A Pragmatic Theory of Truth and Ontology

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* I use upper case roman numerals 'I' or 'II' where a chapter is divided into two parts; I use arabic numerals '1', '2' etc. for sections within a chapter (or one part of a chapter); I use lower-case letters 'a', 'b', etc. for divisions within a section; and I use lower-case roman numerals 'i', 'ii', etc. for divisions within divisions.

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For: C.U. and E.H.

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Conventions

I use double quotation marks for all direct quotations, single quotation marks for all other purposes. All emphases within direct quotations occur within the original text unless otherwise stated.

Summary

At the heart of my pragmatic theory of truth and ontology is a view of the relation between language and reality which I term internal justification: a way of explaining how sentences may have truth-values which we cannot discover without invoking the need for the mystery of a correspondence relation. The epistemology upon which the theory depends is fallibilist and holistic (chapter 2); places heavy reliance on modal idioms (chapter 4); and leads to the conclusion that current versions of realism and anti-realism are deficient (chapter 5). Just as my theory avoids the need for an epistemic 'given', it avoids the need for a metaphysical 'given' or 'joints'. I offer a view of the nature of philosophy and what it can properly achieve with respect to ontological questions (chapter 3); since those views lead me to believe that philosophical discussion about what exists should be restricted to 'entities' discussed in non-philosophical contexts, my views on how we should understand claims made about the existence of middle-sized physical objects (chapters 2 and 6), theoretical entities in science (chapter 6), and abstract entities in mathematics (chapter 7), give the thesis a schematic completeness. My theory leads me to a conception of inquiry which defends the cognitive status of moral statements whilst being critical of Kantian and utilitarian approaches to morality (chapter 8). Chapter 1 explores the views of my closest philosophical allies: William James and Nelson Goodman.

"Can we actually 'know' the universe ? My God, it's hard enough finding your way around Chinatown." Woody Allen.1

Introduction

What is pragmatism ? Why a pragmatic theory of truth and ontology ? Essentially, the conviction that all pragmatists share is that concepts are meaningful only to the extent that they are explicable in terms of our experience. Peirce wrote,[1]

> "...consider what effects, which might conceivably have practical bearings, we conceive the object of our conception to have. Then, our conception of these effects is the whole of our conception of the object."

And,[2]

> "...reality is independent, not necessarily of thought in general, but only of what you or I or any finite number of men may think about it."

The pragmatist's insistence that concepts are meaningful only if they can be explained in terms of our experience applies equally to the concepts of 'truth' and 'reality'. James wrote that the[3]

> "...notion of a reality independent of...us, taken from ordinary social experience, lies at the base of the pragmatist definition of truth. With some such reality any statement, in order to be counted true, must agree. Pragmatism defines 'agreeing' to mean certain ways of 'working', be they actual or potential."

Peirce wrote of the pragmatic maxim (quoted above) that,[4]

> "This maxim once accepted - intelligently accepted, in the light of the evidence of its truth, - speedily sweeps all metaphysical rubbish out of one's house. Each abstraction is either pronounced to be gibberish or is provided with a plain, practical definition. The general leaning of the results is toward what the idealists call the naive, toward commonsense, toward anthropomorphism."

Above all else, pragmatists are opposed to absolutism - the view that (real) knowledge of how the world is is knowledge of the world non-relatively to human experience. An absolute conception of the world may, I think, reasonably be defined as one which is entirely non-anthropocentric, and absolutist philosophers as those who believe that

entirely non-anthropocentric knowledge is (in principle) obtainable. This, effectively, is the view that Williams took in his (1978) when he said that,[5] "Knowledge is of what is there anyway." The absolute conception of the world requires it to be possible, in principle, to separate any contribution to knowledge of the world that might be peculiar or parochial to us from that of the world. Note that this is what the classical empiricists tried to do (in part) by employing the distinction between primary and secondary qualities. Absolutism is the attempt to describe the world sub specie aeternitatis and all pragmatists share the view that that attempt cannot succeed. (See figure 1 overleaf).

So far I have merely characterized pragmatism as the claim that all meaningful concepts must be explicable in terms of some relationship to human experience. The central problem for pragmatists is to specify exactly what that relationship is, and it is here that differences between pragmatists begin to emerge. The central tension within pragmatism is caused by the desire of pragmatists to insist that the meaning of concepts must be explicable in terms of our experience without endorsing subjectivism.

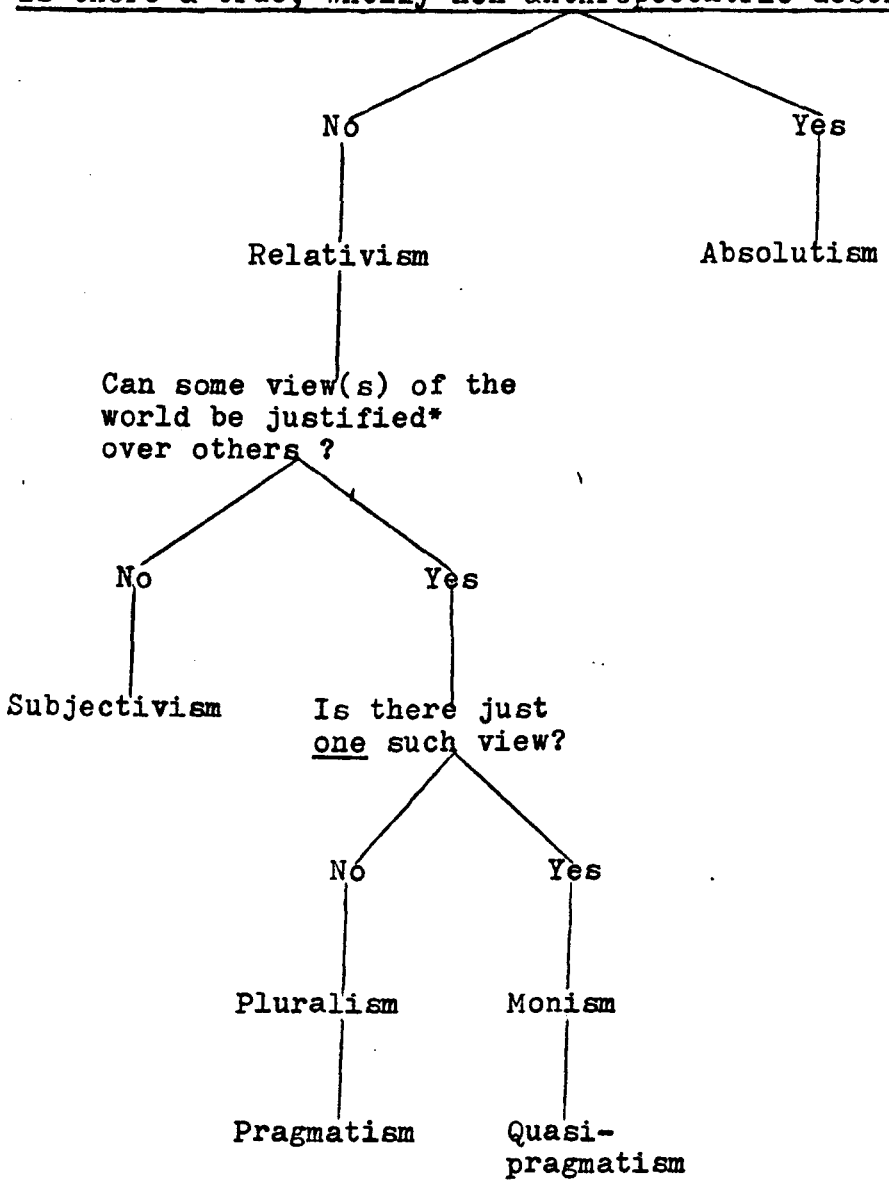
Peirce, characteristically, specifies the relationship of meaningful concepts to our experience in terms of the long-run.[6]

> "The opinion which is fated to be ultimately agreed to by all who investigate is what we mean the truth..."

That way of specifying the relationship suited Peirce temperamentally and was of a piece with his realism. James, however, was not only temperamentally a very different philosopher from Peirce,[7] but his interests and nominalist tendencies led him to stress the concrete historical situation facing any inquirer.[8] James, much more than Peirce,

Figure 1

Four Kinds of Philosophical Positions



* in any way, e.g. epistemologically, metaphysically, morally.

was inclined to stress that whatever may ultimately come to be regarded as true, here and now one must simply act on the best available evidence.

My own way of dealing with the tension within pragmatism rather cuts across the different emphases of Peirce and James. My overall approach resembles that of Peirce insofar as I rely upon modal idioms to state my views, although my view does not incur the difficulties of the idea of the 'long-run'. Temperamentally, however, I am much closer to James and, like him, I stress the view that inquiry is shaped by our various purposes. Unlike both James and Peirce I am inclined to think that the nominalist/realist debate is ill-conceived. (See chapters 3 and 7).

Referring to figure 1 it will be seen that in addition to relativism pragmatists uphold both non-subjectivism and pluralism. Taking the second view first, I define pluralism to be the view that there is more than one justifiable way of knowing the world, monism to be the view that there is only one such way. At first glance it might seem odd to draw this distinction <u>under</u> relativism at all; and it is true I think, that all absolutists are monists, but not conversly. (That all absolutists should be monists stems not only from the fact that it sounds odd to speak of absolute conceptions (plural) of the world but also that it is very unclear what would distinguish one absolute conception from another. By the definition of 'absolutism' it cannot be anything which depends on <u>us</u>, e.g. our purposes.) One might, however, be both a relativist and a monist on the grounds that whilst the absolutist programme cannot be carried out, nevertheless one is still left with only one justifiable way of knowing the world - scientifically perhaps. A philosopher who holds such a view might be Quine.[9] The issue between monism and pluralism might appear to be merely verbal for it is

certainly true that which position one adopts will depend upon how one is prepared to describe certain kinds of statements (whether, for example, one is prepared to describe commonsense everyday beliefs about the world as proto-scientific or as non-scientific). I shall argue, however, that this issue is more than verbal, that what distinguishes some ways of knowing the world from others are differences in purpose. An emphasis on the variety of purposes has been a traditional pragmatic theme.

Pragmatists have from time to time been accused of being subjectivists and it is one of my tasks in this thesis to show that there is a viable position between the rejection of absolutism and the endorsement of subjectivism. One way of being subjectivist is scepticism since scepticism often takes the form of claiming that certain kinds of beliefs cannot be justified. Rather than tackle scepticism head on I would say that to the determined sceptic there is little one can say; the reluctant sceptic I would hope to convince by my positive arguments.

A natural question to be asked in relation to figure 1 is where the later work of Wittgenstein fits. All is plain sailing I think, until one reaches the question concerning the justification of one view over another. Two questions then arise: what would Wittgenstein's attitude have been ?; and did Wittgenstein's work fall into subjectivism ? With respect to the first question, I think that Wittgenstein simply refused to answer it - I don't believe that he explicitly endorsed or denied subjectivism (although he denied the sceptical reason for being a subjectivist). I think it is clear that if Wittgenstein had chosen the path of non-subjectvism he would also have chosen the option of pluralism. Wittgenstein's work, then, is close to pragmatism whilst not being fully-fledged pragmatism. (I might add that my views were not

essentially derived from reading Wittgenstein's work.) Wisely or not, my attitude is more robust than Wittgenstein's, partly because I believe that some views of the world can be justified over others, and partly because I find nothing reprehensible about a philosopher wanting to change the world as well as to understand it. On the second question, I think that Wittgenstein's work sometimes totters on the edge of subjectivism even if it manages not to fall into it. (See chapter 2).

So far I have given some indication as to how my conception of pragmatism relates to the work of philosophers who are absolutists, to Quine, and to Wittgenstein. What of the philosophers whose work does not easily fit into either the relativist or absolutist camps ? (Assuming that they are working within epistemology and metaphysics). In my view the work of Davidson (in semantics) falls into this category although it has been claimed by absolutists (see chapter 5). One may not be able to categorize the work of a philosopher as relativist or absolutist for one of two reasons: (i) the work in question simply does not touch on the issues which would enable one to be able to categorize it; (ii) it is not possible to decide whether their work is relativist or not because of ambiguity. With respect to point (i) I do not regard it as a criticism of someone's work that it is not commited one way or the other. I would comment however that the question which heads figure 1 is a question which philosophers have tried to answer at least since Plato; not to have a view seems to me rather like being the conductor of a symphony orchestra, but having no interest in music ! With respect to (ii) I would argue that the work in question must be unstable; the choice between relativism and absolutism is mutually exclusive.[10]

I think that I have sufficiently indicated what I mean by pragmatism, and it will be obvious why I desire a theory of truth, but

perhaps I should explain why I also discuss ontological questions. The essential reason is that my defence of pragmatism would be incomplete if I did not; *prima facie* it appears to be a good argument against relativism to say that unless one adopts idealism (which I do not) then *what there is* exists independently of us and that describing what there is a way of viewing the world *sub specie aeternitatis*. I admit the plausibility of that argument and I meet it by providing at various points an account of what it means to say that middle-sized objects, theoretical entities in science, and abstract entities in mathematics do or do not exist independently of us. One reason why, for instance, I have to deal with abstract entities in mathematics is that if Platonism in mathematics (that numbers exist timelessly and independently of us) were the correct or the only acceptable view within the philosophy of mathematics, absolutism would, in part, be justified.

One important point ought to be made concerning figure 1. Figure 1 is not intended to suggest that all philosophers are equally absolutist or subjectivist with respect to *every* kind of statement. That is manifestly not the case. One important example is where a philosopher is absolutist with respect to science and subjectivist with respect to ethics. Given that very few, if any, philosophers now believe that there is an absolute realm of values it is not difficult to see why that combination of views should be frequently adopted. Such views are of a piece with some kind of epistemological or metaphysical dualism but an important feature of my theory is that it provides a unified account of knowledge. I do not rely upon, or accept, sharp distinctions between analytic/synthetic; facts/values; or the categories of the abstract and the concrete. Temperamentally I am somewhat like Dewey: when shown a dualism I reach for my arguments. It is true that I am free and easy with distinctions but distinctions are, I think, the acceptable face of

dualisms.

My theory simultaneously offers both comfort and unease in relation to subjectivists and absolutists alike. To the absolutist I say, 'yes, it is correct that given a linguistic context or vocabulary, statements may be true or false independently of whether we believe them to be true or false.' Against the absolutist I say, that 'the choice of linguistic context or vocabulary cannot be justified absolutely - the choice may justified by reference to some criteria or other but not by reference to what is 'purely given''. My fallibilism therefore has two sources: (i) we may falsely believe that some criteria are satisfied when they are not (and vice versa); (ii) that it is always open to challenge as to whether one is employing the most appropriate criteria. Often (ii) is the source of the unease and uncertainty we feel when defending our views. Often it is more difficult to know whether one does have the best theoretical framework or the most appropriate moral vocabulary than it is to know what the 'correct' conclusions are given that framework or vocabulary. To the subjectivist I say, 'yes, it is the case that we are fallible, and that no view can be justified absolutely', but against the subjectivist I argue that this does not justify the conclusion that there cannot be any non-arbitrary justification for one view over another. All in all, my view implies a shift towards the idea that, directly or indirectly, inquiry is always more or less <u>normative</u>. (A theme I take up in chapter 8).

Had pragmatism been a more fashionable philosophy it would not have been necessary for me to attempt to provide a general reworking of a framework to enable a reasonable defence of pragmatism to be made. My largest regret about this is that some of the themes of the thesis deserve an entire thesis to themselves. (I think that this is especially

true of the themes of chapter 8). There is also some irony in this situation. By and large the general drift of analytic philosophy in this century seems to me have been in a pragmatic direction (with the odd rearguard action here and there); but analytic philosophy has moved in this direction without most philosophers realizing it and without the wider implications becoming apparent. I am not surprised at the general direction since I believe that serious thought about philosophical issues in the 20th century will, other things equal, lead in a pragmatic direction. The lack of awareness of the wider issues, however, has meant both that there is still much to be learnt from Peirce, James and Dewey, and that we have not been able to focus on the issues and tensions which remain.

I have already made reference, at various points, to future chapters but I think it may prove useful to provide a brief description of the contents of each chapter here.

Chapter 1: Two Pragmatists

I place my work in an historical context by discussing the pragmatisms of William James and Nelson Goodman. James and Goodman are, with few qualifications, paradigm cases of pragmatist philosophers. Discussion of their work enables me to introduce my approach.

Chapter 2: Relativism

This chapter and the next form the core of my argument. At the centre of my relativism is a view which I term internal justification. The essential claim of this view is that because the relation between language and reality is holistic rather than atomistic, one does not have to accept a correspondence theory of truth in order to accept a relational theory of truth. Internal justification also makes it clear

how sentences can have truth-values which we cannot discover, without supposing either that we ever confront 'unconceptualized reality', or that we need the notion of 'unconceptualized reality' to defend a fallibilist epistemology.

Chapter 3: Philosophy and Ontology

What role philosophers, qua philosophers, may play in answering the ontological question of 'what there is' partially depends upon one's conception of the nature of philosophy. I argue for a view of philosophy as the 'inquiry into inquiry'. This metaphilosophical view does allow that philosophers may have a role to play in answering the ontological question but, crucially, I believe that this role must be secondary to the task of explaining how knowledge of various subject areas is possible.

Chapter 4: Possible Problems

The epistemological and metaphysical views developed in chapter 2 rely heavily upon the use of modal idioms. This chapter provides an analysis of modality in order to show that that employment of modal idioms is consistent with my general epistemology and metaphysics. Additionally, this chapter represents the approach to ontological questions argued for in chapter 3.

Chapter 5: Realism, Anti-Realism and Truth-Conditions

By employing the epistemological and metaphysical views developed in earlier chapters, together with the analysis of modality developed in chapter 4, I am able to argue against both sides in the recent realist vs. anti-realist debate. On the one side of the debate, I argue that Dummett's anti-realism is, ironically, the result of his adoption of a

realist premise that needs to be qualified according to my analysis of modality. On the other side of the debate, I argue that a proper understanding of the notion of truth-conditions shows that neither Tarski's theory of truth, nor Davidson's truth-conditional semantics, vindicate absolutism; contrary to the claims that Popper makes about Tarski's theory of truth, and contrary to the claims made by some Davidsonians about Davidson style truth-conditional theories of meaning, both Tarski's theory of truth, and Davidson's truth-conditional semantics, are compatible with the relativism that I favour.

Chapter 6: Realism in Science

This chapter considers the claim that science represents a vindication of absolutism. I present two arguments against that view: part I argues that whilst it is reasonable to uphold a realistic view of science, the most reasonable variety of realism does not justify absolutism; part II presents an argument against absolutism by considering the relation between 'common-sense' and scientific theory.

Chapter 7: Mathematics - Epistemology and Metaphysics

In this chapter I sketch a view of the epistemology and metaphysics of mathematics. The view developed, which I call structuralism, provides a view of mathematics which is not only consistent with my other views but also, I believe, interesting and attractive in its own right.

Chapter 8: Philosophy and the Norms of Inquiry

In this final chapter I extend my rejection of both absolutism and subjectivism to the area of morality. My views uphold the cognitive status of moral statements whilst being critical of Kantian and utilitarian theories. I sketch a view of inquiry which stresses its

historical and normative nature. I conclude with some reflections on philosophy and pragmatism.

Chapter 1

Two Pragmatists: William James and Nelson Goodman

In this chapter I want to give a general account of the views that will be developed throughout the rest of this thesis by considering some aspects of the work of W.James and N.Goodman. The purpose of this chapter is not, therefore, purely historical; although by presenting my views in relation to theirs I hope that I may be able to correct what I believe to be misunderstandings of their work. Although James was not blameless he has been subjected to a great deal of needless criticism that seems to have resulted from a lack of sympathy and imagination on the part of critics. If James was thought to be too radical to be taken seriously perhaps Goodman has suffered the opposite fate - that of being taken seriously but not being recognized as a radical. Goodman's radicalism (especially before his (1978)) has, perhaps, tended to lurk quietly in the background. I intend to bring it to the fore.

James and Goodman might be thought to be an unlikely combination. Certainly they differ considerably in <u>style</u>; James is the flamboyant stylist, Goodman the careful technician. Yet I hope to show that underlying these differences and others there is fundamental agreement both in the content and aims of their philosophies. The two basic doctrines that James and Goodman share are <u>pluralism</u> and <u>relativism</u>. Accordingly this chapter divides into two parts that explicate these views. Two caveats ought to be made now. First, that my treatment of James and Goodman has been cavalier in the sense that I have felt no obligation

to treat all aspects of their work that might rightly be considered important but I have dealt only with those aspects of their work that concern my general aims. Second, that the aims of sections 2a and 2b is merely to indicate the prima facie case in favour of their pluralism. The justification for any liberties that I take with their work is my conviction that they themselves would much prefer the rough and tumble of continuing debate to fossilized historical admiration.

