Feyerabend, in his later writings, is not moving away from the idea of the theory-ladenness of observation; rather, Feyerabend is *reinforcing the idea of realism*. The world is indeed independent, and its independence is manifested in the fact that the abundance, multiplicity and profusion of our experience and perception contain indications of the multitudinous aspects of the world.

### **7.5 The Widening Circle of Rationality.**

In chapter 1 I discussed Feyerabend's *reductio* of 'Rationalist' philosophies in terms of his analysis of Copernicus and Galileo. We saw there that one of the major criticisms of 'Rationalist' interpretations of the Copernican revolution, according to Feyerabend, is the illegitimate selective emphasis given by these philosophies to particular aspects of the problematic situation at the time, at the expense of other relevant considerations. Thus, the accepted Aristotelian world-view was just that, a world-view: it provided a more or less systematic and interrelated vision of all phenomenon. In the Aristotelian system cosmology, physics, physiology, psychology, perception and observation, sociology, theology, ethics and any number of other disciplines and ideas were all loosely interrelated and gave mutual support to each other as descriptions of reality. In comparison to the Aristotelian world-view, the original

Copernican heliocentric hypothesis, though exhibiting some strengths in astronomy, nevertheless ran into a number of anomalies and difficulties which showed that it was no better than the Ptolemaic model it was supposed to supplant. Moreover, the Copernican hypothesis did violence to the accepted views in cosmology, physics and theology, and Copernicus offered no alternative conceptions in these areas which would be compatible with his hypothesis. When Galileo enters the fray we find that he defused some of the arguments against Copernicanism by proposing a new dynamical physics. But these new arguments themselves clashed with deep-seated Aristotelian conceptions of perception and in no way redressed the other incompatibilities of the Copernican hypothesis with the accepted Aristotelian world-view. Even the evidence of the telescope was highly debatable at the time: the auxiliary sciences necessary for substantiation of telescopic images did not exist; that is, no one knew how the telescope worked. Given these considerations pertaining to the overall evidential situation at the time, Feyerabend contends that empiricists, Popperians and other 'Rationalists', should have labelled the continued support for the new science as irrational: the new science should have been rejected.

Given the situation as described, how, according to Feyerabend, did the new science rise to dominance? The answer is that Feyerabend does not conceive science as being independent of non-scientific disciplines and practices: science is always part of a wider form-of-life, a wider world-view. The scientific revolution gained support from, and found receptive audiences in, a variety of individuals and groups who felt a growing dissatisfaction with the prevailing world-view. The work of Galileo provided a rallying point, a concrete achievement around which a burgeoning new world-view could gain leeway in which to overcome the entrenched social and political orders of the time. The rising middle-class, the reformation, the discovery of America and the corresponding idea that there may be an 'America of knowledge', the incessant political power struggles across Europe, as evidenced in the Thirty Years War, and many other factors militating against the prevailing world-view, created the circumstances which allowed the new science to flourish despite an original paucity of theoretical underpinning.

Thus, we can say that one of the main posits of Feyerabend's philosophy is a rejection, contra Lakatos and others, of 'internal' reconstruction's of the history of science and other rational disciplines. Feyerabend sees science as a complex historical process; therefore, attribution's of rationality to historical agents need to be informed by a general knowledge of the historical period in question. The historical, cultural, social, metaphysical and religious attitudes of the times encroach upon, and affect, the decision making processes of the historical agents: rational decisions are not made in an objective vacuum free from influence. This is not just a regrettable tendency which can be avoided if only we are more assiduously rational and dispel all extraneous material from rational debate. Rather, it is the ground and condition from which all opinions come, and from which mutual collateral support is gathered.