My major criticism of James will be that he failed to develop his theory in sufficient detail to be able to answer the accusations of subjectivism that were leveled against him. That is not to say that everything he did say was satisfactory but I do believe that much of what he said does provide a reasonable base for a pragmatic theory of truth. In one way I feel much the same about Goodman. Goodman says relatively little about truth but there is, I feel, a theory lurking in the background waiting to be made explicit.

Part I: Pluralism

(1) Sources

Pragmatism has been accused of being an 'engineer's philosophy'. This accusation stems, I imagine, from the assumption that 'pragmatist' as in 'pragmatist philosopher' and 'pragmatist' as in everyday usage has the same meaning. In this section I want to show how utterly misconceived this view is and how wide of the mark the idea is that the philosophies of either James or Goodman are limited in either aim or achievement to the 'merely practical'. In fact only by realizing the very broad and ambitious nature of their philosophies can one understand them and, incidently, their relevance to this thesis. James said that the great thing about a person is their vision and this is no less true

of him than of others.

One of the major aims of James' philosophy was to reconcile science and religion. It was this aim, I suggest, which led James radically to question the picture of the cognitive process that emerges if one takes science as exhausting the field of knowledge, or takes its pretensions at face value. In other words it was because James took religion seriously that he questioned a positivist view of knowledge. James took religion seriously for he knew as an empirical psychologist how important people's spirtual views could be to them. This fact alone makes the jibe about an 'engineer's philosophy' ridiculous. Whether or not Goodman takes religion seriously, there is, I believe, an underlying structural similarity in the aims of their philosophies: whereas James questioned the claims of science on behalf of religion, Goodman does so on behalf of art. Of course art is not threatened in the same way that religion was, but the positivist picture of cognition has a strong tendency to relegate the status of art to that of emotional outburst.

Defence of art or religion has two aspects, a positive aspect and a negative aspect. The negative aspect consists of an attack on the pretensions of science to <u>exclusive</u> truth about the world. It is important to stress that it is the <u>pretensions</u> of science that are under attack and not science itself. James was himself a scientist of course and (with the possible exception of his views on sets) there is nothing in Goodman's work which is anti-science.[1] The attack on the claim that only science tells us about the world is of course the source of pluralism: there is more than one justifiable way of knowing the world. The other side of the coin, the positive aspect, is the attempt to provide a better account of the cognitive process which makes due

allowance for the claims of religion, art, or any other way of knowing the world.

One problem with pluralism has been expressed by Goodman as follows, "If there is but one world, it embraces a multiplicity of contrasting aspects; if there are many worlds, the collection of them all is one." What, then, is to be said in favour of emphasizing the plurality of aspects of the world ? The answer that Goodman gives is that,[3] "..many different world-versions are of independent interest and importance without any requirement or presumption of reducibility to a single base." What, though, makes for 'irreducibility' ? Two things, I believe, both hinted at in a passage from James,[4]

> "There are so many geometries, so many logics, so many physical and chemical hypotheses, so many classifications, each one of them good for so much and yet not good for everything, that the notion that even the truest formula may be a human device and not a literal transcript has dawned upon us."

The two things are these: (i) plurality of purpose; (ii) the inseparability of the human perspective from knowledge. The second of these will be considered below. As to the former one can perhaps explicate it this way: even if one confines one's attention to the purpose of <u>explaining</u> the world, we seek explanations of many things and actions where the claim that those explanations are scientific could only be defended by relying upon a highly tendentious definition of science. For example, one might seek to explain why a certain political or artistic movement arose at a particular time and it is at least open to question whether such an explanation could count as scientific.

So described such a pluralism sounds plausible but trivial - whoever denied that there are many different points of view from which the world can be known or theorized about ? Perhaps such a pluralism does deserve to be considered trivial but it is in fact deeply at odds

with deep-seated and cherished conceptions of the world which depend upon either the primacy of science or the primacy of certain kinds of descriptions of the world. In explicating what he takes to be Quine's ontological programme, Campbell says that,[5] "...to establish a reasoned inventory of the categories [of the kinds of things there are] would be achievement enough." The basic question is: from what point of view is this reasoning supposed to take place ? It is not scientific since it is a-priori. Well, philosophic then; but then this conception of philosophy - that it is philosophy that will tell us how the world is, of what kinds of things it is composed - is, from the point of view here adopted, a kind of scientism. This supra-science is supposed to have only one purpose, namely to give us the description of the world; but whether there is only one real (proper) kind of description of the world is what is here being questioned. At least relative to what some philosophers have thought, the pluralism which looked innocuous may turn out to be absolutism's Achilles heel.

Having said that the source of James' pluralism is respect for religion, whilst Goodman's is respect for art, I want briefly to consider the relation between their respective conceptions of religion and art and their epistemology. James first.

(2) Pluralism: Justification

(a) James and the right to believe

I said above that it was a major aim of James' philosophy to reconcile the claims of science and religion. James was never able finally to commit himself to theism but he certainly wanted to defend the right of others to do so. I do not want to attempt to provide a full

defence or explication of James' views on religion here but I do want to suggest how James' religious sympathies in fact led him to a (psychologically) more realistic epistemology than that of his opponents.

James' defence of the right to believe occured in his collection of essays entitled The Will to Believe (a phrase James came to regret in the light of subsequent misunderstandings). There are two main theses relevant here which I shall call the weaker and the stronger. Both have their difficulties although it is the latter which requires some reconstruction. I will discuss each in turn.

The weaker thesis is the view that where an option is "live, forced, and momentous", and the evidence for or against option is inconclusive, someone has the right to opt for whichever view would bring most benefit to that person. This thesis was opposed to the views of Clifford and Huxley who said that one should never adopt a view for which there was not conclusive evidence. Clifford wrote, for instance,[6] "It is wrong in all cases to believe on insufficient evidence..." Surely, in general, James is on the stronger ground epistemologically here: the evidence for scientific theories (let alone other kinds of theory) may always and in principle be inconclusive. If we were only to accept those beliefs for which the evidence is conclusive we would have very few beliefs.

The stronger thesis is that the very act of believing may itself produce or provide the evidence for the belief. In describing what I have called the stronger thesis Kennedy writes,[7]

> "...there are cases where the belief in the existence of a future fact may itself help to produce that fact. As James puts it, there are 'cases where faith produces its own verification.' The first of these [the weaker thesis] propositions is the only one which is disputable. The second is a question of psychological fact which in any given instance may be true or false. Surely there <u>are</u> cases

> where belief in the possibility of a future fact may help to bring about the existence of that fact. For example, to enter a marriage believing that it can be a permanently successful union may well help to make it so."

The interpretation Kennedy gives here fails, I think, to do justice to James' aims. Certainly there are cases where someone's having a belief may be a factor in making that belief true but of what relevance is this fact to James' defence of religion ? Only by asking that question do we begin to see the import of James' case but, at the same time, the difficulties also. One problem is this: what reason is there to suppose that the case of a religious believer is analogous to that of a person who believes in the success of the marriage ? Prima facie the cases are very different since it is easy to see how the belief in the success of a marriage may be a causal factor in that success but very difficult to see an analogy here with belief in God. Moreover, the weak thesis seems to establish little from James's point of view. All it says, after all, is that where the evidence for or against an option is inconclusive one may chose. But what constitutes 'evidence' here and is it entirely independent of believers ? The answer to this question, I believe, provides James with a stronger defence of religion, and with a general epistemological insight.

First, consider what James meant by calling an option 'live, forced, momentous'. To see what this might amount to, consider two individuals A and B who have had very different educational backgrounds. A, suppose, was brought up by religious parents, attended a religious school etc. B, by contrast, had atheistic parents and went to a secular school. In those circumstances it would be easy to imagine that for A the question of God's existence could become an issue of tremendous importance; A may hear arguments against the existence of God, and may have reasons

for doubting the existence of God, but their whole background and personality at some stage of their life, may make the question of God's existence of passionate importance. (I am not, of course, saying that this kind of background always produces that sort of outcome - someone 'cynical' about the effects of religious education might say that if one wants to produce an atheist the best way is to give them a religious education.) For A, then, the question of God's existence is a question which is 'live, forced, momentous'. B, by contrast, might be almost indifferent to the question of God's existence - B may hear arguments for and against the existence of God but be virtually unconcerned about them. It needn't be the case that B has firm convictions one way or the other - it is simply that the issue does not really 'touch' him, does not arouse curiosity or interest at all.

Now one traditional epistemological view would say all of the above is merely of psychological interest which may affect the context of discovery but cannot affect the context of justification. That is to say that on the traditional view, the psychological motivations of individuals may contingently affect their predispositions to hold one view or another, but it cannot affect the question of which view is rationally justified. James' strong thesis, however, raises the question of whether things really divide as neatly as that view claims.

Consider, first, claims that have been made to the effect that there can be direct religious experience - claims that through some mystical experience someone can obtain direct knowledge of God's existence. Supposing that A wants to believe in God's existence, A may have some experience that can be interpreted in that kind of way. Now of course atheists (like myself) won't accept those claims, they will say that there is some other (probably psychological) explanation of

the experience in question. And, of course, atheists are not likely to have 'religious experiences' since even if they have phenomenologically similar experiences to those described by theists, they will naturally offer a different explanation of what caused them. From the atheist's point of view, A's religious background etc. will be seen as a part of the cause for their 'misinterpreting' certain experiences. The trouble for the traditional epistemological view however is that theists can make an exactly symmetrical claim with respect to B. That is, they can say that because of B's background, B is simply insensitive to the kind of experience which would enable them to know of God's existence.

So far I have presented the argument in terms of religious experience where the case is perhaps at its strongest. I think, however, that something analogous is true of arguments. For someone of atheistic predispositions the first good looking argument against God's existence will be considered fairly conclusive, whereas for those of theistic predispositions it will set them to work to show what is wrong with it. Similarly, an argument for the existence of God which looks good to the theist will appear fairly conclusive, but to the atheist it will present the challenge of finding the fallacy.

The general moral of this tale, I believe, is that for people who do not share fundamental assumptions the notion of 'evidence' becomes problematic. Now I do not draw the conclusion, and neither did James, that this entails that rationality is a 'sham'. James wanted to encourage tolerance and acceptance of fallibilism. More generally, and perhaps more contentiously, the argument may be taken as pointing to the historical nature of inquiry. The pursuit of any inquiry requires a set of background assumptions against which to work; now the claim is not that those background assumptions don't change - on the contrary - a part of

the moral of the tale is the importance of those changes. For example, many people like myself have been in a position not unlike B's; it is not the case that such people have meticulously examined all of the arguments for and against the existence of God and then reached the 'rational' conclusion. For many of my generation, the issue never was a 'momentous' one - the background assumptions had changed from what they were for earlier generations. That, of course, does not entail these background assumptions are the correct or the best assumptions, but the demand that each assumption one makes should be rationally justified is unreasonable because impossible to fulfill. One is not precluded from bringing those background assumptions into question but I think it is true that one can't provide or produce non-question begging arguments (relative to some rival view) for every assumption presupposed by one's overall world-view. On major questions, the history of philosophy is less the history of arguments won as it is the history of assumptions changed.

I think that one other point is worth stressing with respect to James' view of religion. Partially because of an argument like the above, and partially for temperamental reasons, James was often more interested in the overall effects of sets of beliefs than he was in the individual justification for each one. For James, if there were a religion R which, for those who believed it, had benefical effects (e.g. that it gave them a zest for living, that it encouraged sensitive human relationships) he would have thought highly of R whatever he thought of the epistemological or metaphysical status of any of the beliefs that constituted R. Indeed, James was on occasion inclined to imply that the benefical effects were evidence for its truth. Now that does indeed look like the subjectivism of which James was accused but one thing to be bourne in mind is how broadly James was inclined to use

the concept of truth. Non-academics are sometimes inclined to speak of 'seekers after the truth' in something like the sense 'seekers after wisdom'. James' use of the concept of truth sometimes has more in common with the non-academic usage than the academic usage. If that is the usage in question then it seems to me that James' view is less objectionable than it may appear. (See part II for more comments in this vein).

The recognition of ineliminable historical factors in the very nature of inquiry seems to me to be both psychologically realistic and (partly for that reason) healthy. James astutely recognized that temperamental factors in philosophy divided the 'tough-minded' from the 'tender-minded'.[8] (There is an affinity with Goodman here.[9]) Apart from the psychological realism of this view, recognition of the influence of historical factors is a guard against humbug and an ally of fallibilism. I have heard it said that philosophers cannot support radical politics because they must commit themselves to the disinterested pursuit of truth. That view seems to me, as I think it would to James, to be nonsensical humbug. The view is nonsensical since the philosopher who does support radical politics presumably thinks that that is where the truth (or 'the good') resides. The view is humbug because those who take this view implicitly pass their own value-judgements off as though they were the final arbiter of truth. James wrote,[10] "The human mind always has and always will be able to interpret facts in accordance with its moral interests."

(b) Goodman: Art as Cognition

The kind of monism which James and Goodman oppose is the kind which says that only science tells us about the world. This doctrine was held explicitly by the logical positivists and implicitly by others. The plausibility of the doctrine has depended, I believe, on fixing the evidence. That is to say that the contrast between science as cognitive, and any other kind of inquiry as non-cognitive, has depended upon both exaggerating the claims of science (in a positive direction) and mimimizing the claims of non-science. I want to consider the positivist's view of art from a Goodmanian perspective. I think that the positivist's view of art may be not unfairly summarized as follows,[11]

science is cognitive because it:	art is non-cognitive because it:
- is discovered	- is invented
- is capable of being true/false	- is not capable of being true/false
- is non-emotive	- is emotive

Of these allegedly sharp contrasts I believe that Goodman would say different things; of the invention/discovery dichotomy and the true/false capability of science vs. art, Goodman would argue that neither is exclusively true of science or art. Of the emotive/non-emotive distinction, Goodman agrees that there is some contrast between art and science along this dimension but denies that it corresponds to the cognitive/non-cognitive dichotomy.

(b)(i) Invention vs. Discovery

The claim that science is discovered whilst art is invented appears to be patently false: scientists are required to invent their theories no less than artists are required to create their works. Moreover, the methodology according to which scientists simply construct their theories

after observing the world has long been discredited. Still, perhaps the view can be represented this way: whereas science does not invent its subject-matter, art does. This is of course oversimplistic given the long tradition of representation in the arts. One version of this view could be based upon what below I call a weakly activist epistemology. This would be the view that whilst science requires invention to discover truth, insofar as the enterprise is successful we can later discern our contribution from that of the world. That we can do this in the case of art would be wildly implausible. If there is one central tenet of Goodman's epistemology however it is that the idea behind weakly activist epistemology cannot be carried out. To suppose that we could strip away our contribution to knowledge would require that the remainder stand in some relationship to what is 'purely given'. On Goodman's view (and mine) we can make sense neither of what that relationship would be, nor of the idea of the 'purely given'. Goodman constantly, and effectively, reminds us that relationships like 'resemblence' or 'copying' can't be understood outside of a tradition or a set of conventions. (For my arguments against the given see chapter 2, sections 1 and 2). However, even given this view I don't think that this entirely disposes of the idea that science has more to do with discovery than invention since there is a more general source for this intuition.

The more general source might be described as the idea that science faces the recalcitrance of the world, art does not. Goodman quotes Sessions,[12] "...it is not sufficient to have the whole world at one's disposal - the very infinitude of possibilities cancels out possibilities as it were, until limitations are discovered." Of course anyone is free to put what squiggles they like on a piece of canvas but equally anyone is free to propose any crazy hypothesis or explanation they like.

Still, that does not entail either will be considered as art or as science. In the case of both art and science there is a symbolic system operating within a tradition that enables us to make relative judgements of success and failure. Recalcitrance has its analogue in the arts in terms of artistic success and failure, and if it is said that the cannons of artistic success and failure are difficult to codify the reply is obvious.

One variation on this theme would be to say that whilst there may be an analogue of recalcitrance in the arts there is no analogue of evidence. Goodman, like Gombrich, is fond of quoting Constable's remark that,[13] "Painting is a science..of which the pictures are but the experiments." It is no doubt very difficult to say what it is that makes for artistic success or failure but it is tempting to say that whatever it is provides the analogue of evidence in art. (That is not to suppose, of course, that there is any one thing which makes for artistic success - just as there is not one thing which counts as 'evidence' for the sciences.) If that idea seems far-fetched, I find Goodman's way of putting the boot on the other foot convincing,[14]

> "..to suppose that science is flatfootedly linguistic, literal, and denotational would be to overlook, for instance, the analog instruments often used, the metaphor involved in measurement when a nummerical scheme is applied in a new realm, and the talk in current physics and astronomy of charm and strangeness and black holes. Even if the ultimate product of science, unlike that of art, is a literal, verbal or mathematical, denotational theory, science and art proceed in much the same way with their searching and building."

(b)(ii) Truth and Art

If one takes the view that it is sentences that are true or false then of course some of the arts are immediately disqualified from having truth as their goal. Not all arts would be disqualified since

novels employ sentences. Nor is this point trivial; even if one leaves aside metaphorical truths, novels seem eminently capable of expressing literal but general truths about the world. The larger and more important question however is whether the dichotomy between activities capable of expressing truths for falsehoods and those which are not should be thought of as corresponding to the difference between cognitive and non-cognitive activities. Goodman expresses his view thus,[15]

> "Truth and its aesthetic counterpart amount to appropriateness under different names. If we speak of hypotheses but not of works of art as being true, that is because we reserve the terms 'true' and 'false' for symbols in sentential form. I do not say that this difference is negligible, but it is specific rather than generic, a difference in field of application rather than in formula, and marks no schism between the scientific and the aesthetic."

What may underlie the intuition that science is concerned with reality and art not, is what may well be a correct view of art captured succintly by the statement that 'art is not a copy of the world'. If so, however, what would be questioned here is the contrast this is supposed to provide with science; giving up an epistemology based on copying and a correspondence theory of truth, whilst accepting a strongly activist epistemology (see below), makes it impossible to say how much of science is invention or imitation. The viability of this alternative has yet to be shown and is here defered. However, if it is granted that the existence of some art forms such as the novel do blur the distinction between science conceived as the pursuit of truth and art as not, perhaps one should look to the third dichotomy between emotive and cognitive meaning for the correct explanation of the difference between art and science.

(b)(iii) Emotive vs. Cognitive Meaning

Intuitively, art would seem to involve emotion in some essential way that science does not; but what way ? It is certainly far from obvious that science requires any less emotional commitment or yields any less emotional satisfaction than art. One answer, favoured by the positivists, sees a difference in intention: art is intended to give emotional satisfaction, science not. Goodman asks the rhetorical question of whether the scholar seeks knowledge or the satisfaction of knowing. These seem so much the same that trying to do one without the other would seem to be a precarious enterprise. It seems as difficult to locate the role of emotion in aesthetic experience in a work of art as it is to locate it in the intentions of the artists themselves. Is a painting by Mondrian more or less emotive than Newton's laws ? One way or another most of the 'obvious' views as to the special role of emotion in aesthetic experience do not stand up to scrutiny - yet there does seem to be some special role. Goodman argues, following in a tradition that includes Langer and Herbert Read,[16] as follows,[17]

> "On the one side we put sensation, perception, inference, conjecture, all nerveless inspection and investigation, fact and truth; on the other, pleasure, pain, interest, satisfaction, disappointment, all brainless affective response, liking and loathing. This pretty effectively keeps us from seeing that in aesthetic experience the emotions function cognitively. The work of art is apprehended through the feelings as well as through the senses. Emotional numbness disables here as definitely if not as completely as blindness or deafness."

What I say in chapter 8 offers a similar view placed in the context of a general view of the nature of inquiry.

Two things need to be said in concluding this section. First, that I do not claim to have shown that Goodman's conception of art is correct (which is, in any case,more complex and subtle than I have intimated here) but rather to have given some indication of what his view is.

I hope that the following chapters will support, in broad terms, the conception of the cognitive process that Goodman offers. Second, that it is not being claimed that there are no important differences between art and science, rather that the differences are other than has often been supposed. Goodman puts it this way,[18]

> "The difference between art and science is not that between feeling and fact, intuition and inference, delight and deliberation, synthesis and analysis, sensation and cerebration, concreteness and abstraction, passion and action, mediacy and immediacy, or truth and beauty, but rather a difference in domination of certain specific characteristics of symbols."