The existence of groups of anti-Aristotelians and of other enemies of school philosophers was necessary for turning such subjective acts [of Galileo's] into a more comprehensive social phenomenon and, finally, into the elements of a new science. Concentrating on the internal history of Copernicanism we notice an increase of content (Galileo's observations) ... But adding the external history ... to our information we notice *that the agreement 'inside' science is the result of numerous violations 'outside' of it*, we realize that these violations were necessary for the transition ... *and that they therefore belong to science itself* (AM, p. 211.)

Without the historical context, without the placing of new ideas within burgeoning world-views, new ideas strike no responsive chord. Consequently, it is rational to consider the life-world of an historical agent, in fact, it seems impossible for any person to separate themselves completely from the historical context of their existence. To try and explicate a scientific episode on purely internal desiderata is irrational.

In this respect, Feyerabend can be seen as wanting to dissolve the dichotomy of internal and external history. In this he is following the general trend of his entire philosophy: Feyerabend likes to destroy naive dichotomies. There is no clear and unambiguous distinction between that which is rational in science - internal- and that which is irrational in science -external. Putative 'external' factors are integral to the rationality of science; while putative 'internal' factors, applied universally, would destroy the rationality of science. Thus, it should be noted that in this dichotomy busting



Feyerabend doesn't jettison the substantive content of these dichotomies. For example, when Feyerabend dissolves the theory/observation dichotomy, he still keeps the idea of theory and the idea of observation. But these ideas function as a pragmatic distinction, a distinction which, at any one time, in any one context, can be made to help conceptualise the situation you find yourself in, and to separate problematic from unproblematic, and so on. The point is that there is no actual line where theory ends and observation starts. Similarly, Feyerabend, in discussing the context of discovery/context of justification dichotomy, itself intimately linked with the internal/external dichotomy, states that "we are dealing with a single uniform domain of procedures all of which are equally important for the growth of science". ( AM, p. 167) Justification procedures are a part of discovery, and discovery procedures and insights are part of justification. The situation is the same with respect to the rational-internal, irrational-external dichotomy: what was said to be rational and what was said to be irrational both still exist, it is just that they are indissolubly united. No unqualified judgments of rationality can be made, in that we have to give the context in which we are making the judgment: in different contexts one and the same action, or thought, may be both rational and irrational.

It would be a misunderstanding to interpret this position as supporting some sort of radical sociology of knowledge: that our ideas are caused by, and can be reduced to, the social and cultural surroundings in which we are placed. Feyerabend does not contend that any one aspect of existence has primacy over other aspects such that we must engage in reductionistic exercises. Rather, all aspects of existence should be considered to be on the same level. Thus, Feyerabend does not deny, for example, that Galileo had *arguments* for his position. The point Feyerabend is making is that the arguments of Galileo are but one part of a complex historical context, the other aspects of which were just as important for the eventual triumph of the new world-view. The following diagram gives a schematic representation of the situation Feyerabend sees as operative in historical contexts.

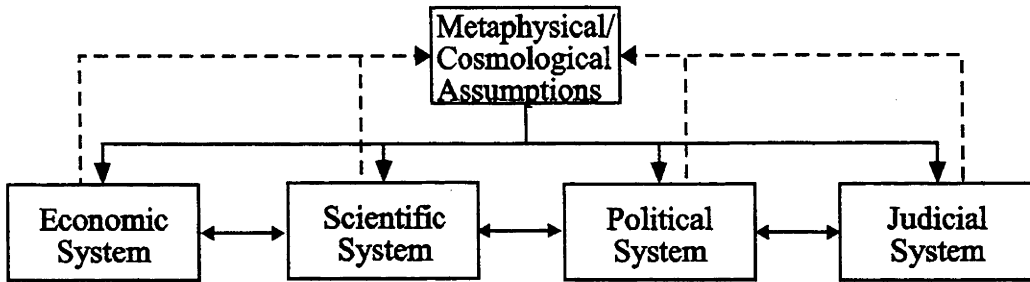


Fig. 7.4. Feyerabend's Rationality IV: Forms-of-Life.

We see in this diagram the idea that the various discipline/value systems are interconnected and gain mutual support as part of wider forms-of-life and world-views. This happens in two ways. Firstly, the interconnectedness of the various systems can be seen to arise through the tacit and covert sharing of deep-seated cosmological assumptions. Secondly, there is overt interaction between the systems; for example, the existence of science, as we know it, is dependent upon political and economic factors. These value-systems, which it is supposed that there are an indefinite, vague number, are all connected with the world via the individual experiences to which they refer. It is also implied that the various systems are meant to provide a systematic and comprehensive picture of all aspects of reality. This comprehensiveness is, however, never in fact accomplished: there is no axiomatic, systematic, tightness between the various systems; nor do the systems exhibit an exhaustiveness of explication. The constraints placed upon any one system, by another system, varies with the systems in question. And the constraints that are supplied are constraints of boundary conditions, not of any logical implication of content. For example, given the sort of discrete, atomistic, metaphysics, epistemology and psychology propounded by Hobbes, Locke and Hume, the sort of ethical and social theories that those thinkers see as possible fall within a certain range: there is no place in these conceptions of the universe for an Aristotelian ethics. But this then does not imply that the British empiricists are then committed, by way of logical

necessity, to supporting one particular ethical or social theory. All that they are committed to are ethical and social theories of a certain family resemblance.<sup>6</sup>

### **7.6 Epistemological Anarchism and Forms-of-Life.**

We saw in chapter 2 that Feyerabend's epistemological anarchism is not really anarchism at all: "The one thing he [the epistemological anarchist] opposes positively and absolutely are universal standards, universal laws, universal ideas such as 'Truth', 'Reason', 'Justice', 'Love' and the behaviour they bring along" (AM, p. 189.) What Feyerabend means by anarchism is the relatively uncontroversial idea of not being guided, forced or determined by *universal* standards. This does not imply that there are no standards, or that Feyerabend's anarchism is identical to chaos and complete disorder.

Feyerabend also distinguished epistemological anarchism from scepticism. (AM, p. 189) Feyerabend regards scepticism as the idea that no theory is better or worse than any other theory. Epistemological anarchism, on the other hand, is the idea that theories *can* be better or worse. The evaluation of theories being determined according to their ability to enhance or hinder the aims and motives of the epistemological anarchist. Does this leave us in an irredeemable subjectivistic attitude towards theories? I don't think that it does. That prognosis may be correct if the aims and motives could be any old arbitrary aims or motives. But this is not how Feyerabend approaches the matter: "it is the *sceptic* who cannot assert things positively; the anarchist can assert anything he wants and often will assert absurd things in the hope that this will lead to new forms of life" (SFS, fn.2. p. 210.) That is, we see here additional support for the idea that Feyerabend wants to restore the idea that it is entire forms-of-life, incorporating a comprehensive account of man and the universe, which is the ultimate basis from which we should judge arguments, and which is the desired goal toward which theorising is directed. In this context Feyerabend contends that "happiness and the full development of the individual human

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<sup>6</sup> Cf. for example, A. MacIntyre, *Whose Justice? Which Rationality?*, wherein MacIntyre argues that conceptions of justice are indelibly linked to conceptions of practical rationality. If the idea of values-based rationality is correct, then the distinction between theoretical and practical rationality cannot be upheld. We then have a situation wherein questions of scientific rationality have a bearing on questions of justice, and vice versa.

being is now as ever the highest possible value."<sup>7</sup> Thus, he commends J.S. Mill and Aristotle for creating theories which are meant to be comprehensive accounts of man and the universe, including the ways in which happiness and full personal development can be achieved.