Part II: Relativism

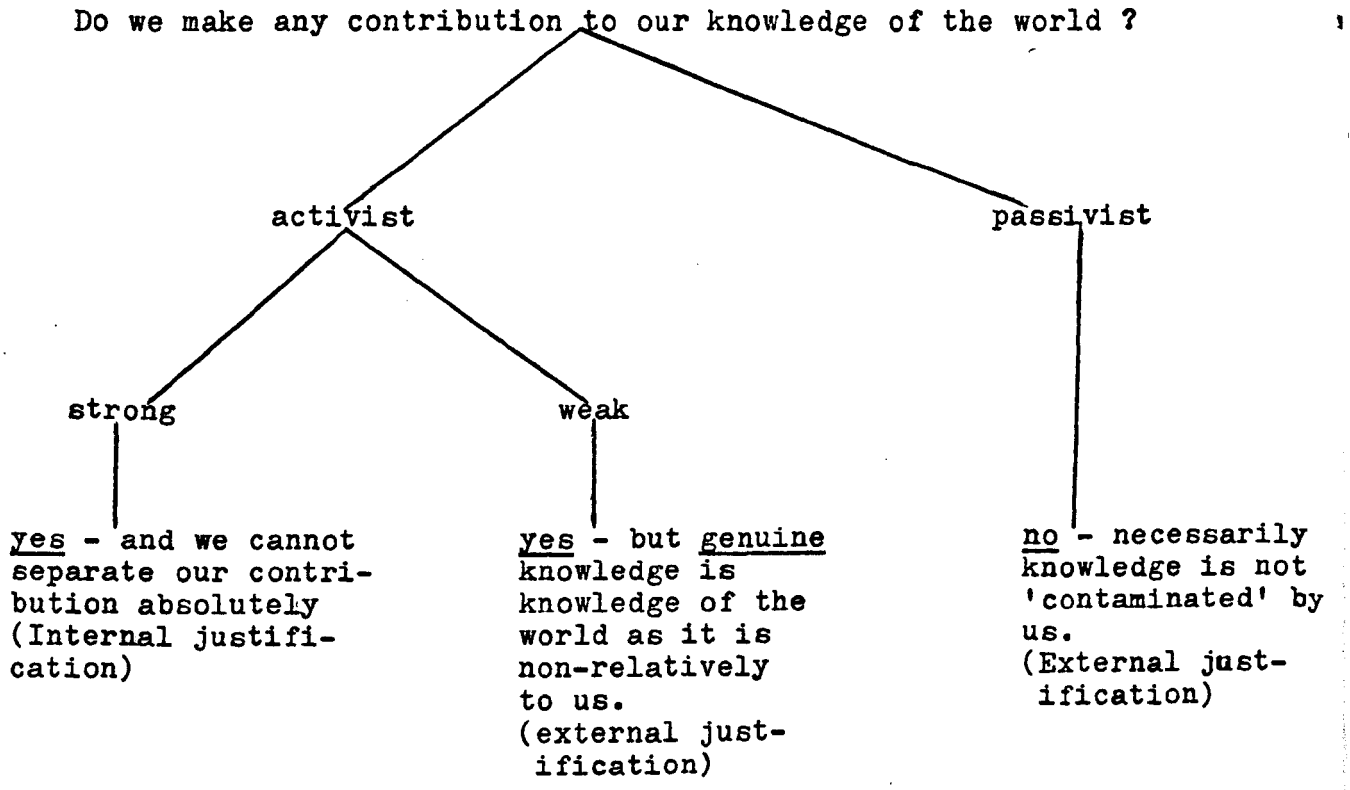
I want to begin my account of James' and Goodman's versions of relativism by considering three general epistemological views. (See figure 2 overleaf). The first distinction I want to mention was drawn by Lakatos in his (1970).[19]

> "There is an important demarcation between 'passivist and 'activist theories of knowledge. 'Passivists' hold that true knowledge is Nature's imprint on a perfectly inert mind: mental activity can only result in bias and distortion. The most influential passivist school is classical empiricism. 'Activists' hold that we cannot read the book of nature without mental activity, without interpreting it in the light of our expectations or theories."

Lakatos then went on to draw a further distinction between conservative and revolutionary activists; a distinction between those (the conservatives) who believe that we are trapped within our conceptual scheme and those (the revolutionaries) who believe that although we always work within our conceptual scheme we can nevertheless improve it as we proceed. James and Goodman are certainly revolutionary in that sense, but more important to my concerns here is a distinction I draw between strong and weak versions of activism. Hardly any philosophers now maintain a purely passivist view of knowledge; no one today seriously argues that knowledge is, or can be, acquired without the background

Figure 2

Views of the relation between knower and known



of our interests. However, it is still argued that although our interests and activities provide a necessary background for the acquisition of knowledge, real knowledge will be that knowledge from which we can isolate those interests. Thus Williams (reconstructing Descartes) writes,[20]

> "Are not all our concepts ours, including those of physics ? Of course: but there is no suggestion that we should try to describe a world without ourselves using any concepts, or without using concepts which we, human beings, can understand. The suggestion is that there are possible descriptions of the world which are not peculiarly ours, and not peculiarly relative to our experience."

The weak activist, then, agrees that in the acquisition of knowledge we will have to rely upon, say, linguistic conventions or our sensory mechanisms, but then suggests that we can later separate what we contribute from what the world contributes. The strong activist says that whilst it is the case that we can say how the world is from our point of view we can't make sense of how the world is absolutely. To say how the world is relative to a point of view is to say (in part) how the world is, and therefore, relative to that viewpoint says what the world contributes, but this does not say how the world is absolutely.

That both James and Goodman are strong activists is, I think, clear. James said,[21] "You see how naturally one comes to the humanistic principle: you can't weed out the human contribution."
And,[22]

> "What shall we call a thing anyhow ? It seems quite arbitrary, for we carve out everything..to suit our human purposes..We break the flux of sensible reality into things..at our will. We create the subjects of our true as well as our false propositions."

Goodman writes,[23]

> "Not only how but what [the eye] sees is regulated by need and predjudice. It selects, rejects, organizes, discriminates, associates classifies, analyses, constructs. It does not so much mirror as take and make; and what it takes and makes it sees not bare, as items without attributes, but as things, as food, as people, as enemies, as stars, as weapons. Nothing is seen nakedly or naked."

And,[24]

> "...what has been recieved and what has been done to it cannot be distinguished within the final product. Content cannot be extracted by peeling off layers of comment."

The whole emphasis of these quotations raises the spectre of subjectivism they raise the question of whether James and Goodman can, consistently with the views quoted, maintain that truth and reality are in <u>any</u> way, to <u>any</u> extent, independent of our desires and wishes. Now in figure 2 I labeled strong activism as <u>internal justification</u>. The term 'justification' appears to emphasize that neither James nor Goodman hold that every view, or theory, or practice is as good as any other <u>given</u> some purpose, framework or vocabulary.[25] The term 'internal' appears in order to emphasize that in James' and Goodman's view it is <u>only</u> relative to some purpose, framework or vocabulary that some view or theory can be justified relatively to some other. The whole thrust of internal justification was neatly summarized by James when he wrote,[26]

> "Theoretic truth is..no relation between our mind and archetypal reality. It falls <u>within</u> the mind, being the accord of some of its processes with other processes and objects - 'accord' consisting here in well definable relations."

I have so far given some indication of the overall view that James and Goodman want to defend; the question remains as to what extent they were successful. My overall view is that in both cases they did not sufficiently develop their view in order to be able to rebut the charges of subjectivism that may be leveled against them. Further, I think that James did fall into saying formally inconsistent things but that it is possible to rescue him from this inconsistency whilst fulfilling all of his major ambitions. My feeling about Goodman is somewhat different: the way in which I at any rate can see of defending Goodman's overall position does not show his views to be inconsistent but it does leave one wondering about the <u>motivation</u> for some of his views (on the eschewal

of non-extensional languages; his views on sets, for example). To develop these points, and to further explain the concept of internal justification I want to look at James' theory of truth.

James said,[27]

> "Truth...is a property of certain of our ideas. It means their 'agreement', as falsity means their disagreement, with 'reality'. Pragmatists and intellectualists both accept this definition as a matter of course. They begin to quarrel only after the question is raised as to what may precisely be meant by the term 'agreement', and what by the term 'reality', when reality is defined as something for our ideas to agree with."

James, then, was at least commited to a relational theory of truth. James said three things about truth which give rise to worries that his view may entail subjectivism. He said that truth was,

- man-made,
- satisfactory to believe,
- mutable.

What James meant by saying that truth is 'man-made' has already been hinted at: we make a contribution to knowledge which cannot be isolated. James did not mean that any statement can be made true. Whether that view can be justified depends upon spelling-out the theory of internal justification, which depends in turn upon the other things James said about truth.

Why should truth be 'satisfactory to believe' ? James' view was that it was satisfactory because a belief which is true will be one that is immune to overthrow by subsequent experience - with its attendent upheaval of our belief system. Two related misunderstandings give rise to fears of subjectivism here. First, there would seem to be many beliefs that we find satisfactory to believe but which are false. However, to the claim that[28] "'The true'..is only the expedient in our way of thinking" James added "...expedient in the long run and on the

whole of course; for what meets expediently all the experience in sight won't necessarily meet all further experience equally satisfactorily. Experience, as we know, has ways of boiling over, and making us correct our present formulas."

If one misunderstanding of the thesis that the truth is satisfactory to believe is the supposition that a belief is true if it is satisfactory now, another is the view that a belief may be true merely in virtue of satisfying our practical interests. R.B.Perry wrote two articles in which he said that the meaning and truth of ideas should be identified with their cognitive use. James reply in a letter is instructive and well worth quoting fully,[29]

> "[Pragmatism] seems to most people to exclude intellectual interests and relations, but all it means to say is that these are subjective interests like all the others, and not the sole aim in determining the beliefs that count as true...The pragmatic test of a concept's meaning is a possible experience somewhere, but the experience may be a pure observation with no 'practical' use whatsoever. It may have the tremendous theoretical use of telling us which concept is true however; and that may remotely be connected with practical uses over and above the mere verification or it may not."

And,

> "You speak..as if the 'degree of satisfaction' was exclusive of theoretic satisfactions. Who ever said or implied this ? Surely neither Dewey, Schiller nor I have ever denied that sensation, relation and funded truth 'dispose', in their measure, of what we 'propose'. Nothing that we propose can violate them; but, they satisfied, what in addition gratifies our aesthetic or utilitarian demands best will always be counted as more true. My position is that, other things equal, emotional satisfactions count for truth - among the other things being intellectual satisfactions."

Where James did run into difficulty, I believe, was with his thesis that truth is 'mutable'. James notion of mutability is evident in such passages as the following,[30]

> "The truth of an idea is not a stagnant property inherent in it. Truth happens to an idea. It becomes true, is made true by events. Its verity is in fact an event, a process: the process namely of verifying itself, its veri-fication. Its validity is the process of its validation."

And,[31]

> "The most fateful point of difference between being a rationalist and a pragmatist is now fully in sight. Experience is in mutattion, and our psychological ascertainments of truth are in mutation - so much rationalism will allow; but never that either reality itself or truth itself is mutable. Reality stands complete and ready-made from all eternity, rationalism insists, and the agreement of our ideas with it is that unique unalyzable virtue in them of which she has already told us. As that intrinsic excellance, their truth has nothing to do with our experiences. It adds nothing to the content of experience. It makes no difference to reality itself; it is supervenient, inert, static, a reflexion merely."

But now I think that the 'intellectualist' or 'rationalist' may legitimately object that it is inconsistent for James to begin by agreeing that truth is 'agreement with reality' and then claim that truth is 'mutable'; that is, it is hard to see how some statement which agrees with reality at some particular time could yet cease to be true at some other time unless this were trivally the case, e.g. that the statememt in question is indexical.

There is, I think, a genuine difficulty for James's theory here but there are three things I want to say about it. James's pragmatism led him to emphasize the concrete historical situation faced by each inquirer rather than the long-term theoretical possibilities. In this vein he wrote,[32]

> "Of course, if you take the satisfactoriness concretely, as something felt by you now, and if, by truth, you mean truth taken abstractly and verified in the long-run, you cannot make them equate..Yet at each and every concrete moment, truth for each man is what that man 'troweth' at that moment with the maximum of satisfaction....; and similarly, abstract truth, truth verified in the long run, and abstract satisfactoriness, long-run satisfactoriness, coincide."

It seems to me that James needed an account of modality which he did not provide. I believe that a correct analysis of modal idioms allows one to show how James' overall ambitions are consistent with the rejection of subjectivism but James himself did not provide this account. (Strangely enough, I believe that something similar is true of Goodman,

for although Goodman has written brillantly on modality he refuses to employ modal idioms in defending (or explicating) his own views).

The second, and more fundamental, thing I want to say concerning James' notion of mutability of truth relates it to the central concept of internal justification. Internal justification, as I see it, requires two elements: it requires something like a framework, or vocabulary, or purpose, and then it requires relatively better or worse satisfiers of the framework, vocabulary, or purpose. When James stressed the mutability of truth I think he intended to deny that the framework, vocabulary or purpose can be justified absolutely by what is 'purely given'. In stressing the mutability of truth James was stressing that the framework, vocabulary or purpose is, quite properly, alterable in the light of experience. Consider, for example, a Newtonian vs. an Einsteinian 'framework'. Argument still rages as to whether it is better to say that the Newtonian world-view has been falsified or whether it has merely been shown to be true within a more limited domain. James would here, I think, have sided with those who say that the Newtonian framework has been falsified for the truth, on his view, consists in whatever the best theory says. (A point I shall return to below). What James failed to emphasize sufficiently is that a sentence such as 'Caesar crossed the Rubicon', given that the framework (and, hence, the meaning) remains constant, is either true or false eternally.[33] The general point is so important to the concept of internal justification that it is worth dwelling upon.

External theories of justification (which in terms of figure 2 include both the weak activist and passivist views) typically take one of two forms: (i) there is the view that there is an epistemic

'unconceptualized given' to which statements must be true. (Lewis argued for this view - see chapter 2). (ii) that there is an ontological structure to the world (sometimes called 'joints') to which statements must be true. (Williams, Platts and others seem to accept this view - again see chapter 2). Now both of those views are that there is a 'given', something entirely independent of our conceptualizations which true statements must describe. Both of those views would ensure, so to speak, the absolute immutability of truth, for if a statement does describe that 'given' which is entirely independent of our conceptualizations that is the end of the matter. As James put it, truth is here thought of as mere 'reflexion'. However, James (like Goodman) rejects both of those views and it is not difficult to see why this is of a piece with the stress on the mutability of truth. Roughly the view is this: given some framework or vocabulary etc. statements are true or false, but since no framework or vocabulary is, or can be, justified purely by the 'way things are' there always remains the possibility of a better framework or vocabulary. Now I don't think that this does remove the inconsistency from James' view - if a statement really is true (as opposed to our merely believing it to be true at a given time) then it will always remain true - but it does I think explain why James was led to say what he did. The third thing I shall say will reinforce this point but first I want to make two further points concerning internal justification.

A part of the attractiveness of ideas of the given no doubt resides in the fact that they obviously provide a reason why truth and ontology should be independent of us. A major task in the chapters that follow is to show in what sense truth and ontology are independent of us consistently with a theory of internal justification.

Above I have spoken of the two forms that external theories of justification typically take but those two forms do not exhaust the possibilities. Consider, for example, possible accounts of the nature of 'logical laws'. One kind of account might speak of the 'laws of thought', another of the set of a-priori truths, but the advocate of internal justification is likely to take a different approach. Goodman, considering the justification of deduction, writes,[34]

> "...deductive inferences are justified by their conformity to valid general rules, and..general rules are justified by their conformity to valid inferences. But this circle is a virtuous one. The point is that rules and particular inferences alike are justified by being brought into agreement with each other. A rule is amended if it yields an inference we are unwilling to accept and an inference is rejected if it violates a rule we are unwilling to amend. The process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either."

This strategy of viewing the cognitive process as a process of mutual adjustment between different parts of our conceptual scheme provides a typical example of a theory of internal justification for it avoids the need to suppose that there is an a-priori or absolute starting point from which inquiry must commence. The only 'given' starting point that pragmatists accept is the historically given problem situation in which inquirers find themselves. That is to say, each member of a cognitive community finds themselves with a cognitive inheritance (e.g. via the process of language aquisition) and a number of 'problems'. Inquiry is then seen as the attempt to solve the problems, an attempt which may require the inquirer to modify or to reject parts of their cognitive inheritance. One distinguishing feature of pragmatist philosophy, as opposed to most other schools of analytic philosophy, is its tendency to stress the historical nature of the cognitive process. In broad terms this is a major aspect of the pragmatists' rejection of the idea that

we can understand the world sub specie aeternitatis to which I refered in the introduction. (This theme is taken up in chapter 8 but echoes of this view will be found throughout the thesis).

Above I quoted James as saying that "..other things equal, emotional satisfactions count for truth - among the other things being the intellectual satisfactions." This comment by James suggests two distinct attitudes that one might adopt toward the notion of truth. I can illustrate this difference by supposing that we have two theories T_1 and T_2 which are adequate for some purpose. Suppose further that T_1 is preferable along certain pragmatic dimensions - simplicity, economy etc. Supposing that it is not the case that one theory is really true and the other isn't true (this kind of situation, many believe, obtains with respect to various forms of set theory - see chapter 7) two further attitudes are possible: some will say that both theories are equally true but that T_1 is pragmatically better, whilst others might want to say that T_1 is a truer theory. James opts for the second option because 'intellectual interests and relations' are 'subjective interests like all the others'. My attitude differs from that of James here. I suspect that there may be good reason for prefering the first to the second alternative. One reason, for instance, might be that truth does not come in degrees.[35] However I would entirely agree with James in rejecting a bad reason for taking the first view. A bad reason would be one which patronized the pragmatic criteria or, in James's terms, held that truth was the paramount consideration because truth is 'objective' whereas the pragmatic are held to be 'merely subjective'. My attitude is this: one can either argue that truth does not come in degrees and distinguish truth from 'pragmatic criteria' or one can argue that truth, like other considerations, is only one of many pragmatic criteria provided that one adopts compensating

attitudes to the importance that attaches to the pursuit of truth. That is, if one takes the former view, which apparently supports the attitude that truth is 'objective', one must recognize that truth is not the sole or necessarily paramount consideration. This view is taken by Goodman when he writes,[36]

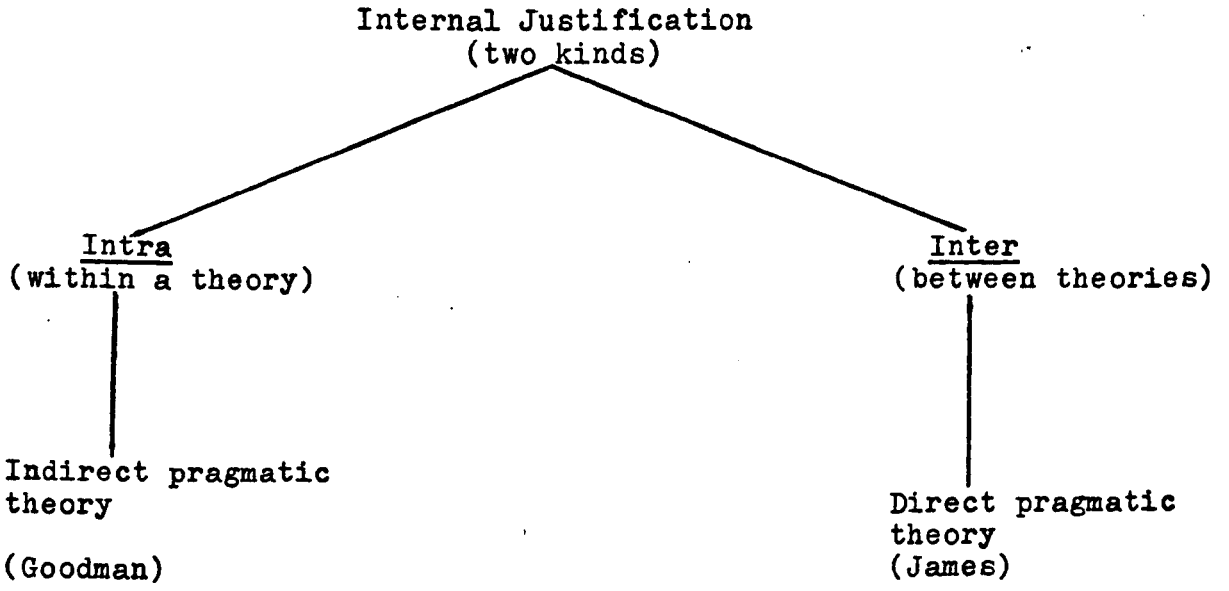
> "The scientist who supposes that he is single-mindedly dedicated to the search for truth deceives himself. He is unconcerned with the trivial truths he could grind out endlessly; and he looks to the multi-faceted and irregular results of observations for little more than suggestions of overall structures and significant generalizations. He seeks system, simplicity, scope; and when satisfied on these scores he tailors truth to fit."

The second view, which James takes in the passage quoted, takes 'truth' to include considerations of scope, simplicity etc. If one adopts this conception of truth then it is indeed appropriate to call science a pursuit of truth but it is also to make truth itself pragmatic. I am suggesting, therefore, that there are two major options for a pragmatic theory of truth the choice of which involves no important difference of principle. For convenience I will term them the indirect pragmatic theory of truth (the first view) and the direct pragmatic theory of truth (the second view). The indirect view still deserves to be called pragmatic, in my view, for two reasons. First, because it rejects absolutist or 'external' theories; second, that although within some theory truth appears to be non-pragmatic, independent, 'objective', choices between theories can, even in principle, only be made on pragmatic grounds. (See figure 3 overleaf).

The distinction between direct and indirect pragmatic theories of truth accounts for two things: (i) it helps to explain James' stress on the mutability of truth since clearly if one includes considerations of simplicity etc. the rate at which we change our assessment of theories as true (or as likely to be true) will be all the faster; (ii) this distinction accounts nicely for a difference between James and

Figure 3

Direct and Indirect Pragmatic Theories of Truth



Goodman. I suggest that it is because James is inclined to take the direct view, and Goodman the indirect view, that accounts for the fact that although their epistemology is strikingly similar, James said a good deal about truth whereas Goodman says little. Since it will simplify the attainment of my objectives I shall, in subsequent chapters, be developing an indirect theory.

I have said less about Goodman than about James in this section. Partially that is because, as just noted, Goodman says little about truth. I shall however return to some of Goodman's views in future chapters (see chapter 3 in particular) as I now proceed to develop my own version of relativism and internal justification.

Chapter 2

Relativism

In this chapter I outline my general approach to epistemology and metaphysics. The first two sections give a preliminary outline of the reasons I find for rejecting the idea of an epistemic or metaphysical given. Although section 1 only considers one philosophers's arguments (those of Lewis) in favor of an epistemic 'given', the conclusions reached are, I believe, generally applicable to foundationalist epistemology. Section 2 considers an intuitive picture of inquiry which involves the notion of 'joints'. Arguably one of the things wrong with this picture is that it requires the idea of a private language. I sketch one way of viewing Wittgenstein's argument against the possibility of a private language and my response to it. Section 3 develops the concept of a 'linguistic context', that to which truth and ontology are relative. The central problem is how one may view the relation between language and reality without relying on a 'correspondence' relation on the one hand, or falling into subjectivism on the other. That, indeed, is a problem of the whole chapter (and thesis), but in this section I show how certain linguistic phenomena (e.g. open-texture) motivate my view and may be accomodated within it. Section 4 considers some of the epistemological implications of my views and shows them to be compatible with fallibilism.

(1) Epistemology and the 'given'

"...the purpose of knowledge", Lewis writes,[1] "...is to be true to something beyond it." This meant, on the view Lewis developed, that there must be a 'given' which is independent of the activity of the

mind,[2] "The two elements to be distinguished in knowledge are the concept, which is the product of the activity of thought, and the sensuously given which is independent of such activity." Concerning their relationship Lewis claimed that,[3] "The pure concept and the content of the given are mutually independent; neither limits the other." Lewis's fundamental argument for the given is this,[4]

> "If there be no datum given to the mind, then knowledge must be contentless and arbitrary; there would be nothing which it must be true to. And if there be no interpretation or construction which the mind imposes, then thought is rendered superfluous, the possibility of error becomes inexplicable, and the distinction of true and false is in danger of becoming meaningless."

Importantly, however, Lewis did not argue that the mind first receives the given and then processes it for he was not concerned with an actual separation of the process of observation from conceptualization, but with a theoretical analysis of different components of experience. The given is an abstraction,[5] "..it is in, not before, experience." Indeed he says,[6] "This given element is never, presumably, to be discovered in isolation."

The independence of the given is not, then, the isolation of the given: it does not imply that there can be pure observational states which precede the act of interpretation. How, then, are we to identify the given for theoretical purposes ? He gives the following example,[7]

> "At the moment, I have a fountain pen in my hand... I abstract this item from the total field of my present consciousness and relate it to what is not just now present in ways which I have learned and which reflect modes of action which I have acquired. It might happen that I remember my first experience of such a thing. If so, I should find that this sort of presentation did not then mean 'fountain pen' to me. I bring to the present moment something which I did not then bring; a relation of this to other actual and possible experiences, and a classification of what is here presented which I did not then include in the same group. This present classification depends on that learned relation of this experience to other possible experience and to my action, which the shape, size, etc., of this object was not then a sign of. A savage in New Guinea, lacking certain interests and habits of action which are mine, would not so classify it."

And,[8]

> "...suppose my present interest to be slightly altered. I might then describe this object which is in my hand as 'a cylinder' or 'hard rubber' or 'a poor buy'. In each case the thing is somewhat differently related in my mind, and the connoted modes of my possible behaviour toward it, and my further experience of it, are different. Something called 'given' remains constant, but its character as sign, its classification, and its relation to other things and to action are differently taken."

It is, then, the constancy of the given through all variation of interests that characterizes the given and limits possible conceptualiztion,[9] "I can apprehend this thing as pen or rubber or cylinder, but I cannot, by taking thought, discover it as paper or soft or cubical." He points out however,[10]

> "While we can thus isolate the element of the given by these criteria of its unalterability and its character as sensuous feel or quality, we cannot describe any particular given as such, because in describing it, in whatever fashion, we qualify it by bringing it under some category or other, select from it, emphasize aspects of it, and relate it in particular and avoidable ways."

And,[11]

> "So that in a sense the given is ineffable, always. It is that which remains untouched and unaltered, however it is construed by thought. Yet no one but a philosopher could for a moment deny this immediate presence in consciousness of that which no activity of thought can create or alter."

This view is therefore one version of foundationalist epistemology: unalterable, directly intuited, ineffable, certain, the given provides an independent control over conceptualization and a firm foundation for knowledge. It is not difficult to see, however, that this view faces serious internal difficulties. When Lewis said that he could not, by taking thought, discover the pen as soft, paper or cubical, he intended to illustrate the distinction between the given and its interpretation,[12] "My designation of this thing as pen reflects my purpose to write; as 'cylinder' my desire to explain a problem in geometry..." and these descriptions do not reflect the given because they are 'alterable' according to purpose. In that case however, how can the inability to

to apprehend the pen as paper, or soft, or cubical, be a constraint on the given, for are not the descriptions 'non-paper', 'non-soft', and 'non-cubical' equally available for different purposes ? Perhaps the reason for asserting the 'givenness' of the things being non-paper, non-soft, and non-cubical, is that under any variation of interest I cannot bring myself to use the contrary notions 'paper', 'soft', or 'cubical'. But if the inability to entertain contrary notions is the mark of the given then 'pen', 'cylinder' and 'rubber' must equally belong to the given since their contraries are equally inapplicable.

There is a more general problem with Lewis's notion of the given than the one just discussed. The doctrine of the ineffability of the given seems to be inconsistent with the example he gives of its unalterability. The alleged impossibility of apprehending the pen as soft seems to imply that its hardness is given and to provide the description of it as hard or non-soft. More generally still, either the given must be describable (and thereby subject to error in Lewis' view), or ineffable as he says it is - but in the latter case its epistemological role must be severly limited. This last point can be expanded thus: if we are to justify the claim that observation may provide some independent control over conceptualization in science for instance, we need to explain how this can occur at the level of descriptions rather than at the level of experience private to individuals. Evidence for scientific theories must be amenable to intersubjective verification but on Lewis's view that which is capable of intersubjective verification is not that which is certain.

The rejection of a certain, conceptually untainted foundation for knowledge seems to leave us impaled on the second horn of a dilemma

posed by Scheffler,[13]

> "Observation needs to be construed as independent of conceptualization if conceptualization is not to be simply arbitrary; yet it cannot plausibly be thought to be independent of conceptualizatio On the contrary, it is shot through with interpretation, expectation, and wish."

And,[14]

> "So, on the one hand, observation must be independent, and, on the other hand, it cannot be. To suppose it is independent commits us to an implausibly pure observational given, and makes a mystery of observational control over thought. To suppose, on the other hand, that it is not independent commits us .... to the view that apparent observational control is always circular and hence incapable of restricting the arbitrariness of conception...."

He answers the dilemma as follows,[15]

> "Conceptualization relates both to the idea of categories for the sorting of items and to the idea of expectation, belief, or hypothesis as to how items will actually fit available categories; it links with the notion of category and, also, with the quite different notion of hypothesis. The very same category system is, surely, compatible with alternative, and indeed conflicting hypotheses: that is, having adopted a given category system, our hypotheses as to the actual distribution of items within the several categories is not prejudged."

I believe that this answer to the dilemma is fundamentally correct and it provides the cornerstone of my relativism. What makes this view so important in my view is that it allows one to understand how observation can provide independent control without it being the case that we ever confront unconceptualized reality. A prosaic example might help. Faced with the question, 'is there a table in that room ?' I know what kind of thing to look for; I know what would count as evidence for or against one answer from my background knowledge of the English language. When I look in the room I do not confront an unconceptualized set of facts but neither does this mean that the truth or falsity of any answer I give is not independent of me.

This case is an instance of what in chapter 1 I called internal justification and it is worthwhile explaining this description. The

strategy of internal justification is to suggest that we should view some of the changes in our beliefs as the result of adjustments we may make between principles (or 'categories') and particular judgements (or 'hypotheses'). I hope to show that by adopting this strategy one can explain in what sense truth and ontology are independent of us without the need for a mysterious correspondence relation and consistently with fallibilism. The strategy of internal justification can be extended beyond showing we do not need the idea of an epistemic 'given', to show that there is no need for a metaphysical 'given' either. Perhaps I should make it clear that my talk of categories is a <u>façon de parler</u> invoked to explain the theory. I do not believe that there is a set of categories that we must possess and that someone could list - indeed it is partly that fact which permits fallibilism. (But see section 4.a. below). I call this view <u>internal</u> since it is a crucial consequence of the theory that epistemologically we don't need the concept of data unsullied by conceptualization, and metaphysically we don't need the idea of a pre-existing ontology to which our true theories must correspond It is, or ought to be, uncontroversial that we <u>do</u> always work with our concepts but I claim further that we have no need of the idea that if we are lucky those concepts will be ones that pick out 'what is there anyway.'[16] In my view it is just as well that we don't need such things for they have always defied explication in the past and they continue to do so today. I call my view <u>relativist</u> (to contrast with <u>absolute</u>) since I claim that <u>relative to</u> a particular 'category' a statement may say something true or false about the world, and relative to a 'category' one may make true or false existential statements about what exists. Which 'categories' to adopt is subject to revision in the light of our experience and purposes.

(2) Metaphysics and the 'given'

An intuitive picture of how inquiry is possible might go as follows: imagine the world before there were any sentient beings in it; at that time there would already have been certain things and kinds of things, e.g. mountains, trees. Now imagine a solitary sentient being appearing on the scene; this solitary being is to be imagined as having a certain sophistication - in particular it has the capacity to form concepts and to use language. After the elapse of some time, the sentient being starts to utter sentences. Now it is possible that if the sentient being happens to utter the correct sentences, the ontological commitments of those sentences may correspond to the things and kinds of things that existed before the sentient being arrived.

Intuitive though the picture may be I believe that just about everything is wrong with it. My targets are the ideas of the ontology which exists prior to the sentient being (which from here on I shall refer to as 'joints') and the idea of 'correspondence'. One way of arguing against those concepts is to employ an argument developed in the later work of Wittgenstein. I will first sketch this argument and then indicate my attitude toward it.

Kripke's presentation of the private language argument in his (1982) takes the central problem to arise from what have been called the 'rule following considerations'.[17] Kripke first presents the problem in the form of a sceptical paradox and then offers Wittgenstein's 'sceptical solution'. Following Kripke I will present the problem by using a mathematical example but the argument is intended to cover any use of language.

English speakers use the word 'plus' and the sign '+' to denote

the mathematical function of addition. This function is defined for all pairs of positive integers. An important fact about all English speakers is that they will have only computed the value of the function for a finite number of cases. Imagine one speaker S, and suppose that S has never previously computed the value of 68 + 57. S performs the computation and gets the solution 125. Perhaps after checking, S is confident that 125 is the correct answer both in the sense that it is arithmetically correct and that '+' has been used in accordance with S's past intentions. Now suppose that a bizarre sceptic queries S about about this result and suggests that as S had used '+' in the past the answer S should have given was 5 ! S's confidence that 125 and not 5 is the correct answer cannot be because S had previously performed the computation. S may say that they intended to use the same rule or function that they had used in the past but, the sceptic asks, who is entitled to say what that rule or function was ? Suppose that all of the computations S has performed in the past have involved numbers smaller than 57, so perhaps the function S used could be the 'qunus function', symbolized as '⊕' and defined thus:

$$x \oplus y = x + y, \text{ if } x, y < 57$$
$$= 5 \text{ otherwise.}$$

Bizarre though the sceptic's proposal is it does seem logically possible. Imagine that S did mean addition but that under the influence of LSD S misinterpreted past uses of '+' as the function ⊕ and computed 68 + 57 as 5. In those circumstances S would have made a mistake concerning their past intentions and if, as it seems to be, that is possible, the sceptic asks why it could not happen the other way around, i.e. that S has made a mistake of that kind but with 'plus' and 'qunus' reversed.

The sceptic's proposal is bizarre but if it is false then there must be some fact about S in virtue of which S meant '+' and not '⊕'. S will undoubtedly have felt 'directed' by a rule to answer 125 and not 5 but those directions cannot have been (ex hypothesi) that S would answer 125 if queried '68 + 57 ?'; nor of those of acting in accordance with the rule exhibited in previous examples since quaddition (defined by the ⊕ function) is compatible with that. Thus the sceptic poses two challenges: (i) is there any fact about S that S meant '+' and not '⊕' ?; (ii) does S have any reason to be so confident that they should answer 125 and not 5 ? If the sceptic is correct, the concepts of meaning and intending one function rather than another make no sense, for the sceptic holds that no fact about S establishes that he meant plus rather than qunus. (Note that this would hold in one's own case - the argument does not presuppose a first/third person asymmetry).

A full examination of the various responses to the sceptical problem is not possible here but two points are worth making. One possible response is to suggest that the problem can be overcome by appeal to a dispositional analysis. On this view the fallacy in the argument consists in supposing that one must find some occurent mental state in virtue of which S meant 'plus' rather than 'qunus'. The dispositional response concedes that there was no such state but points out that there was all along the disposition to respond in a certain way. Certainly if S was a typical speaker, under normal circumstances, then S would have had the disposition to respond '125' rather than '5'. How to spell out 'normal circumstances' and 'typical speaker' need not detain us for the sceptic need not challenge the fact that S does indeed have this disposition, but the sceptic will claim that this is irrelevant. The problem with the dispositional response is that the sceptic is seeking justification for the fact that S would respond '125'

rather than '5'. The fact that S would respond that way does not justify the response. The second point is that what gives the sceptical problem its force is that it looks unlikely that the nature of S's mental states could provide the kind of justification sought. Even if it were true that some particular kind of mental state accompanied every disposition to respond in one way or another to '68 + 57 ?' how could that fact justify one response rather than another ?

What, then, is Wittgenstein's answer to the sceptical problem ? Scepticism is a substantive philosophical thesis and cannot therefore, officially at least, be Wittgenstein's view. (Philosophy "...leaves everything as it is."[18]) Kripke argues, convincingly to my mind, that Wittgenstein proposes a sceptical solution to the problem. Whereas a 'straight' solution to a sceptical problem would show what was wrong with the argument (in this case it would point to some fact about S which distinguished S's meaning 'plus' rather than 'qunus') a sceptical solution accepts the facts that the sceptic points to but disputes the contention that our ordinary beliefs and practices stand in need of the kind of justification the sceptic has shown to be untenable. (Compare Hume's 'solution' to the problem of induction). The Wittgensteian view appears to concede to the sceptic that for a speaker considered in isolation, there is no substantive content to the idea of a rule guiding behaviour. If our attention is restricted to one person alone, following a rule 'privately', all one can say is that they are entitled to follow 'the rule' as it strikes them. Wittgenstein says,[19]

> "..to think one is obeying a rule is not to obey a rule. Hence it is not possible to obey a rule 'privately': otherwise thinking one was obeying a rule would be the same thing as obeying it."

However, the situation appears differently when one considers the rule follower in relation to the wider community. A teacher training a child

in the practice of addition will only accept certain responses as correct, but once the child gets most of the responses 'right' then the child will be said to have learnt addition. Something similar can be said of adults: if someone starts giving bizarre (qunus like) responses we will say that they are not following a rule. From this one can discern rough assertability conditions for sentences of the form, 'Jones means addition by 'plus'', e.g. if Jones generally gives the responses accepted by others, and will accept correction if he makes a mistake. Generally, of course, there is a great deal of uniformity in responses and this fact makes the usefulness of certain practices possible.

I think, as Kripke says, that it is important to stress that Wittgenstein's view is a view about assertability conditions rather than truth-conditions. One can understand, at the communal level, how one may be justified in asserting of someone that they have such and such concepts and follow such and such rules. The only justification required or possible for those assertability conditions is that they make possible a form of life. It is not Wittgenstein's view that the fact that someone's response agrees with the response expected by the language community that that makes that response true. Those who do not have the dispositions to make the responses we do cannot participate in our form of life.

Reverting to the intuitive picture from which I began, the Wittgensteian argument is intended to show its incoherence not because the sentient being is physically isolated but because that picture wrongly presupposes that one can make sense of an individual possessing concepts when considered in isolation.

The interesting, important and difficult question concerning the

rule-following considerations is what they show. Wittgenstein's view is not, as I noted, a theory of truth-conditions but does it entail that a theory of truth-conditions is impossible ? I shall try to show that understood one way the rule-following considerations are compatible with a theory of truth-conditions; understood another way they are not compatible with a theory of truth-conditions but this view leads to subjectivism which, arguably, is inconsistent with Wittgenstein's non-revisionism.

Above it was noted that that the dispositional account did not seem to meet the problem posed by the sceptic: the fact that some people are disposed to respond one way rather than another does not provide justification for 'one rule' rather than another. What I think was wrong with the dispositional view is that it was brought into the analysis at the wrong place; what the dispositional account may be used to achieve is to say when two people are following <u>one</u> rule. We can say that two people <u>A</u> and <u>B</u> are following one rule governing a sentence *S* iff they would be disposed to give the same response to a question based on *S*, 'S ?', under roughly similar circumstances. Spelling this out fully would take some time but I shall make two comments. First, one response by <u>A</u> which differs for <u>B's</u> would not show that they are following different rules - some allowance has to be made for 'mistakes'. We can say that they are following the same rule if either <u>A</u> or <u>B</u> would withdraw their response when requeried, they had been given time to reevaluate the evidence etc. Second, 'similar circumstances' must include their intention to respond sincerely. Note that I have so far made no assumptions that violate the rule-following considerations: the sceptic did not deny that people do have dispositions to respond in similar ways under similar circumstances, merely that that in itself does not justify one way of responding over another. Against this background,

imagine that I say to an English speaker of my acquaintance, 'Come into my room and see my new desk.' When we get into the room however, the vistor, call him W, says 'But there isn't a desk in this room.' Futher suppose that the vast majority of English speakers would be disposed to assent to the question, 'is there a desk in that room ?' Now there could be many reasons why W would deny that there is a desk in the room: W might have taken LSD and not perceive in the 'normal' way; W might be playing a philosophical game to see what my response will be. These hypotheses would be unusual, caused by unusual circumstances, but suppose something more bizarre is going on. Suppose W is following a qunus like rule for desks. The day before W would have agreed that there is a desk in the room but after that time W thinks that there are no more desks, only quesks. The question now is whether there would be any more justification for one way of responding rather than another. I think that we need to distinguish the claim that the choice of rules may only have a pragmatic justification from the claim that no question of truth or falsity arise. Suppose that a quesk is merely a desk after time t. In those circumstances W would have a true belief that there is a quesk and I would have a true belief that there is a desk. However, W would have a false belief if he believed that my belief that there is a desk is false. If a quesk = a desk after time t, and if it is after time t, then there is a quesk iff there is a desk.

In chapter 1 I drew a distinction between the direct and the indirect pragmatic theory of truth: whereas the former includes pragmatic considerations in the process of attributing truth-values to statements, the latter keeps those considerations separate. This distinction cam be applied here: given my definition the term 'quesk' would seem to offer no advantages over the familiar 'desk', but some new term might well

have pragmatic advantages. On the direct approach one would say that the class of statements which employ the more pragmatically useful terms are truer; on the indirect approach one would say that statements employing the term 'desk' or the term 'quesk' may be equally true even if one set of statements does have advantages over the other. One way of viewing Wittgenstein's work might be to see it as taking something like the direct approach but there is a possibility of a more radical interpretation. On the more radical view, it is inappropriate to speak of truth or falsity at all; various uses of statements may have their role in our form of life, and we may call those statements 'true' which are most useful, but there can be no question of statements being true or false in virtue of a 'way the world is' (even in the impure and modest sense in which I would defend that phrase). On this radical view all notion of the idea that statements may be true or false independently of us has gone, subjectivism reigns. I don't wish to argue that the radical view is Wittgenstein's but I do want to indicate my attitude toward it.