We see here the idea that it is not only the case that judgments of rationality must be made with the full historical context in mind, and that actions and thoughts must be placed within the form-of-life in which they happen, but there is also the further idea that there are always a multiplicity of actual and possible forms-of-life vying for allegiance. Consequently, Feyerabend applauds Mill's model of reasoning where there is a competition of alternatives:

Now this competition is not only between ideas, standards, rules, but between fully fledged *forms of life*; and it is supposed to affect not only ideas, but also feelings, intuitions, attitudes, actions, the imagination, in a word - it is supposed to affect one's whole existence ... Mill realises that ideas have not just logical but also psychological, sociological and other aspects and must be judged by all of them.<sup>8</sup>

What all this implies is that the epistemological anarchist uses theories to achieve their aims, and *the ultimate aim and motivation is the construction or defence of their preferred form-of-life*. They may want to destroy some theory which they perceive as being destructive of their preferred form of life, and they will adopt any means at their disposal to achieve this goal. Particular theories and particular circumstances are not the sort of thing that an epistemological anarchist would think of defending: the epistemological anarchist's focus is on the bigger picture of forms-of-life.

In saying this we must understand Feyerabend as saying that any aspect of existence can provide the launching pad for the explication of all existence. We are not constrained to begin with science, or to leave science sacrosanct: there are no privileged bases from which to begin. Just as in previous centuries it was considered necessary for all ideas to be consistent with theology and religion, so too in recent centuries it has been

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<sup>7</sup> Feyerabend, "Consolations for the Specialist" p. 210.

<sup>8</sup> Feyerabend, "In Defence of Aristotle" p. 169. That this is one of the major themes of Feyerabend's philosophy should be obvious to anyone who has read AM.

considered necessary for all ideas to be consistent with the findings of science. However, just as the theological presuppositions were overthrown, so too the current scientific presuppositions may be overthrown. Consequently, Feyerabend contends that one of the starting points may be the idea of the good and happy life: what it is believed constitutes a good life may provoke the demand for a new social, political and economic order, which may then produce new scientific research programs, which may then transform the deeply entrenched scientific world-view. For Feyerabend, it is necessary to explore as many different forms-of-life as possible so as to enhance our knowledge of the world.

### **7.7 Proliferation and the General Values of Inquiry.**

We can see that one of the recurring themes in this vision of rationality is the idea of proliferation and plurality of ideas, systems and forms-of-life. We saw in chapter 4 that Feyerabend follows J.S.Mill on this and gives five reasons for supporting proliferation. Firstly, we are fallible creatures and, therefore, a view we may want to reject may in fact be true. Secondly, all views usually contain some truth, consequently, it is only through comparing and contrasting disparate views that we have a chance of integrating and finding all truths. Thirdly, uncontested views, even if wholly true, become dogmatically accepted without an understanding of the rational grounds of their acceptance. Fourthly, it is argued that meaning is something only fully grasped in terms of contrasts. And fifthly, in relation to the second point above, it is often only by the proposal of alternative views that evidence against a dominant view can be discovered.

This idea, the so-called 'principle of proliferation', has as a logical consequence what Feyerabend has called the 'principle of tenacity': "[the] advice to select from a number of theories the one that promises the most fruitful results, and to stick to this one theory even if the actual difficulties it encounters are considerable."<sup>9</sup> That is, if we are to have proliferation, some people must hold onto and develop alternative theories in the face of counter-evidence and anomalies. These two ideas are important for understanding Feyerabend's conception of rationality:

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<sup>9</sup> Feyerabend, "Consolations for the Specialists" in *Phil. Pap. Vol.2.* p. 137.

a science that tries to develop our ideas and that uses rational means for the elimination of even the most fundamental conjectures must use a principle of tenacity together with a principle of proliferation. It must be allowed to *retain* ideas in the face of difficulties; and it must be allowed to introduce *new* ideas even if the popular ideas should appear to be fully justified and without blemish.<sup>10</sup>