When I began discussing the imaginary W asserting that there isn't a desk in the room, I said that W may have said that because he had taken LSD. Now it is that kind of case, rather than the contrast between 'desk' and 'quesk' where we need to keep a grip on the notion of statements being true or false independently of us. When W asserts that there is no desk he may perfectly well understand what a desk is etc., and when the effects of the drug have worn off he may well concur that there is a desk. Now it is true that without a background of pragmatic interests the concepts of truth and falsity would have no applicabilty (without interests in writing etc. we would not have the concept of a desk. This theme is taken up in chapter 8) but it is vital that we retain the idea that we can't make any belief true that we might want

to make true - it is vital for pragmatic reasons. When critics of the pragmatic theory of truth have complained that it mistakenly identifies the true with the useful they have overlooked the fact that one of the most useful things about truth is that we can't make any statement true at will. Attempting to make one's theories 'true' by ignoring evidence, for instance, is not likely to be a pragmatically useful thing to do since one's theories will soon cease to work.

On my view, and on Wittgenstein's, it is not community-wide agreement on the use of statements which makes them true; on my view community-wide agreement only provides necessary conditions for statement to be true or false. In the introduction I said that Wittgenstein's latter work tottered on the brink of subjectivism; I don't believe that Wittgenstein commited himself to either the radical or the non-radical views discussed, but if Wittgenstein did favor the radical view I should say that his work actually fell into subjectivism. Given Wittgenstein's official policy of non-revisionism one might argue, although I won't develop the point, that this would make his views inconsistent since the concept of truth plays a vital role in our 'form of life'.

To say that statements may be true or false independently of our knowing which they are invites the question of whether one can say more than the unhelpful, 'well, they are true or false according to whether they correspond to reality or not.' I believe that one can, and to begin to show how, I define the truth-conditions of a sentence as follows: the truth-conditions of a sentence S are those conditions such that if someone could discover that those conditions obtain S would be true; or those conditions such that if someone could discover that they do not obtain S would be false.

My definition of truth-conditions employs the modal auxillary

'could' and this proves essential to my ambitions. By employing modal idioms I am able to endorse the view that there are verification transcendent sentences (i.e. sentences which have a truth-value that we cannot discover) and fallibilism. I shall discuss these points at some length (modality in chapter 4; truth-conditions and verifiability in chapter 5) but the crucial point at this stage is why my endorsement of verification transcendence is not also an endorsement of 'joints'.

I allow a gap between our knowing what the truth-conditions of a sentence are and our knowing whether those truth-conditions are satisfied Now a 'joint' theorist will also allow that gap but the difference between us concerns the way in which the truth-conditions of a sentence may fail to be satisfied. On my view a sentence can only be false for reasons which are, or could become, conceptually available to us. My talk of 'conceptual availability' is a way of insisting that it is not sufficient to say that a sentence may be false because it 'fails to correspond' to reality. If someone claims that a sentence I believe is true is really false because it 'fails to correspond to reality', and that is all that they can say, I would not be overly concerned. If they say that my belief may be false for reasons I have never entertained I would concur, but so long as no further chacterization of how my belief may be false has been given I would have no reason for abandoning my belief. By way of further explanation, note that a 'joint theorist' must be commited to something stronger than the claim I have emphasized. The intuitive picture of inquiry supposed that prior to sentient beings the world already contained an 'ontology', a purpose of inquiry being to discover that ontology. Now imagine a sentence of our language with an ontological commitment to Xs. Suppose that we have a whole range of beliefs about what Xs are and that all of those beliefs are true. If we

take 'joint theory' seriously then we must suppose that even that sentence may be false because Xs may not have figured in the pre-existing ontology. That is, suppose that S is the sentence committed to Xs, S is true, and that we will never have reason for believing S to be false; in those circumstances it seems to me that S is true (period), but the serious joint theorist must allow that S may be false - Xs may not have existed before sentient beings. The short shrift I give to that view - to deny that strictly speaking there was an ontology prior to our conceptual scheme looks radical and bound to entail subjectivism. In fact I think it merely rids of us some unwelcome mystery, in particular it rids us of the need for a mysterious correspondence relation. By way of brief reassurance that this is the case note that I spoken of sentences being true or false for reasons which could become available to us. It is likely that some of the beliefs that we now have are false for reasons we have never entertained. Putnam imagined the possibility of our one day discovering that pencils are organisms[20] - subject to the question of open-texture (discussed below) that discovery might lead us to say, much to our surprise, that there weren't any pencils after all. Talking of that example has made it 'available' but of course there are many other bizarre ways in which our current beliefs might turn out to be false - ways that we have never entertained. Note that the 'joint theorist' can never explain to us ways that our current beliefs may be false in a way that this view cannot accomodate - the explanation would make it available. I do not deny that a sentence such as 'There were mountains prior to the existence of human beings' makes sense and asserts something important, but I believe that its analysis calls for subjunctive conditionals.

(3) Linguistic Contexts

(a) 'Contexts'

Truth and ontology, on the view advocated here, are said to be relative to a linguistic context: liguistic because I take sentences to be truth-bearers (discussed below). The point of talking of contexts is less easily captured but of some importance. I may best begin to explain this concept by discussing Quine's paper "Two Dogmas of Empiricism". 'Two Dogmas' contained not only an attack on the analytic/synthetic distinction but a highly condensed sketch of an alternative philosophy ('empiricism without the dogmas') which would not rely on the concept of 'meaning' that Quine found objectionable. I shall consider the epistemological ramifications below; first I want to consider what the attack on meaning amounted to and its implications.

Quine distinguished two classes of statements which philosophers had called analytic. The first class, of logical truths, consisted of those statements which remain true under all reinterpretations of the non-logical vocabulary. The second class comprised those statements which can be transformed into logical truths by the replacement of synonyms with synonyms. Quine's attack was primarily directed at the second class of statements but the outcome of the attack led Quine to suggest that even statements of the first class were revisable. Quine's strategy was to argue that no satisfactory account could be given of the second class of statements, or of the notion of synonmy upon which it rested. The difficulty was that any attempt to explain an unclear concept needed to define the second class of statements needed to appeal to equally unclear concepts: analyticity, synonomy or necessity. The second dogma that Quine attacked was the dogma of 'reductionism':[21]

"...the belief that each meningful statement is equivalent to some logical construct upon terms which refer to immediate experience." Quine referred to radical reductionism as the view that,[22] "Every meaningful statement is held to be translatable into a statement (true or false) about immediate experience." However, Quine detected reductionism in more liberal guises also,[23]

> "The dogma of reductionism survives in the supposition that each statement, taken in isolation from its fellows, can admit of confirmation or infirmation at all. My countersuggestion..is that our statements about the world face the tribunal of sense experience not individually but only as a corporate body."

The natural way of reading this passage is as an epistemological thesis and this reading gains support from the fact that Quine refers to Duhem's (1904) where Duhem had argued against the possibility of crucial experiments. However something else seems to be going on as well since Quine went on to say that,[24]

> "The dogma of reductionism, even in its attenuated form, is intimately connected with the other dogma - that there is a cleavage between the analytic and the synthetic."

And added that, "The two dogmas are, indeed, at root identical." It is possible to reconstruct an argument in favour of an holistic attitude toward meaning as well as an holistic attitude toward confirmation. In crude outline the argument might go as follows: if statements had meanings in isolation we should expect to find statements which are true or false in virtue of experience in isolation from all other statements. However, we know (from Duhem) that statements are not true or false in isolation. Therefore, statements do not have meanings in isolation.

The argument is crude partially because it is unclear exactly what statements 'having a meaning in isolation' amounts to. Moreover, if there is an argument for a kind of holism about meaning then there is the kind of objection raised by Hofstadter,[25]

> "...science requires the efficient use of statements. For this, they must have determinate possibilities of use. The capacity to be used determinately is just the sort of thing one thinks of as uniquely connected with statement's being meaningful or significant. Hence statements <u>should</u> be individually, uniquely significant 'in isolation'."

Before replying directly to that argument, consider what an holistic attitude to meaning might amount to. Quine objected, on grounds of clarity, to the idea of a statement being true 'in virtue of meaning'. One could try to sharpen the point by saying that that phrase invites the picture of a statement <u>having</u> a meaning. Now the idea of a statement <u>having</u> a meaning is certainly obscure but if we are to avoid the absurdity of denying that statements are <u>meaningful</u> what else can one say ? Roughly the alternative is to say that a statement may be meaningful because there are intersubjectively agreed patterns of usage of that statement in a language community. The idea, of course, is to attempt to get away from the mystifying picture of meanings as <u>things</u>. What, then, of Hofstadter's objection ? The first point is to agree with the normative <u>should</u>: if communication is to be possible at all statements must have at least roughly determinate possibilities of use. The second point, however, is that if we to understand how 'determinate possibilities of use' are possible we need not to reject an holistic attitude toward meaning but to embrace it. One can perhaps say that one should reverse Hofstadter's priorities: it is not a statement's 'having a meaning' that makes determinate possibilities of use possible, rather it is determinate possibilities of use that makes a statement meaningful. What has this to do with revisability ? Roughly, my view (if not Quine's), is that investigation of the world always faces two kinds of question: in the terminology of this section, one may face the question, is this sentence true ? A necessary condition for a statement to be true is that it should be meaningful and, according to the above, its being

meaningful amounts to there being an agreed pattern of use in the language community. If that is so, then there is always the possibility of raising the second kind of question viz. should we retain or alter that pattern of usage ? In other words, the truth or falsity of statements is never simply a brute (or absolute) fact for the result will depend upon the pragmatic question of whether or not to make other adjustments to our conceptual scheme. Most of the time we do not make those adjustments, and there are often good pragmatic reasons for not so doing, but that does not entail that that choice does not exist.

Before I attempt to clarify the relevance of the above discussion for my talk of 'contexts' it is worth making two points. First, I have adopted the same strategy in arguing against the idea of the 'given' as I have in explicating Quine, i.e. distinguishing two kinds of question: should we adopt this pattern of use/category system ?; and, given this pattern of use/category system, is this sentence true ? The second point is that there seems to me an underlying structural similarity in the work of the later Wittgenstein and Quine - an attempt not to reify meaning and an attempt to come to terms with the consequences of not doing so.

Statements are meaningful, I have said, because they have inter-subjectively agreed patterns of use; a linguistic context is just such any agreed pattern of use. It is the existence of linguistic contexts, in my view, that makes it possible for speakers of a language community to utter true or false sentences, and to make true or false existential statements about what does or does not exist. To talk of linguistic contexts is to immediately invite the question of how they may be individuated and I'm afraid that my answer may be disappointing: there is no better way of individuating them than that available to a

competent speaker of the language in question. The reason why there is no better way of individuating them is that the description one can give of what a speaker must know, if he knows what a sentence means, can be, at best, schematic and incomplete. I think it is worth dwelling on this point, doing so provides a good antidote to the 'picture' picture of meaning.

The first way in which the description of a speaker's competence can only be characterized schematically might be called the incompleteness of language. As an illustration, consider what someone must know if one can attribute the concept of a pencil to them. Three things immediately suggest themselves: a competent speaker must know (i) a pencil is a physical object; (ii) the general function of a pencil; (iii) that a pencil is an artifact. Consider (i). It is not just that it is difficult to characterize what someone must know if they know that something is a physical object (although that is true), at the margin it is unclear what a physical object is. Philosophers often have need (or feel they have need) to press language to its limit, i.e. to ask such questions as 'well, what is a physical object really ?'. The existence of such questions, and the difficulty of answering them, shows that language has a certain incompleteness - at the extremes the answers to such questions are not entirely determinate. The second way that a speaker's competence can only be characterized schematically concerns what one might call deviance. I take it that most of us believe a pencil left lying on a table, unaffected by any 'abnormal' physical forces, continues to exist when we leave the room. Suppose someone believes otherwise however: they believe that pencils come into and go out of existence according to the presence or absence of human beings. Suppose they only believe this of pencils. Does that person lack the

concept of a pencil, have a deviant concept of a pencil, or does that person have a deviant concept of reality ? Third, there is the question of vagueness. How large must a town become before it is a city ? When does a hill become a mountain ? Vaugeness is akin to, but distinct from, what Mellor calls conceptual imprecision. He points out that a precise definition of a tributary of a river could easily be given were it not for the fact that one wants to draw several logically distinct inferences: that the tributary is shorter than the river; that it has smaller volume flow etc. But,[26]

> "...as the two properties (of being shorter, of having smaller flow) do not always correlate, the term 'tributary' cannot always be applied without falsifying one or other accepted relation. Since 'tributary' is not a variable term, this vagueness of application cannot be removed by ascribing a definable imprecision to its value, as can be done with length, temperature...All that can be done with a non-variable term is to recognize that it is...an 'inexact concept' for which there are 'neutral candidates'. The term is made 'exact' by an arbitrary assignment of neutral candidates as 'positive' or 'negative' instances."

This phenomenon is akin, in turn, to open-texture. I suggested that someone who had the concept of a pencil should know that pencils are artifacts. What, though, if pencils turned out to be organisms - should we say that there weren't any pencils after all, or that pencils were different from what we took them to be ? Finally, there is the phenomenon Putnam drew attention to of the linguistic division of labour.[27] Someone can know that a pencil is a physical object, an artifact, and what the function of a pencil is. Other people,however, will also know that pencils usually contain graphite and still others will know that graphite is a crystalline allotropic form of carbon. The linguistic division of labour is liable to give rise to different lists of properties something will have if it is to be called a so-and-so,

The point of mentioning these linguistic phenomena is not to suggest (falsely) that in most cases it is not the case that sentences

don't have a perfectly clear meaning to most members of the language community. The point is rather that we should not think of the relationship of language to reality on the proposition/fact model. Talk of 'facts' may be innocuous but not when it conjures up the picture of a 'determinate part of reality' to which propositions must correspond. On that picture one can raise the absurd question, 'how can a vague concept or sentence correspond to reality which cannot be vague ?' The absurdity of the question does not show that we therefore need precise concepts to correspond to precise reality - it shows that we should forget about 'correspondence'. 'Vagueness' and 'precision' are properly applied to descriptions, not that described. This point appears so obvious that no one could have seriously entertained the idea that vague sentences cannot describe (non-vague) reality, yet I am not sure that something like this confusion doesn't underlie some worries about bivalence. (See below).

A linguistic context then, as I use the term, is an inter-subjectively agreed pattern of use of a statement by a language community. Whenever I make the claim that a given sentence is true relative to a linguistic context it is subject to the qualification that the case in point falls within that agreement. What I have in mind is this: if pencils did turn out to be organisms a decision would be called for, a decision which would result either in the conclusion that, contrary to what we previously believed, there weren't any pencils, or the conclusion that pencils weren't what we thought they were. The latter decision would preserve the truth-value of some of our sentences (those that asserted that there were pencils). I take it that which decision the community would make (different parts of the community might make different decisions) is not determined now. However, if the situation

arose, then subject to what future decisions might be taken, I take it that now either we have a false belief that there are pencils (because they are in fact organisms), or a true belief that there are pencils but a false belief about what pencils are. I take it that all of the linguistic phenomena discussed can be dealt with in a similar manner, ie. that we can make statements determinately true or false by appropriate decisions. In one way this example encapsulates much of what I want to say since (i) it provides a model of the relation of language to reality which does not presuppose a 'correspondence' relation; (ii) it illustrates how truth and falsity depend on an holistic relation, viz. how we adjust our 'categories' will affect which truth-value our sentences have; (iii) I take it that the reasons we would have for adjusting the categories one way rather than another would be pragmatic.

(b) Truth-Bearers

I do not propose to review the general state of the argument over the issue of truth-bearers. My justification for this is twofold: (i) that the arguments seem to be fairly inconclusive; (ii) a general review of the arguments against sentences as truth-bearers, showing them to be (at best) inconclusive is available elsewhere (see e.g. R.J. and S.Haack, 1970). I do, however, wish to make some general comments and consider two arguments against sentences as truth-bearers since these arguments are germane to my conerns.

The claim that sentence-tokens may be the primary bearers of truth ought to be distinguished from the claim that sentence tokens are the only bearers of truth. Throughout the thesis I speak of statements being true or false, where a statement is understood to be a declarative

sentence. That is a matter of convenience but I would not care to argue that statements defined as that which declarative sentences assert may not also be truth-bearers.[28] I am, however, somewhat sceptical conerning the claim that propositions are the primary bearers of truth. Generally speaking it seems to me that sentences have two features which argue in their favour: (i) since sentence tokens are concretely inscribed they are publically accessible and this fact at least augurs well for the possibility of explaining how intersubjective agreement in language use is possible. The difficulties, by contrast, of explaining how we 'grasp' propositions threaten to be insuperable. It is sometimes claimed that propositions have the advantage of being objective but this claim seems puzzling in the light of this observation.[29] Arguments that propositions are more objective seem to rest on objections which are easily defused. (ii) Sentences possess a gramatical structure that may be exploited in a definition of truth as, for example, in Tarski's work.

Having made those general comments I now wish briefly to examine two arguments against the claim that sentences may be truth-bearers. The first argument has been presented by Pitcher,[30]

> "Suppose someone said truly 'It is raining', so that what he said was true. He spoke or uttered the English sentence 'It is raining', but that is not what we want to call true. If instead of 'It is raining' he had said 'Il pluet' or 'Es regnet', then in the sense of 'said' in which what he said was true, he would still have said the same thing; but he would have uttered a different sentence. Therefore what he said, in the relevant sense - i.e. in the sense according to which what he said was true it is not the English sentence 'It is raining'...it is this element which all three utterences have in common - this same thing that is said in all three cases - that is the real bearer of truth, not the different sentences which the speakers happen to utter."

The trouble with this argument is that from the fact that more than one sentence can say something true it does not follow that none can, i.e. just because one need not have uttered 'It is raining', one could just as well have uttered 'Es regnet', it does not follow that 'It is raining'

is not true or false. I mention this point since I can employ it to clarify my definition of truth-conditions of a sentence. I defined truth-conditions thus: the truth-conditions of a sentence S are those conditions such that if someone could discover that they obtain S would be true, or those conditions such that if someone could discover that those conditions do not obtain S would be false. The relevance to the above argument is that the person who discovers that the truth-conditions of a sentence obtain need not know the language of which S is a sentence. A German speaker who is unfamilar with English can discover that the truth-conditions of the sentence 'It is raining' do obtain although, of course, he or she won't know that they are the truth-conditions of that sentence. I don't believe that this view violates the 'rule following considerations' discussed in section (1). The point of those considerations is that there must be a language community if the attribution of concepts is to make sense. On my view there must be a language community if the attribution of true or false beliefs is to make sense. That is another way of saying that it is not the physical isolation of an individual that is important. In principle we could observe, without interacting with, a Robinson Crusoe whose non-verbal behaviour could exactly resemble that of a speaker of our language community to whom, and on the basis of which, we would unhesitatingly attribute a certain belief. In those circumstances I think we would be entitled to attribute that same belief to the Robinson Crusoe even though we may not know what, if any, language community he was from. Arguably something like this goes on when attributing 'beliefs' to animals. The point is that in some sense we would, to some degree, be treating them as a member of our language community.

The second objection to sentences as truth-bearers that I wish to consider has been succintly stated by Cargile,[31]

> "Before there were any languages or sentences or words or letters it was true that two plus two equals four. Since this truth existed before there were any sentences, it could not be a sentence. That two plus two equals four is a proposition, and all the linguistic changes in history will not affect it."

Assuming that one wants to uphold the view that two plus two equals four was always true, what can the upholder of the claim that sentences are truth-bearers say about this argument ? One thing they can say (I'm not sure that it is the only thing they can say) is that two plus two equals four was always true because if anyone had been able to utter the sentence '2 + 2 = 4' at any time they would have spoken truly. That way of meeting the argument allows both the concept of internal justification (we never get outside our conceptual scheme) with the thought that 'truth' is not simply invented. Similarly, the claim that mountains existed before language was invented can be handled by saying that the sentence 'There are mountains' would have been a true sentence if someone could have uttered it before they in fact could.

Finally, it is worth noting something else Cargile says. He points out, rightly in my view, that positing propositions cannot help <u>explain</u> what it is for two people to believe the same thing, or to <u>explain</u> what it is to understand a sentence. He adds,[32]

> "...we account for someone's understanding a sentence in terms of his being able to use it correctly, to answer questions about it, and such like...To say that propositions existed before language is not to say that our understanding of propositions is not entirely dependent on language. Anyone who undertakes to explain what it is to grasp a proposition or a concept or property is bound to be struck by the fact that ontological assumptions about the existence of these entities will not be helpful in this task."

But when one adds the thought that 'truth' and 'ontology' are also words of our language and can therefore, presumably, be explained in same manner, what need propositions ? This relates to a typical Jamesian thought: even if there are 'things' and 'truths' which cannot be captured within our conceptual scheme they ought, for that very reason,

to be of no concern. What lies forever beyond our conceptual scheme is of no interest.

(4) Fallibilism, Logic and Truth-Conditions

In the concluding pages of "Two Dogmas of Empiricism" Quine claimed that,[33] "The unit of empirical significance is the whole of science." However, Hofstadter asked,[34]

> "In what way....does my discovery that I have forgotten whether I have boarded the train at 12:12 or 12:13 affect the sunspot theory of economic crises ? Is it not a dogma to suppose that the whole of the language of knowledge is involved in every cognitive decision ?"

That objection seems to me correct and was, I believe, accepted by Quine.[35] From the rejection of the idea that a statement can be tested in isolation it does not follow that the whole of science is involved in every decision; between the two extremes there is the possibility, as Hofstadter put it, of a 'group' or 'island' theory of the relation between evidence and assertion. My concept of a 'linguistic context' is intended to be just such a 'group' or 'island' theory as I have intimated and will explain further below. My second reason for adopting a more limited form of holism springs from my acceptance of pluralism: one of the things that distinguishes one linguistic context from another is that they may serve different purposes. The importance of this point will be discussed in chapter 3.

(a) Fallibilism and Logic

The epistemology sketched at the end of 'Two Dogmas' contained two complementary slogans: 'any statement can be held true come what may', and 'no statement is immune to revision'. Quine included logic within the scope of fallibilism,[36]

> "Revision even of the logical law of the excluded middle has been proposed as a means of simplifying quantum mechanics; and what difference is there in principle between such a shift and the shift whereby Kepler superseded Ptolemy, or Einstein Newton, or Darwin Aristotle ?"

I will return to the first slogan presently but first I want to raise the question of whether there might be reason for qualifying the claim that no statement is immune to revision. Fallibilism, as Haack points out in her (1978a), can be predicated either of statements or of agents. Statement fallibilism is the logical thesis that some statements are possibly false, whereas agent fallibilism is the epistemological thesis that we are liable to hold false beliefs. The importance of this distinction is that it shows that even if the statements of logic are necessary truths that fact does not entail that our logical beliefs are infallible,[37]

> "...even if the laws of logic are not possibly false, this by no means guarantees that we are not liable to hold false logical beliefs. In claiming that we are fallible in our logical beliefs... I am not, of course, asserting the contradictory thesis that though say, 'p v -p' is necessary, we might falsely believe p v -p; rather, I am claiming that, though 'p v -p' is necessary, we might falsely believe that -(p v -p), or else, perhaps, though 'p v -p' is not necessary, we falsely believe that it is."

For that reason, even if logical laws are necessary truths this fact does not threaten fallibilism with respect to logic. Are the laws of logic necessary truths ? Suppose that some law of logic, call it L, is said to be necessarily true. Two attitudes that might be taken toward L are either to regard it as a profound metaphysical truth or to regard it as an utter triviality. (This latter view was that often taken by the positivists). I can begin to explain my attitude by saying that I find neither of these attitudes is very helpful. The first attitude suggests, wrongly in my view, that a logical truth is a metaphysically general description of reality, whilst the latter view suggests that there is some way of dividing the trivial from the non-trivial truths. My attitude is this: I think it is possible that there are statements

which we might never have good reason for giving up. In view of the controversy surrounding its status the law of excluded middle is not a likely candidate for such a role but the law of non-contradiction might be. To explain my reasons for taking this view it will be helpful to return to the first slogan that 'any statement can be held true come what may.' (It may be worth noting in passing that the statement I have emphasized is the nearest I come in the entire thesis to needing necessity).

Lewis wrote,[38]

> "...the law of excluded middle formulates our decision that whatever is not designated by a certain term shall be designated by its negative. It declares our purpose to make, for every term, a complete dichotomy of experience, instead - as we might choose - of classifying on the basis of a tripartite division into opposites (as black and white) and the middle ground between the two. Our rejection of such tripartite division represents only our penchant for simplicity."

One way of interpreting Lewis's view is by way of an analogy with a filing system.[39] Suppose that one has a filing system that consists of the three categories A, B, and the miscellaneous. One could understand the 'miscellaneous' in either an unrestricted or an restricted sense. In the former case, anything which is not an A or a B belongs to the miscellaneous. In the latter case, if for instance, As and Bs were two kinds of screw, what would belong to the miscellaneous would be any kind of screw not of the A or B kind. Now both of these filing systems are fallible in the sense that items may be misclassified but there is also a sense in which both are infallible. In the unrestricted case nothing can exist which cannot be classified. In the restricted case no kind of screw can exist which cannot be classified. What has this to do with logic ? Consider, for instance, the law of excluded middle (LEM). We may or may not choose to adopt a logical system which incorporates that principle. The analogy is this: just as adopting either the restricted

or unrestricted filing system may prove more or less useful, so adopting a logical system with or without LEM may prove better or worse, but in neither case can reality force us to adopt one rather the other. There is the suggestion that LEM can be 'held true come what may' because whatever reality dishes up we needn't reject LEM because we can adjust our categories in such a way as to preserve it. There is also the suggestion that just as we needn't worry about the apparent a-priori claim that nothing can exist which can't be classified in a filing system with a 'miscellaneous' category because it prejudges nothing at all, so we may choose LEM or not because that choice prejudges nothing at all.

I said above that there may be reason to qualify the statement that 'no statement is immune to revision' since we may find that there are some statements which we have no good reason to give up. If the revisability thesis is modified in this way it makes explicit the pragmatic reason for so doing.

When first raising the question of the necessity of logical laws I said that I thought it a bad idea to suggest that the grounds of logical necessity lay in the fact that logical laws are metaphysically general descriptions of reality. I said that because (changing the example slightly) it invites the question, 'isn't it risky for someone to propose adopting a bivalent logical system since reality may turn out not to be bivalent ?' Put so crudely perhaps the mistake is obvious: this would be the same worry that vague sentences cannot describe reality because reality is not vague. I can't help feeling however that some such thought does underlie some concern about either bivalence or LEM. I also suggested that we should reject the idea that logical laws are necessary because trivial. One possible response to that might be

to say, 'you are suggesting that the justification for logical laws would be the same as the justification for using a filing system, i.e. if you want to do some filing you have no choice but to use some sort of filing system and that claim is trivial.' I am prepared to concede the word 'trivial' if need be but the important point has been to explain (or, better perhaps, to explain away) logical necessity, without appeal either to the dubious category of the self-evident or the analytic/synthetic. Perhaps it is worth repeating that I remain a fallibilist with respect to logic because even if there are statements presupposed by any inquiry I have no way of identifying them. (Agent fallibilism holds even if statement fallibilism doesn't). Perhaps I ought to add that I don't necessarily reject the idea that logical truths may just be highly general descriptions of reality depending upon how one understands that statement, viz. provided (at least) that one doesn't think a logical law could be false simpliciter because it 'fails to correspond to reality'. I don't believe that is true of any statement, be it as 'empirical' as one will. Perhaps I also ought to add that I don't consider that I have argued either in favour of or against classical or deviant logics (since that would involve many more issues than I have considered), I have merely tried to suggest that one sort of reason for rejecting bivalence or LEM is a a very bad reason indeed. It seemed important for me to make this point since it is also a way of saying that I see no radical difference in kind between 'empirical' and 'logical' truths.

(b) Fallibilism and Truth-Conditions

I defined the truth-conditions of a sentence S as those conditions such that if someone could discover that those conditions obtain S

would be true, or those conditions such that if someone could discover that those conditions do not obtain S would be false. To explain further how this definition is intended to work I will return to the sentence 'There is a table in that room' imagined to be uttered to a particular person on a particular occasion. What would a person have to know if they were to know that the sentence is true ? A part of what they would have to know would be what kind of experience they could expect under normal circumstances. They would know, for instance, that if there is a table in that room then they could have certain visual and tactile experiences although they need not know exactly what kind of experiences to expect, e.g. they might not know the colour or of what materials the table is made. (I have also assumed that the person spoken to is 'normal', e.g. not blind). I spoke of 'normal circumstances' since a speaker would have to know that if they were to enter the room blindfolded the fact that they could not have the appropriate visual experiences would not show that there wasn't a table in that room. (Perhaps I can employ that point to clarify the motivation that underlies my talk of 'linguistic contexts' and an holistic attitude toward meaning. Asked for a definition of a table I take it that it would occur to very few people to say that a speaker would have to know that a table can exist even if one is blindfolded. Is that rather something one has to know about the category of 'physical objects' ? But a table is a physical object. Dictionaries are only as successful as they are because of a massive amount of background information we normally take for granted. Taking an holistic attitude toward meaning is taking the view that even in principle there is no definite amount of information one could list which simply gives the meaning of a term or a sentence. That, in turn, is the reason why linguistic contexts cannot be individuated in any better way than that available to a competent speaker). It may or

may not be possible for the person in question to be able to verify whether the experiences they would expect to occur would occur, but in any case someone who understands the sentence 'There is a table in that room' knows that if true it entails unverifiable facts. It is essential, in my view, that one allows that unverifiable sentences may nevertheless have a determinate truth-value. That the sentence 'There is a table in that room' may be true or false although some of its entailments are unverifiable is a claim I wish to defend subject to the problems raised by the various linguistic phenomena I mentioned above (i.e. incompletenes open-texture, vagueness, deviance, conceptual imprecision and the linguistic division of labour). Take open-texture. I suppose that a broken table may still be a table, which is to say that someone may enter the room, see a broken table and still assent to the sentence 'Is there a table in that room ?' However, at some point a table broken into sufficiently many pieces ceases to be a table and there is, presumably, some point at which competent speakers would disagree about whether there is a table or not. Call the continuum from the point where all competent speakers would agree that there is a table, to the point where only one competent speaker would say that there is a table in the room, the range of application of the concept of a table. Call the experiences which give rise to universal agreement on the part of competent speakers of the language community the central range of application of a concept.[40] I can now state my central claim as follows: a declarative sentence has a determinate truth-value provided that the concepts employed fall within their central range of application.

One important feature of my view may best be brought out by varying the example. In his (1959) Dummett discussed the sentence, 'A city will never be built on this spot.' (Call this sentence C).

Suppose that that sentences is uttered by some speaker referring to a particular place. Dummett was worried about whether C could be said to have a determinate truth-value now since it appears that there is nothing in virtue of which C is either true or false. I take it that if uttered on a particular occasion there may well be no way of telling whether C is true or false. I want to say that C either is true or false provided that we set aside worries that might arise over exactly what counts as a city etc. (which weren't the worries that Dummett had about the case). The crucial point from my point of view is that I claim that although in one sense the truth-conditions of C are inacessible - the sense in which we may not have any of telling whether C is true or false, in another sense the truth-conditions of C are accessible. The truth-conditions of C are accessible in the sense that a speaker of English knows what would make C true, viz. a city never being built, and an English speaker knows what would make it false, viz. a city being built. I claim that provided that the concepts concerned fall within their central range of application C has a determinate truth-value now. These claims make it look as though I am simply coming down on the realist's side in the recent realism vs. anti-realism debate. To explain why that is not the case it will be useful to consider a statement made by McGinn. He writes,[41]

> "Realism is the thesis that truth (falsity) is an epistemically unconstrained property of a sentence; there is nothing in the concept of truth (falsity) to exclude the possibility that a sentence be unknowably true (false). This property reflects the realist conviction, embodied in our customary linguistic practices, that the world, or a given sector of it, is determinately constituted quite independently of any limitations on our capacity to come to know truths concerning it."

Given what I have already said it is obvious that I accept that truth may be an epistemically 'unconstrained property of a sentence', i.e. that sentences may be unknowably true or false. Where I dissent from many realists is in how I understand this to be possible. I dissent

at least from some of the implications of the second sentence of the quotation from McGinn. Take some sentence S. Suppose that we believe that the truth-conditions of S are satisfied and that the truth-condition of S are satisfied. On my view S is true and that is the end of the matter (given that one accepts the vocabulary). Strangely enough however other philosophers have wanted more than this. McGinn talks of the world being 'determinately constituted quite independently of any limitations on our capacity to come to know truths concerning it.' That phrase is pernicious if it leads one to say, 'yes, I know what the truth-conditions of S are, and I am prepared to believe that the truth-conditions of S are satisfied, but I also want to know whether the truth-conditions of S correspond to the determinately constituted way-the-world-is.' That last question makes no sense on my view. Perhaps I can put my view by saying that although I believe that truth is an epistemically unconstrained property of a sentence it is not a conceptually unconstrained property of a sentence. Phrases which talk of the 'determinate structure of the world' are, I believe, ambiguous, an ambiguity reflected in different ways of understanding what the truth-conditions of sentences are. Consider this apparently straigtforward definition of truth-conditions:

the truth-conditions of a sentence S are those conditions in which S is true or S is false.

I don't believe that that definition is incorrect since it is neutral between what one might call an absolute and a relative conception of truth-conditions. An absolute view goes something like this: the world is determinately constituted independently of us and our capacities to come to know it; we are subject to all sorts of limitations and it is therefore possible that the most profound truths about the world may not be conceptually available to us - that is to say that it is possible

that we may be unable to form the concepts required to discover the most profound truths about the world. The absolute view sees truth as a relation between truth-bearers and unconceptualized reality. The relative riposte to all this is to say that what is unconceptualized is of no interest so long as it remains unconceptualized. The relative view sees truth as a relation between the truth-conditions of sentences and their satisfaction - a relation which does not invoke the idea of 'unconceptualized reality'. On the relative view inquiry has two basic aims: to discover whether our conceptualizations are satisfied and to invent new conceptualizations which will achieve the fulfillment of our cognitive interests in a pragmatically better way. To say that we always work within our conceptual scheme is not to say that we are trapped within it.

My definition of truth-conditions speaks of inquirers being able to discover whether certain conditions obtain. Now I see no reason for saying that we can ever be <u>certain</u> that the truth-conditions of a sentence <u>are</u> satisified. However, I don't wish to rest content with the idea that a sentence may be false because it 'fails to correspond to reality' (whatever that may mean). Rather, I want to say that a sentence now believed true may turn out false for reasons we have never entertained On my view a sentence everyone, everywhere accepts as true may yet turn out false, but to say that is to say that we might discover reasons we have never imagined for believing it false. Thus I see a relevant difference between saying that a sentence may turn out false for reasons which in fact may never be discovered (but which we would understand and accept if they were discovered) and saying that a sentence may be false for reasons we could never even understand. Moreover I claim that those who take the idea of 'the world determinately constituted independently of us' <u>seriously</u> must be commited to the idea that a

sentence may be false for reasons we could never even understand - that is so because that supposed independent structure (if it really is totally independent of us) may not be 'describable' by our concepts. To reject this concept is not, I believe, to risk making any a-priori judgements about how the world may turn out - it is merely to reject an unintelligible concept of truth. My manoeuvre steals the realists' defence: if they explain how a sentence may be false they ipso facto make it available for this view; on the other hand, if they merely keep saying that a sentence may be false for reasons we cannot even comprehend I think we should say that of that hypothesis we have no need.

Conceptions of truth-conditions which are neutral between absolute and relative versions are, I believe, to be found in Tarski's definition of truth and Davidson's truth-conditional semantics. That neutrality has led some to read an absolute version into their work (Popper on Tarski; Platts on Davidson). I shall return to this point in chapter 5 where I shall also defend my claim that there is an important way of cutting across the recent realist vs. anti-realist debate. The essential reliance I have placed on modal idioms will be defended in chapter 4.

Chapter 3

Philosophy and Ontology

There are, I find, important and complex relationships between the question of what philosophy can and cannot properly aspire to achieve (one's overall metaphilosophy) and what role, if any, philosophy has to play in answering the question 'what is there ?' To explain my views on these matters I shall distinguish my views from those of Wittgenstein, Quine and Goodman. (See figure 4 overleaf. In view of the illustrious company I have not placed my name over the column on the extreme right hand side which represents my views). I have found it necessary to distinguish five issues over which there is dispute. The most general and surprising feature of the relationships between these views is how favouring one answer to one question may radically affect the whole tenor of the overall view which emerges. Thus, my conception of the nature of philosophy starts out from a much more optimistic premise than would appear to be acceptable to Wittgenstein, yet by the time my view is processed by means of the answers I give to the other questions the differences seem far less radical than one might have expected. Conversely, my position as described only differs from Goodman's in one category but that difference is of some importance. In this area of philosophy I find myself as concerned and worried about the views of my philosophical allies as I do my philosophical enemies.

I should stress at the outset that although the views discussed are bound to have implications for the possibility of metaphysics in general, I have explicitly restricted myself to the question of what philosophy may or may not achieve with respect to the question of ontology. In particular, as will become apparent, the distinction I

Figure 4

Some Views of the Relation between Philosophy and Ontology

	Wittgenstein	Quine		Goodman	
Meta-philosophy	therapeutic	meta		meta	meta
Aim	descriptive	discovery		explanation	explanation
'Joint-theory ?'	No	No	Yes	No	No
Monist or pluralist ?	pluralist	monist		pluralist	pluralist
Revisionary or non-revisionary ?	Non	Non		rev.	Non

draw between revisionary and non-revisonary ontological conclusions concerns a narrower issue than that raised by the distinction Strawson drew in his (1959) between descriptive and revisionary metaphysics. I will explain the terms employed in figure 4 as I proceed but some general remarks may prove helpful.

There are, it seems to me, important similarities and tensions between the philosophy of Quine and the later work of Wittgenstein. In chapter 2 I said that an important aspect of both philosopher's work was the attempt to rid us of the tendency to reify the concept of meaning. That both philosophers had some success in this regard is a formidable achievement since it is not easy to persuade others that there is something wrong with a view that seems obviously correct. There is also some sort of agreement that a certain kind of metaphysics is untenable and illegitimate but to say that is to immediately draw attention to their differences - Wittgenstein seemed to regard all metaphysics as illegitimate and Quine does not think that. These differing views of what philosophy can achieve seem to be linked to the issue of pluralism. In Wittgenstein's later work there seems to be important connections between the idea of a 'language game' and 'forms of life', at least one of those connections would seem to be that different language games may serve different purposes. That view seems to find no echo in Quine's work. I find myself in tension with these interesting tensions: on the one hand I do believe that pluralism is philosophically important but, on the other hand, I have more sympathy with Quine's more optimistic view of what philosophy can achieve. Having said that however, I do find that Quine attaches an importance to what philosophers say about ontology that I do not. I have attempted to capture this difference of attitude by my distinction between

'discovery' and 'explanation' discussed in section 3. (Perhaps I ought to add that it is that distinction which motivates the need for question two in figure 4, i.e. I don't attach any great weight to the distinction between Wittgenstein's therapeutic metaphilosophy and the aim of philosophy as 'descriptive'. The term 'descriptive' merely fills a gap so to speak).

Quine's views as found in his (1951a) seemed to me to be an explicit rejection of 'joint theory' and were a source of 'inspiration' for my attempt to develop a pragmatic theory of truth. On the other hand, I think that a very reasonable interpretation of much of Quine's writings (especially, perhaps, after Word and Object) make it seem more like a fulfillment of 'joint theory' than a rejection of it - hence the two positions marked in figure 4.