Thus, if we look at Feyerabend's conceptions, as summarised in the preceding sections, we find proliferation at all levels of analysis. If we step back from preconceived notions we find that empirical experience is characterised by multiplicity and abundance; a proliferation susceptible of many and varied interpretations. When we consider rationality as characterised by valuation and judgment, we again see that the very idea is characterised by plurality: it is only valuation and judgment when there are incompatible alternatives. We have also seen that the deep-seated metaphysical/cosmological assumptions underlying forms-of-life can be developed and explicated in a variety of incompatible manners; indeed, this looseness can be one of the mechanisms by which wholly different forms-of-life develop. And finally, Feyerabend sees the proliferation of forms-of-life as essential to avoid dogmatism and ensure development. Of course, as a necessary means for achieving this proliferation, Feyerabend contends that individuals, groups and institutions must tenaciously hold on to their ideas and forms-of-life, despite apparent inconsistency with established and entrenched ideas and world-views.

This idea of Feyerabend's must not be interpreted as saying that any old crack-pot theory should be endorsed: the proliferation of ideas, theories and forms-of-life are meant to to be seriously proposed and seriously defended. Proliferation is designed to provide contrasts and tests, consequently,

The distinction [between 'respectable' people and cranks] does not lie in the fact that the former suggest what is plausible and promises success, whereas the latter suggest what is implausible, absurd, and bound to fail. ... the distinction between the crank and the respectable thinker lies in the research that is done once a certain point of view is adopted. The crank usually is content with defending the point of view in its original, undeveloped, metaphysical form, and he is not at all prepared to test its usefulness in all those cases which seem to favor the opponent, or even admit that there exists a problem. It is this further investigation, the details

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<sup>10</sup> *Ibid.*, p. 143.

of it, the knowledge of the difficulties, of the general state of knowledge, the recognition of objections, which distinguishes the 'respectable thinker' from the crank. The original content of his theory does not. If he thinks that Aristotle should be given a further chance, let him do it and wait for the results. If he rests content with this assertion and does not start elaborating a new dynamics, if he is unfamiliar with the initial difficulties of his position, then the matter is of no further interest. However, if he does not rest content with Aristotelianism in the form in which it exists today but tries to adapt it to the present situation in astronomy, physics, and micro-physics, making new suggestions, looking at old problems from a new point of view, then be grateful that there is at least somebody who has unusual ideas and do not try to stop him in advance with irrelevant and misguided arguments.<sup>11</sup>

It may be objected that the sentiments expressed in this passage, coming as they do from an early paper, were later rejected by Feyerabend. The later Feyerabend was notorious for saying that witchcraft, astrology and other such ideas should be defended. However, the statements that we find supporting witchcraft and astrology should be understood as partly derived from his *reductio*, and partly derived from the contentions expressed in the above passage. These ideas probably contain some truth, and may, if developed seriously, provide alternative frameworks for understanding the world. For example, when Feyerabend defended astrology in SFS (pp. 91-6) against recent attacks, what abhorred Feyerabend was the uninformed nature of the criticisms. Firstly, many people who criticise astrology do not know astrology: they have not done the requisite in-depth studies into the subject-matter so as to make their criticisms pertinent and decisive. Commenting on a series of papers criticising astrology, written by scientists, Feyerabend states that

what surprises the reader whose image of science has been formed by the customary eulogies which emphasize rationality, objectivity, impartiality and so on is the religious tone of the document, the illiteracy of the 'arguments' and the authoritarian manner in which the arguments are being presented. The learned gentlemen have strong convictions, they use their authority to spread these convictions ... they know a few phrases which sound like arguments, but they certainly do not know what they are talking about. (SFS p. 91.)

This is not to say that an in-depth study would vindicate astrology. But an in-depth study is a necessary condition for objectivity, rational comparison and impartiality.

<sup>11</sup> Feyerabend, "Realism and Instrumentalism" p. 305.