(1) A Meta-metaphilosophy

Quine's rejection of the analytic-synthetic distinction implied the rejection of a metaphilosophy that had acquired some popularity in the 20th century - the view that philosophy was primarily conerned with conceptual analysis. That rejection made possible a more optimistic view of what philosophy might legitimately achieve since if the distinction between 'conceptual' and 'empirical' change was, at best, unclear, could not the philosopher, in principle, make some positive contribution to the communal enterprise of inquiry ? If philosophy is not to be characterized as being primarily concerned with conceptual analysis, how should it be characterized ? A clue to an answer might be found where Quine speaks of the[1] "...rejection of a first philosophy, somehow prior to science." One way of construing this is to suggest that philosophy must begin by reflection upon other areas of inquiry -

it cannot begin with a-priori reasoning about the nature of reality (or anything else). Philosophy then might be seen not as an inquiry superior to science (as some metaphysicians had seen it[2]) but as a meta discipline, something like the 'inquiry into inquiry'. I think that this conception of philosophy is fundamentally sound but one should note some of the difficulties in making it precise. The chararacterizatio of philosophy as a second-order inquiry would not seem to provide a sufficient condition for the definition of philosophy since other disciplines may be said to work in this manner, e.g. literary criticism. Here I see no alternative, and little harm, in saying that philosophy is the meta discipline concerned with the investigation of historically given questions, e.g. those concerning knowledge, truth, logic etc. Thus I think it is not so much the questions that have changed in recent times as it is the way in which those questions are tackled. The meta view of philosophy obviously fits well with the 'philosophy of ....' schema: 'philosophy of science' etc. To say that, however, immediately draws attention to two central areas of philosophical inquiry that don't fit that schema, viz. epistemology and metaphysics.[3] I don't think that that fact should lead us to reject the meta conception of philosophy - I think rather that epistemological and metaphysical questions should be thought of as concerning issues which are not specific to one discipline but cut across many. There is though, a more serious problem. On the view being suggested epistemology should be thought of as the attempt to explain how other areas of inquiry may produce knowledge. Unfortunately, however, in identifying other areas of inquiry one cannot rely on the fact that they may not be described as philosophy. Thus the so-called ontological argument for the existence of God may be described as a theological argument and yet, in my view, is just the kind of a-priori reasoning that is illegitimate. (Here I

think that one cannot ultimately define one's metaphilosophy without having taken a substantive position on some philosophical views). I propose to put all my question-begging eggs in one basket, so to speak, by saying that epistemology is the attempt to explain how legitimate areas of inquiry may lead to knowledge.[4] Anyone who wished to defend the ontological argument for the existence of God would rightly object that this begs the question but here, as I said above, I am more concerned with the views of my philosophical allies who would not, I think, dispute that the areas of inquiry I take to be legitimate are legitimate.[5]

The picture so far then is this: there seems to be a reasonable conception of philosophy as the inquiry into inquiry. Moreover, although it is difficult to draw the first-order/second-order distinction clearly or precisely it does not seem to be a picture which could legitimize either wholesale armchair metaphysical systems or arguments which cannot in some way be based on an appeal to experience. It seems to me an attractive picture in that it defines a role for the philosopher in the community of inquirers without supposing that the philosopher is the final arbiter. (It could be said however, that a less flattering description of this conception of philosophy is that philosophy is an essentially parasitic discipline. I agree that that description equally fits what I have said - indeed I should say that, for reasons I shall come to, as applied to the question of 'ontology' the less flattering description is the more appropriate). This rosy picture faces a tough question however: does the philosopher keep the books ? That is to ask whether the philosopher, after studying the results of other areas of inquiry, e.g. physics or mathematics, then sorts out what can and cannot properly be said to exist. The dilemma seems to be this:

if one answers 'no' we might be back to the idea that philosophy can only be therapeutic or descriptive after all; on the other hand to answer 'yes' threatens to allow back illegitimate a-priori considerations that the first-order/second-order distinction was intended to disallow in the first place. Dewey wrote that,[6] "Philosophy has assumed for its function a knowledge of reality. This fact makes it a rival instead of a complement to the sciences." I think that the feeling that philosophy ought not to be a rival to the sciences is a healthy one that, prima facie at least, the 'yes' answer would violate.

An important part of the motiviation for answering 'no' is a thought such as this: if scientists construct a theory which says that 'there are electrons' what possible expertise or reasons could the philosopher, qua philosopher, have for disputing that conclusion ? The thought that philosophers would be engaged in an illegitimate enterprise because they would be stepping beyond their realm of expertise immediately suggests one reason for qualifying the 'no' answer however: scientists are entitled to question the work of other scientists, so surely philosophers are entitled to question the work of other philosophers. That sounds both reasonable and correct. Philosophers do occasionally posit various kinds of entities and surely philosophers are entitled to query whether that is the best way forward. Philosophers have been led, by their analysis of modal statements, to posit 'possible worlds', a procedure I shall challenge in chapter 4, so I do believe that that is a legitimate activity.

A second reason for qualifying the 'no' answer might be this: philosophers who questioned the existence of electrons might not be stepping beyond their realm of expertize since they might not be questioning any of the facts that the scientist has discovered. That is

to admit that the philosopher, qua philosopher, could have no grounds for questioning scientific evidence as such and yet still have reasons for wanting to deny that a certain form of discourse should be thought of as commited to various kinds of entities. I take it an instrumentalist philosophy of science might be like that, i.e. without questioning the scientific evidence for various theories the instrumentalist might try to show that all of that evidence is compatible with the denial that there are unobservable entities. In the light of this observation I introduce the distinction between revisionary and non-revisionary conclusions. A non-revisionary conclusion would be one that cast no doubt on the legitimacy or the evidence of a given area of inquiry but claimed that compatible with that evidence there are reasons for asserting or denying that there are so-and-sos. A revisionary conclusion by contrast would be one which did cast doubt on the conclusions reached by the inquiry and claimed that either the evidence, methods, or results of the inquiry should be modified or rejected. Without saying that revisionary philosophical conclusions cannot ever be justified, I think it is reasonable to conclude that the reasons for the revisionary conclusion need to be compelling and their justification made apparent. (See below on Goodman).

(2) Independent Existence, Internal Justification and 'Joint Theory'

Consider the sentence,

(1) There is a possibility that James will come.

(1) has a prima facie commit ment to something called a 'possibility' and that commitment might be thought to be problematic. (I express myself this way to avoid question-begging). Now at least four ways of responding to this situation can and have been proposed. These are:

(i) qualify 'there is', i.e. argue that if there are possibilities they

don't exist in the same way as, say, tables. (Existence vs. subsistence).
(ii) deny that (1) really is commited to possibilities.
(iii) accept its *prima facie* commitment and assert that possibilities are *things*.
(iv) Claim that (1) may be perfectly meaningful if one can find some sentence equivalent to it which is not commited to the existence of possibilities. Perhaps 'It is not certainly false that James will come' is adequate.

I think that (i) creates more difficulties than it solves, so without arguing that this is the case I shall set (i) aside. I think that there may be reasonable motivation for (ii) and I shall return to this below. (iii) is, I think, obscure. Since the sentence in question concerns *possibilities* my views on this will be given in chapter 4. (iv) is basically Quine's way and it is the way I favour also but it is just here that the fun begins.

The approach to ontological questions *via* paraphrase may have quite different motivations for different philosophers and I wish to distinguish two sources of motivation:
(A) as a way of fulfilling 'joint theory';
(B) as a way of clarifying the truth-conditions of sentences.
Those who want to do (A) will also want to do (B), but those (like myself) who merely want to do (B) may not want to do (A). In other words (A) is a stronger position than (B).

'Joint theory', I said in chapter 2, is the intuitive picture of the world containing things and kinds of things prior to our arrival and the creation of our conceptual scheme. Now the paraphrase approach to ontology holds the promise of being a way of fulfilling that theory, i.e. after analy-izing various kinds of sentences, the ontological

commitments of those sentences we are forced to take seriously (those we cannot parphrase) will provide a list of at least potential candidates of things that existed prior to our arrival. I objected to 'joint theory' on the grounds that it required the correspondence theory of truth which, I believe, has little prospect of success. There are however two other reasons for objecting to joint theory: (a) it requires a very strong and obscure concept of what constitutes a 'thing' or 'entity'; (b) it requires a strong and obscure concept of 'independent existence'. These points are not independent in that a likely definition of an entity is as <u>something capable of existing independently of us</u>. Moreover I believe that 'joint theory' entails that certain philosophical issues must have a right or wrong answer where it seems to me very doubtful that they do. The essential reason for this is that if one thinks of there being things prior to our conceptual scheme then they must have been either universal or particular; concrete or abstract; possessed only primary qualities or both primaary and secondary qualities. I have choosen those examples since they belong to the metaphysical tradition which has attempted to separate the contribution of our conceptual scheme from the contribution of the world. It is the viability of that tradition that I attempting to bring into question.

Before proceeding with the prosecution case against joint theory it will be as well to say something about my own view. One objection to what I have already said might go as follows: 'you have cast doubt on the concept of 'independent existence' but unless you are an idealist you will need that concept yourself.' Now I do feel that I have a firm grip on what it means to say that middle-sized objects exist independently of myself and others. I have already said, however, that I understand assertions about the existence of physical objects <u>via</u> the claim that

a set of subjunctive conditionals is true. I have also said that one can interpret a claim that mountains existed before human language by saying that 'There are mountains' would have been a true sentence before it was. Those manoeuvres, in my view, avoid the need for a correspondence theory of truth and, more relevantly here, they avoid the need to decide either in favour or against universals/particulars; primary/secondary qualities; or, the categories of the concrete or the abstract. Those who believe that these are important philosophical issues will no doubt feel, as Russell put it, that my method has all the virtues of theft over honest toil. What I say below may go someway to pacifying such souls.

A 'joint theorist' may say that our intuitions about what it means to say that physical objects exist independently of us implies something stronger than believing true a set of subjunctive conditionals. I think that is probably true but it does not show that those intuitions are justified. Moreover, I am inclined to think that ontological questions have been bedeviled by those intuitions. Armed with the feeling that we understand what it is to say that physical objects exist there is the temptation to ask the question whether, say, numbers exist in that kind of way. Unconsciously we make an analogy between the question 'are there physical objects ?' with such questions as 'are there numbers ?'; 'are there theoretical entities ?' etc. when it is at least unclear whether or not the analogies break down at that point.

Returning to the various responses to the sentence,

(1) There is a possibility that James will come.

I said that I saw some reason for saying that the motivation behind strategy (ii) had some justification. (The strategy which says that (1) is not really commited to possibilities). One such view was expressed

by Alston. When comparing (1) with,

(2) It is not certainly false that James will come.

Alston wrote,[7]

> "...if the translation of (1) into (2)..is adequate, then they are normally used to make the same assertion. In uttering (2) we would be making the same assertion as we would if we uttered (1), i.e. the assertion that there is a possibility that James will come. And so we would be asserting that there is a possibility (commiting ourselves to the existence of a possibility) just as much by using (2) as by using (1)."

From other things that Alston says I think that a part of his motivation for saying the above lay in not wanting to countenance the motives that lay behind (A). One might say this: if one is not 'hung up' about the concept of a 'thing' or an 'entity' one might say that the paraphrase of (1) by (2) shows not that possibilities don't exist but rather tells us what they <u>are</u>. It will be clear that I agree with Alston that one shouldn't be 'hung up' about the concept of a 'thing' or an 'entity' and I also agree that it is a moot point whether the paraphrase of (1) by (2) shows that possibilities don't exist or tells us what they are. (The point is as moot as the concept of 'independent existence' is moot and that, in my view, is pretty moot. See below.) Perhaps Alston's worries would be met by my proposal to describe the purpose of paraphrase as being that of clarifying the truth-conditions of sentences. This way of viewing the matter still preserves one of the reasons Quine has for stressing the importance of paraphrase - to prevent <u>backsliding</u>. If a philosopher tells us that he or she is a nominalist and then promptly proceeds to quantify over numbers, he or she would at least owe us an account of the truth-conditions of the statements committed to numbers in order to show their position to be consistent. Such a view might be consistent pending an account of what numbers are. Thus although I accept Quine's strategy my motivation for doing so, on some interpretations at least, differs from his. To explain this I turn to the distinction between 'discovery' and 'explanation'.

(3) Discovery or Explanation ?

A representation of Quine's philosophy as fulfilling the aims of 'joint theory' would appear, prima facie, implausible. Quine has said many things which appear to support the theory of internal justification, a prime motivation of which is to avoid the need for 'joint theory' at all. That view is reinforced by statements such as the following,[8]

> "The fundamental-seeming philosophical question, How much of our science is merely contributed by language and how much is a genuine reflection of reality ? is perhaps a spurious question..."

However, by the time of his (1973) there would appear to be a volte face,[9]

> "...we ought to be able to see just to what extent science is man's free creation; to what extent, in Eddington's phrase, it is a put-up job."

Now I don't see any way of reconciling those statements but I think that there is a way of describing many of the things Quine has said as a theory which does not start out with the aim of fulfilling 'joint theory' but which would, if successful, represent a reasonable fulfillment of that theory. To articulate this way of viewing Quine's work I think it is helpful to contrast a conception of the aim of philosophy as that of discovery with a conception of the aim of philosophy as that of explanation. In his (1951a) Quine said that,[10] "Ontological questions are on a par with natural science." It would be an instructive exercise to discover how many ways that statement is ambiguous but the discovery interpretation may represent one meaning it may have.

In his (1976) Campbell explicitly describes and extends Quine's ontological programme as attempting to,[11] "...provide a reasoned inventory of the categories [of the things that there are] ." If successful that would certainly look like a completion of the 'joints' programme. Campbell raises the following questions,[12]

> "Are there any entities which are not things, or are concrete particulars the only items on a complete schedule of distinct categories of being ? What about events, spaces, periods of time, properties, numbers, relations, or tendencies - do all these reduce, somehow, to things ?...In asking about categories, we are asking about the basic varieties of existence."

To get to grip with the issues I think it is useful to have two lists.

Scientific	Metaphysical
atoms	universals
quarks	particulars
electrons	events
sets	sets
⋮	⋮

The difference between the lists is that although they have some items in common, some items only appear on the metaphysical list and some only appear on the scientific list. That reflects the fact that the existence of some entities are only discussed by philosophers, e.g. universals. The explanatory view suggests that we should approach ontological questions via the scientific list and only via the scientific list (subject to certain qualifications that will arise when I bring the issue of pluralism into the picture). The difference between the discovery and the explanatory approach concerns not only what items the existence of which are to be discussed but how and why that discussion arises. The discovery approach starts out, as Campbell does, with questions about what exists; the explanatory approach says that we should start out from questions of how we can explain the workings of a certain area of discourse or inquiry although that exploration may lead us to certain conclusions about what does or does not exist. This may appear an arbitrary distinction but I trust that its point will become clear.

One problem that immediately arises from the metaphysical list

concerns the legitimacy of a-priori reasoning about what does or doesn't exist. An important part of the motivation for describing the meta-metaphilosophy was that it is not obvious that there is a legitimate a-priori method for philosophers to discover what there is. The thought that lay behind the strategy of attempting to say that philosophers ought to restrict their attention to disciplines other than philosophy (with respect to ontological questions) was that the concepts involved would thereby be guaranteed some independent legitimacy. That thought is related to the question of what items the existence of which are worth discussing - it seems to me that it is quite possible that if one starts from the attempt to explain cognitive success in areas other than philosophy one might be able to resolve all of the problems without ever having to ask whether or not universals, say, exist. If that turned out to be the case I should feel no need to press the question. It is worth noting that my view does not entail that every claim and counter-claim in the traditional nominalist-realist debate is necessarily meaningless. It is likely that questions in the philosophy of language (questions which arise because of the attempt to understand how linguistic discourse is possible) will uncover problems concerning predication not entirely unrelated to the nominalist-realist debate, but the suggestion is that this approach is more likely to ensure that there is a genuine problem to be solved.

I said that the discovery approach differed from the explanatory approach not only in what items are discussed but how they are discussed. 'Sets' appeared on both lists. Now it seems to me that if one begins by investigating how mathematicians employ the concept of a set, how students acquire the concept of a set and so on, one may be led to want to say either that there are, or that there are not, sets. To begin from the question of existence seems very likely to lead to perplexity, e.g.

to such questions as 'how can anything as weird as the null set exist ?' (See chapter 7 for my views on the nature of mathematics). The difference in approach concerns a different ordering of priorities and leads me to the suggest the following meta-philosophical hypothesis: philosophical problems are rarely solved either by positing or denying the existence of various kinds of entities, but the solution of certain philosophical problems may have implications for whether one does or does not want to say that certain kinds of entities exist. Arising from that suggestion is the thought that the discovery approach tends to make the wrong kind of questions significant and the right kind of question insignificant. The first suggestion is, I admit, linked to my own theory of internal justification. Many of the questions that arise from the discovery approach have arisen because philosophers have wanted to be able to separate out what we contribute to our knowledge of the world from what the world contributes. That is patently what the distinction between primary and secondary qualities was intended to achieve and, only slightly less obviously, what the universal/particular debate was intended to achieve. I am inclined to agree with (the early) Quine that such a separation is not possible and hence that for that reason the discovery approach is overambitious. It is not the case however that the explanatory approach is less demanding for in certain respects it is more so. An example provides an illustration for my claim that the discovery approach makes the right questions less significant. Quine's view of abstract entities in mathematics can be summarized as follows: (i) science employs statements committed to abstract entities; (ii) there is no known way of ridding science of those commitments; therefore (iii) abstract entities exist. That position is perfectly reasonable in my view but it doesn't achieve what one would really like - it doesn't explain why the commitment to abstract entities in science should be

useful. It is not obvious, after all, what role abstract entities could play in a physical process.

The final contrast I would draw between the explanatory and discovery approaches is somewhat difficult to articulate and is linked to my admitedly cryptic remarks earlier that we should not take the notion of a thing too seriously. One might put the point this way: if joint theory were true then every claim of the form 'there are so-and-sos' or 'there are not so-and-sos' must have an absolutely correct or incorrect answer. That is because according to joint theory there are, prior to our conceptual scheme, 'things' and (perhaps) 'kinds of things' which exist independently of our conceptual scheme. By definition, that which exists independently of us cannot be affected by our descriptions and, hence, either the things we quantify over exist or they don't. The question of what exists must have an absolutely correct answer whether we can discover it or not. By contrast my attitude is that the question of whether to say that there are so-and-sos is always subject to pragmatic gerrymandering such that, whichever way it goes, can't be absolutely correct or not. (Early on Quine took a similar view when he spoke of the choice we have over which strand of the fabric of science to adjust). Thus, to return to the example cited earlier, I can sympathize with both claims when one side says that the truth of the sentence, 'There is a possibility that James will come' shows that there are possibilities; and I can sympathize with the other side when they say that the equivalence to the sentence 'It is not certainly false that James will come' shows that there aren't any possibilities. The former way has the thought that if it is true that there is a possibility that James will come the possibility exists that he will come (if it didn't exist, if there weren't any possibility that he would come, the

sentence would be false). The second way has the motivating thought that if we allow such ways of speaking it won't be long before some idiot comes along and tells us that possibilities are things, that they exist in a way that entails more than the truth of the second sentence. To avoid equivocation and confusion I think that we may have to decide (on occasions) which to say, but that choice, in my view, is an entirely pragmatic one.

Quine once spoke of his taste for desert landscapes and I think that this taste is reflected in his desire to have a powerful, economic, neatly individuated ontology. Whilst I can empathatize with that taste I also appreciate the rich multiplicity of species to be found in the jungle. Moreover it is not just a question of aesthetic taste but a positive mistake to suppose that parts of our conceptual scheme aren't more like the jungle than the desert. To show why I turn to the issue of pluralism.

(4) Ontological Monism vs. Pluralism

In chapter 2 I noted that Quine's claim that 'the unit of empirical significance is the whole of science' seemed to be an unwarranted conclusion from the correct premise that statements cannot be tested in isolation. Epistemologically that objection can be met by adopting a group or island theory but this still leaves the second question one should raise about that statement, viz. why did Quine talk specifically of science ? Prima facie at least many of the cognitive decisions we have to make do not concern scientific beliefs at all. It may be said that Quine is simply using the term 'science' in an extremely broad sense, i.e. that Quine identifies the cognitive with the scientific.

Quine, like everyone else, is entitled to adopt what definitions they like but the question of whether this is a good use of the term 'science' is of more than terminological significance. Consider Quine's statement that,[13] "Epistemology is concerned with the foundations of science." Again one might object that epistemology concerns the cognitive status of claims made in many contexts apart from the scientific context. I can begin to jusify my claim that this objection is of more than terminological significance by asking the question, 'is it not the case that different 'groups' of beliefs may be individuated by the fact that they serve different purposes ?' I think that the answer to that question is 'yes'. Quine, from time to time, correctly observes that science has grown out of commonsense beliefs by 'refining' them. It does not follow from that, however, that every 'commonsense' belief is a crude scientific belief since it may not be a scientific belief at all. At this point the link with ontology begins to become apparent. Consider, for example, tables. It is patently obvious that tables were never positited as a term of a scientific theory, tables are a human artifact invented to serve certain purposes. However, once tables were invented people did come to have cognitive, true or false, beliefs about them - including true or false beliefs about whether a table exists in a given spatio-temporal location. Tables, of course, are only one example of an hetrogeneous list, e.g. fireplaces, paintings, houses etc. It will prove convenient to have a label for this list - call it the 'non-scientific ontology of the world.' In addition to the scientific and the metaphysical lists mentioned in the previous section there is the non-scientific list:

<u>Non-scientific</u>

paintings

tables

novels

:

I take it that everyone would agree that there would be no point in attempting to complete this list, but someone involved in, say, the philosophy of art might find themselves asking the question, 'just what is a painting ?', a form of an ontological question. Now a Quinian or a metaphysician (at least one like Campbell) will see no point in even mentioning this list. In defence of this position they can claim that, paintings, say, will be included in either (or both) the metaphysical or the scientific list. I don't wish to deny that in some sense that is true but since a painting is not just a physical object (plenty of physical objects are not paintings) the question is what makes its characteristics as a physical object more important than its characteristics as a special kind of physical object ? The correct answer, I think, is that absolutely speaking nothing at all. Paintings are irrelevant for science just as electrons are irrelevant for the philosophy of art but I see no difference in their metaphysical status, i.e. paintings exist as surely as electrons or any other favoured item. For reasons I shall give in chapter 6 I believe that it is important to stress that the concept of a painting is not a scientific concept at all. If the concept of a painting were a scientific concept the scientist would be remiss in not including paintings on the scientific inventory. The only reason that I can see why the scientific inventory is taken more seriously is a metaphysical prejudice that the aims of science are more important than the aims of other areas of inquiry. Note that this in no way 'denigrates' the scientific ontology - it merely points out that there are other kinds of things. If this view is correct the consequence is no less than a rejection of Campbell's description of the purpose of ontology as that of providing a 'reasoned inventory of the categories of being.' At least that consequence follows if one doesn't rely on metaphysical predjudice and one admits that there is no point

in attempting to complete the non-scientific list. In fact that last point points up another difference between the explanatory and the discovery approaches: I don't think the importance of the scientific list resides in its length. I don't see the purpose of trying to complete any list.[14]

(5) Revisionary Ontology: A Note on Goodman's Set Ways

"No one shall be able to drive us from the paradise that Cantor created for us." Hilbert. 15.