Secondly, the criticisms are uninformed because they usually disregard *scientific* research which seems to support general astrological concepts: "the assumption that celestial events such as the position of the planets, of the moon, of the sun influence human affairs." (SFS, p. 93.) Feyerabend gives references to planetary plasmas and their effect upon solar activity; the effect of solar activity, and therefore, of planetary position, upon organic life; the possible effect of solar activity and planetary position upon chemical reactions involving water; and the sensitivity of plants and animals to the lunar cycle. (SFS, pp. 93-4.) These are all examples of scientific research which give tentative support to *some* of the tenets of astrology. However, these examples of scientific research are not intended to be vindications of astrology. They are presented by Feyerabend to show that there is some *scientific* research which makes the all too ready rejection of astrology at least a little gray.

Feyerabend's presentation of scientific research which could be interpreted as giving support to some of the tenets of astrology should alert the reader that Feyerabend is not defending astrology as it is popularly known:

The remarks should not be interpreted as an attempt to defend astrology *as it is practiced now* by the great majority of astrologists. Modern astrology is in many respects similar to early mediaeval astronomy: it inherited interesting and profound ideas, but it distorted them, and replaced them by caricatures more adapted to the limited understanding of its practitioners. The caricatures are not used for research; there is no attempt to proceed into new domains and to enlarge our knowledge of extra-terrestrial influences; they simply serve as a reservoir of naive rules and phrases suited to impress the ignorant. (SFS, p. 96.)

This is a very important passage. If we examine the ideas involved here: astrology as a stagnant research program; as a caricature not used for research; as not expanding into new domains, then we can clearly see that Feyerabend's principle of proliferation does not imply that any crack-pot theory should be supported or pursued. Feyerabend places conditions upon the pursuit of alternatives.

We can see here that Feyerabend seems to implicitly support a conception of rationality involving something like Dewey's general values of inquiry. Ideas and theories should be taken seriously if (i) they are linked to experience. (ii) They are experimentally



testable. (iii) They can fruitfully guide further research. And (iv) They show promise as being comprehensively applied. If we examine astrology as it is now, then we find that it is very difficult to link various aspects of the theory to concrete empirical experience. The experimental testability of the theory is highly tenuous; those aspects which do admit of test have not proved very successful. The fruitfulness of popular astrology, its ability to explicate the multitudinous aspects of psychology and behaviour, is rather poor. And the possible comprehensiveness of astrology is difficult to foresee: can it move out of its professed field of application, personality and psychology, and give insights into the nature of justice, ethics, history and so on? Consequently, with Feyerabend, we can *unequivocally* reject astrology, *as it is now practiced*, as having any rational merit.

But the important point to be made in relation to Feyerabend's principle of proliferation is that this judgment is a contextual one: it applies to astrology as it is *now* practiced. Feyerabend does not want to reject *any* theory universally, or atemporally. Consequently, Feyerabend leaves open the, admittedly slim, *possibility* that some researcher(s) will change some of the objectionable tenets of astrology, thereby dissolving the glaring internal inconsistencies, and propose additional premises which are testable, fruitful, and possibly comprehensively applicable. If this situation did arise, and this new astrology became a progressive research program, then astrology should be taken seriously as an alternative conception and not ruled out of consideration *a priori*. Ideas can come from anywhere and ideas can be pursued, extrapolated, and turned into fully-fledged research programs, if not forms-of-life.<sup>12</sup>

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<sup>12</sup> An objection may be raised to the effect that if astrology could be resurrected in the manner outlined, then it would no longer be astrology; it would be science. Therefore, it is not that alternatives to science must be taken seriously, for they may overthrow science; rather, alternatives are handy for providing ideas for science to appropriate. In one sense I don't find this to be an objection at all: 'science' is the honorific title we give to that body of ideas and theories which we believe to be the best, most rationally supportable, ideas and theories available. Consequently, if a new astrology became accepted, then it would be science, by definition. It would be science in the same sense in which there was Aristotelian science. This last idea also brings to light the objectionable feature of the objection: a new 'science', wherever it may come from, may be as different from modern science as modern science is from Aristotelian science. Proliferation of alternatives may only provide ideas to be appropriated by science; but proliferation of alternatives may also throw up a radical alternative, which may change what it is to be science.

Some critics may say that my emphasis upon proliferation as characterising Feyerabend's conception of rationality is wrong, in that Feyerabend himself denied such an ascription. For example, Feyerabend states that

One might ... get the impression that I recommend a new methodology which replaces induction by counterinduction and uses a multiplicity of theories, metaphysical views, fairy-tales instead of the customary pair theory/observation. This impression would certainly be mistaken. My intention is not to replace one set of general rules by another such set: my intention is, rather, to convince the reader that *all methodologies, even the most obvious ones, have their limits*. (AM, 3rd ed. p. 23.)

But I don't think that this statement goes against my interpretation of Feyerabend. Feyerabend is here denying that the principle of proliferation should be set up as a universal, atemporal, necessary standard of what it is to be rational; that would indeed go against the entire push of Feyerabend's philosophy. Rather, we must interpret Feyerabend as saying that if we want our knowledge to evolve, and if we want an ever widening sphere of experience to become comprehensively rationally explicable, then the best highest level conception that we currently have to achieve these goals can be found in the idea of proliferation. This is not to say that the principle of proliferation is necessary for rationality, for, in agreement with Feyerabend's contention that rational standards themselves evolve and change over time, we may find in the future that some as yet undreamt of rational ideal will furnish us with an even better high level concept of what it is to be rational. One of the *strengths* of this approach is its amenability to meta-philosophical explication: the ideas involved are meant to be applicable to itself. Proliferation of concepts incompatible with this conception of rationality is welcome, and the issue is to be adjudged experimentally via the plausibility and cogency of the case studies offered. The whole conception may also be undermined by scientific research, or by spectacular advances by some other form-of-life, in that we may come across some one true theory of everything which may make the plea for proliferation somewhat weakened in strength. If we add Dewey's high level values, then, as Feyerabend has pointed out, the one true theory of everything may necessitate the dropping of the value of fruitfulness, and so on.

## 7.8 Conclusion.

The content of this thesis has been mainly negative, in that the first four chapters dealt with rescuing Feyerabend's philosophy from misunderstandings. Specifically, it has been said that Feyerabend contends that there is no such thing as rationality; there is only power play, prejudice and propaganda. It has also been claimed that Feyerabend's support for epistemological anarchism should be interpreted literally: do what you like and defend what you like, because no theory is better than any other. As evidence for this claim various philosophers have contended that (i) Feyerabend's incommensurability thesis implies that no two theories can be compared in any manner whatsoever. And (ii) The related contention that Feyerabend's ideas constitute a radical, self-refuting, relativism.

I have argued in chapters 1-4 that this interpretation of Feyerabend is wrong in all four claims. In particular, I have argued that Feyerabend is conducting a *reductio ad absurdum* of 'Rationalist' philosophies. Once we read Feyerabend with this in mind, attributions of irrationality and literal anarchism cannot be supported. Feyerabend's incommensurability thesis should also be read with his *reductio* in mind, with particular reference to logicist conceptions of theoretical comparison. That is, Feyerabend never denied that theories could be compared. And though Feyerabend was a relativist in many respects, his relativism was not of the radical, self-refuting, kind. In fact, Feyerabend's philosophy was highly realistic in many respects.

In chapters 5-7 I tied together the various strands of Feyerabend's philosophy to produce a positive conception of rationality. This positive conception of rationality is a historical, contextual and dynamic vision, crucially revolving around the ideas of value and plurality. In presenting this conception of rationality, the ideas of Kuhn and Dewey, with specific reference to values-based rationality, have been incorporated into Feyerabend's account. This was seen to be the minimum necessary addition to Feyerabend's ideas, producing a detailed and comprehensive heuristic program for the analysis and understanding of rationality.

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