Goodman describes himself as a nominalist in the (somewhat unorthodox) sense that he refuses to countenance sets. Contrary to his (1947) paper written with Quine it transpires that Goodman is not opposed to sets on the grounds that they are abstract,[16]

> "I do not look upon abstractness as either a necessary or a sufficient test of incomprehensibilty; and indeed the line between what is ordinarily called 'abstract' and what is ordinarily called 'concrete' seems to me vague and capricious."

Nor does Goodman's opposition to sets concern the issue of finitism since he says his nominalism is compatible with non-finitism. Rather he says that the nominalist denies that "...two different entities can be made up of the same entities." He illustrates this claim as follows,[17]

> "..suppose..that a nominalist and a platonist start with the same minimal, atomic elements for their systems; merely for comparative purposes take the number of these atoms as 5. The nominalist admits also all wholes or individual sums comprised of these, and so has a universe of $2^5 - 1$, or 31 entities. He cannot concoct any more; for whatever individuals among the 31 are added together, the result is another individual among those 31. Our platonist...admits no sums of atoms but admits all classes of them. This, not counting the null and unit classes, gives him also 31 entities. But he further admits all classes of classes of atoms; and by this single step he welcomes into his universe $2^{31} - 1$, or over two billion, additional entities. And he has no thought of stopping there. He also admits all classes of classes of classes of atoms, and so on ad infinitum, climbing up through an explosively expanding universe towards a prodigiously teeming Platonic Heaven. He gets all these extra entities out of his original five by a magical process that enables him to make two or more distinct entities from the same entities."

Goodman anticipates the objection that we know from everyday experience that different things are often made out of the same materials by stressing that the different things are made at different times. It is not clear, however, that this is always true. S.Haack points to the case of two recipes which use the same ingredients, in the same proportions, but which yield very different dishes. In this case,[18]

> "It's not that any particular dish of oeufs a la tripe is made of the very same eggs, onions etc. as some particular dish of oeufs durs soubise - not even at different times. Rather the two dish types are made (timelessly) of the very same ingredient types. The difference between the two dishes lies, not in the ingredients, which are just the same, but in the operations performed on the ingredients: the analogy with set theory when {{a}, b} and {a, {b}} are constructed by different operations from the same atoms is quite close."

It is not clear, therefore, that the so-called 'magical process' is not analogous to processes that do occur in everyday life.

Goodman also anticipates the objection that nominalism would hamper the development of mathematics and other sciences by depriving them of methods they have used to achieve some of their most important results. In part his reply is as follows,[19]

> "The nominalist does not presume to restrict the scientist. The scientist may use platonistic class constructions, complex numbers, divination by inspection of entrails, or any other claptrappery that he thinks may help him get the results he wants. But what he produces then becomes raw material for the philosopher, whose task is to make sense of all this: to clarify, simplify, explain, interpret in understandable terms. The practical scientist does the business but the philosopher keeps the books."

This passage calls for several comments. First, this view threatens to involve not only an a-priorism about the results of science but also an a-priorism about the methods of science. How does the philosopher know which methods constitute 'claptrappery' and which do not ? Surely scientific method evolves in parallel with results, i.e. we make judgements about the methods of science based on the results of science. But if the philosopher is entitled to question both the methods and the

results of science simultaneously how can those judgements be justified ? The second thing I want to say is that the way the passage is written it looks as though Goodman supposes that it follows from the fact that the purpose of the philosopher is explanatory it follows that the philospher 'keeps the books'. I think that claim is reasonable if the philosopher keeps the books in a non-revisionary way, e.g. a philosopher of mathematics may try to show that there are no abstract entities without challenging the results of any mathematical theory. But of course that is not what Goodman is doing - he claims that set theory produces unintelligible results. I don't want to argue that philosophers can never be justified in reaching revisionary conclusions (since it is not possible to anticipate what situations may arise in the future), but I do suggest that such conclusions impose a considerable burden of proof on the philosopher. The burden of proof is imposed by the fact that we need to know how the philosopher can be in a better position than the scientist (or whoever) to know what is the case. We need to know that the philosopher has better reason than a-priori pre-judice for criticizing the results of science. It seems to me that Goodman's criticism of set theory simply fails to bear that burden of proof.

Goodman, obviously, understands set theory formally but nevertheless professes not to know what sets are. One approach in the philosophy of mathematics, an approach with which I have some sympathy, would suggest that someone who understands set theory formally ipso facto knows what sets are. The way that Goodman writes about sets suggest that he would like to say something like 'how can things with the weird properties sets are alleged to have possibly exist ?' Such a question, however, hardly makes sense in the context of Goodman's avowed irrealism and, indeed, I see no way of reconciling Goodman's intemperate dismissal of set theory with the rest of Goodman's temperate and reasonable philosophy.

(6) Conclusion

I should like to attempt to draw together the effects of taking the particular combination of views I favour together.[20] Perhaps I may best do this by considering how my view relates to Wittgenstein's conception of philosophy.

To speak of philosophical explanations and to argue that philosophers may have reasonable grounds for asserting or denying that there are things of various kinds would seem to run completely counter to Wittgenstein's view that 'philosophy leaves everything as it is.' The contrast is not as stark as it appears however. I have suggested that legitimate philosophical assertions or denials about what exists should at least normally be compatible with the first-order inquiry. In one sense that is to 'leave everything as it is.' I have not said that philosophy may not be revisionary in the sense that it may not change 'normal' ways of thinking. (Indeed if it could not legitimately do that I think it would be pointless). An instrumentalist philosophy of science might change the way scientists thought about science. It is not clear though that something is not equally true of Wittgenstein's work: a mathematician who thought of mathematics in a Platonistic way would surely view it differently if they became convinced of a Wittgensteian view of mathematics. Nor is it clear how much weight to place on the idea of 'explanation'. There is some structural similarity between the meta-metaphilosophy and Wittgenstein's injunction to look at the uses of language. Moreover, isn't an account of mathematics which emerges from following that injunction at least an embryonic explanation ? It is true, I think, that I have fewer qualms about philosophers introducing whatever technical apparatus that may further the aim of explanation.

Wittgenstein, however, at least employs quasi-technical terms, e.g. 'language-game', 'criteria'.

An interesting suggestion as to how my view differs substantially from Wittgenstein's might go as follows: on my view philosophers may examine the claims made in the context of many areas of inquiry. As a result of that examination philosophers may, for various reasons, decide that it is reasonable to conclude that there are physical objects but that there are not abstract entities or non-physical objects of any kind. (In fact the conclusions I shall reach are not very different from that position). Now that looks very un-Wittgensteian since it is to envisage the success of the substantial metaphysical programme of materialism. I think, however, that there would be a very considerable difference in attitude between myself and those who have attempted to fulfill the materialist programme. I can hint at the difference by saying that whereas they would view that as an exciting metaphysical result I would not. I think that the thought that makes materialism appear as an exciting programme is the thought that if one could filter out any contribution that we make to knowledge of the world what would remain would be the physical processes and entities that constitute the structure of the world. My view, however, to put the point vaguely and intuitively, is that we can't separate our contribution from that of the world. (Compare what I said about paintings above). Thus the 'success' of materialism on my view would be too easy a victory for the traditionally minded materialist. On my view there cannot be a more successful version of materialism than that provided by physics. In short, it is reductionism which gives philosophical materialism its excitment but my views are not reductionist.

Chapter 4

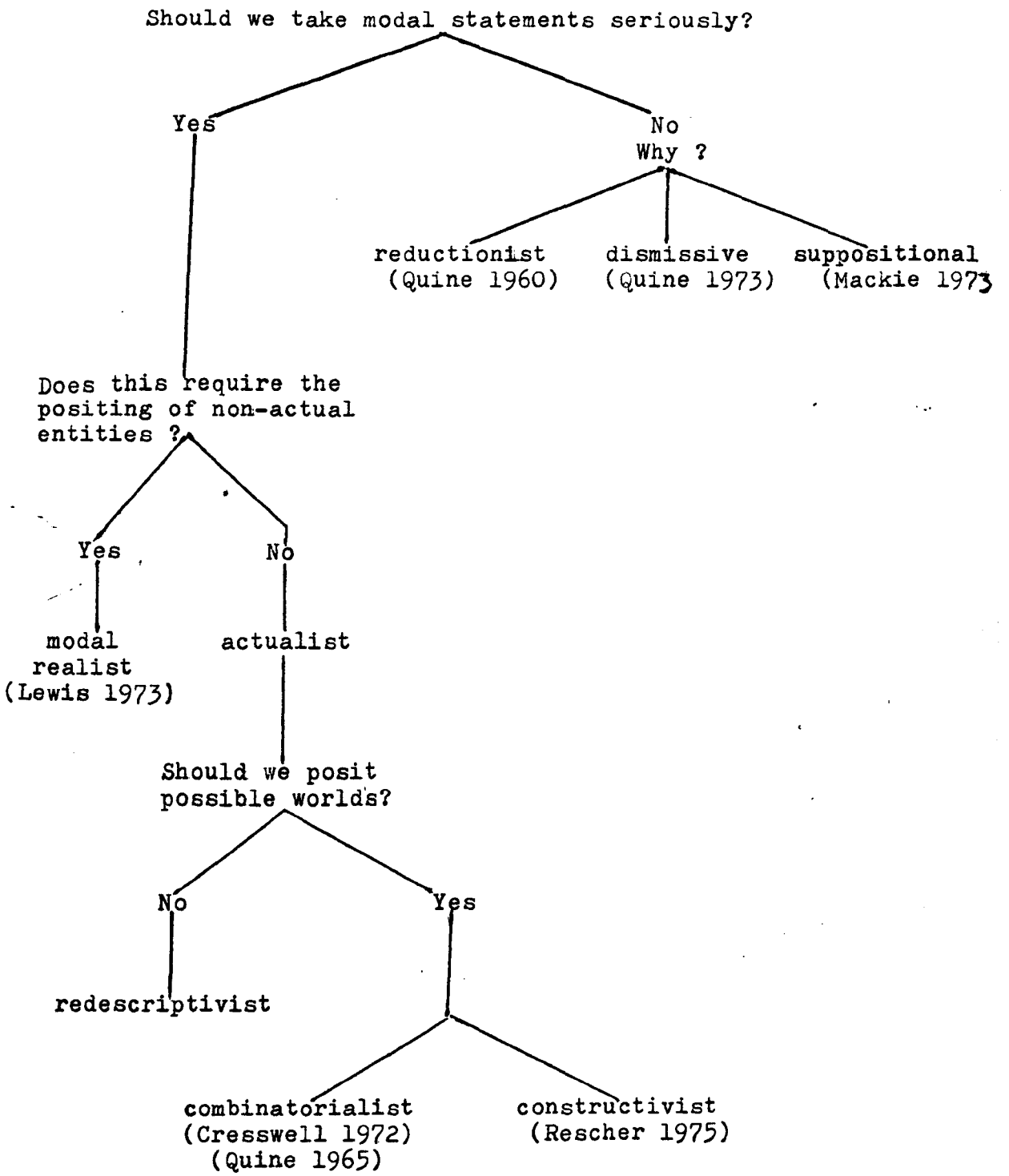
Possible Problems

At various points in previous chapters I have appealed to subjunctive conditionals when explaining my views. In chapter 2 I said that statements asserting the existence of objects ought to be construed as the claim that a set of subjunctive conditionals is true. In this chapter I undertake two tasks: (i) to show that this appeal is consistent with the rest of my views and that in particular one can simultaneously hold the view that modal statements may be true or false without our knowing which, and the view that only actual things exist; (ii) to criticize many of the current approaches to modality. This latter task would be worthwhile even if those views were not (as they seem to be) in conflict with the general views for which I am arguing. Since they are in conflict this task acquires some urgency.

I may best explain the structure of this chapter by reference to figure 5 overleaf. The view which I favour falls under the heading of redescriptionism which will be the subject of part II. Before explicating that view however, part I will outline my reasons for not favouring several other approaches to modality. There are three major questions I raise here: (1) why attempts to avoid taking modal statements seriously (in various senses of 'seriously') are not successful; (2) why some versions of actualism are unsuccessful; (3) why modal realism is unacceptable. As figure 5 shows, I believe that whether actualism is the correct approach to modality is an independent question from whether possible worlds ought to be postulated. I do not claim that the positions indicated in figure 5 exhaust either the possible or even the extant views on modality. I do believe, however, that the views

Figure 5

Some Views of Modality



discussed raise the relevant questions for the purpose of this theis. I will explain terminology as I proceed.

Part I: Some Current Approches to Modality

In part I I examine a number of approaches to modality which have found favour in recent philosophical discussion. First I will examine three distinct ways of refusing to take (some) modal statements seriously (the 'eliminability thesis') and argue that none of these views is satisfactory. Second I will examine two kinds of actualism - the view that only actual things exist. I shall conclude by considering Lewis's argument for modal realism.

(1) The Eliminability Thesis

One general comment which should be made concerning all three versions of the eliminability thesis (the E-thesis) is that they only concern the analysis of dispositional statements. This in itself seems a cause for suspicion. For various reasons (some of which will be mentioned below) dispositional statements seem to provide the simplest case of modality. For this reason, if the views discussed cannot (as I shall argue) provide an adequate analysis of dispositional statements then, a fortiori, they cannot provide an adequate analysis of modality in general.

(a) Reductionism

In Word and Object Quine gave an analysis of dispositions as follows:[1] one defines a relative term M as meaning 'alike in molecular structure' which enables one to paraphrase 'x is soluble' and 'x is fragile' as,

$(\exists y)$(Mxy and y dissolves)

$(\exists y)$(Mxy and y breaks).

Mellor proposed a counter-example to this analysis based on the fact that the analysis always requires that there be some occasion when the dispositional predicate is manifest.[2] The counter-example was that of a nuclear power station - the safety precautions involved in the design of the power station are intended to prevent its disposition to explode from being manifested. In this case there is (or was) no manifestation of the dispositional predicate (no cases of nuclear power stations exploding) for x to be similar to. Mellor concluded that,[3]
"It is absurd to suppose that these precautions have no basis unless they are somewhere and sometimes unsuccessful."

Quine's analysis amounts to the claim that all we need to know in order to be able to apply dispositional predicates truly is that the object in question has a particular microstructure. Quine writes,[4]
"What we have seen dissolve in water.. has a structure suited to dissolving; and when we now speak of some new dry lump of sugar as soluble, we may be considered merely to be saying that it, whether destined for water or not, is similarly structured."
But this claim is false; in saying that the new sugar lump is soluble we are not 'merely' saying that it has a certain structure, but also that that structure would be (partly) responsible for it dissolving if it were immersed in water. The point here is not that the appeal to microstructure is necessarily wrong but that that appeal cannot eliminate modality. It may sometimes be the case that when dispositional statements are truly applied to objects, what makes the dispositional statement true may be the fact that the object in question has a structure such that, in the appropriate circumstances, that structure would be a causal factor in the thing ϕ-ing. It is apparent that this view does not eliminate modality.

I said that it may sometimes be the case that dispositional statements are true in virtue of a certain structure that a given object possesses. Does it not make a mystery of dispositions if, however, one allows that a dispositional statement may be true without it being the case that it is true in virtue of microstructure ? When Quine and others identify microstructure with solubuility they are claiming, I take it, that a given kind of microstructure is identical with the dispositional property. I believe that one may certainly question this claim for reasons closely analogous to those that Davidson has offered in support of the view he calls 'anomalous monism'.[5] Consider the dispositional term 'irritable' as applied to people. It may be the case that whenever the dispositional predicate '...is irritable' is truly applied, some causal story could be told in, say, neurophysiological terms. However, even if some such story can always be told, it does not follow that the neurophysiological states invoked in the explanation form a kind. I shall elaborate upon this view in part II where I shall suggest that the appeal to microstructure becomes wildly implausible when applied to more complex modal idioms.

Mellor's counter-example was, I believe, telling, but Mellor drew from it a stronger conclusion than I would endorse. In his paper 'In Defence of Dispositions' Mellor said that dispositions were 'real' properties and added that nominalists who did not like his terminology could rephrase this as the claim that the intelligible application of dispositional predicates does not require them to be coextensive with non-dispositional predictes. My view is that we cannot know when a dispositional predicate is truly applied without knowing the truth of an intensional description of the object in question. However, when we have, say, a true theory of the solubility of sugar which describes the

microstructure which is, or would be, responsible for it dissolving, the categorical property (the microstructure) is coextensive with the property we call its solubility. Quine's mistake, as I see it, consisted in concluding that because dispositional properties are coextensive with categorical properties, the truth-conditions of the statements describing the objects in question can be given in an extensional language. To clarify: we know the truth of dispositional statements iff we know the truth-conditions of statements employing intensional idioms. Mellor's mistake lies in concluding that because we cannot describe dispositional properties without employing intensional idioms the properties in question must be somehow peculiar.

(b) The Dismissive Attitude

If the reductionist approach to dispositional statements is not successful, does the 'dismissive' approach fare any better ? In The Roots of Reference Quine writes,[6]

> "...if I were trying to devise an ideal language for a finished theory of reality, or any part of it, I would make no place in it for the general dispositional idiom. In developing a theory, on the other hand, the idiom is indispensible. Just as in writing an essay one commonly sketches in ulterior paragraphs before completing the front ones, so in developing a theory one sketches in a few key traits of what is meant ultimately to emerge as a satisfactory explanatory mechanism. And since science is always developing, the idiom is here to stay."

This is really an odd statement; what he gives with the one hand takes with the other. Feeling sympathetic to many aspects of Quine's philosophy I had concluded that there was little room for 'ideal languages' and still less for finished theories of reality. Indeed, having started talking of such he then agrees that the dispositional idiom is here to stay. I find it hard to see anything here but a large piece of question-begging for Quine's general thesis that an extensional language is adequate for the whole of science. Still, this is ad hominen.

In saying that dispositional idioms are dispensible two things could be meant, either (i) that dispositional idioms are not required in scientific theory itself, or, (ii) that a philosophy of science (Quine's naturalized epistemology) need not employ them. The first claim is surely implausible. It is an important part of our theory that the nuclear power station has the disposition to explode. What goes for nuclear power stations must surely go for many of the other things we theorize about. The second claim is equally implausible however and the argument that was rehearsed in the first section reapplies here. What scientists know when they posit various mechanisms cannot be represented within an extensional language. Given that fact, a philosophy of science which attempts to explain how scientific knowledge is possible, whilst at the same time spurning intensional descriptions of what scientists know, is bound to be inadequate.

(c) The Suppositional Account

In his (1973) Mackie offered a 'suppositional' account of dispositional idioms. On this view, to say 'x is soluble' is not to make a true or false statement but, rather, to licence the inference from the premise 'x is soluble and x is placed in water' which one supposes to be the case, to the conclusion 'x dissolves'. Campbell endorses this analysis as a way of upholding Quine's claim that cannonical notation is adequate for the whole of science without adopting his 'draconian programme' of reducing subjunctives. Explicating Mackie Campbell writes,[7]

> "If the liquid is acid, then the litmus paper will turn red is analyzed as:
> Suppose the liquid is acid. Using this as a premise, more or less formal reasoning processes lead us to conclude that the litmus paper will turn red."

What does the 'more or less' mean here ? No formal reasoning will take

us from the premise 'the liquid is acid' to 'the litmus paper turns red' without some supplementary premise - but isn't the supplementary premise the very subjunctive conditional that was supposed to be redundant ? If the claim is that we can know that a subjunctive conditional is true simply by knowing the truth of descriptions expressible in an extensional language (which is what Quine needed) then this claim is false. If this is not the claim being made then it is not clear what the point of the analysis is supposed to be. It may be that Mackie, *contra* Mellor, was concerned to make the point that we may understand dispositional statements without positing 'real properties'. That seems to me true, but it does not follow that we can therefore dispense with intensional descriptions in describing what we know when we know a subjunctive conditional to be true. If Mackie only wanted to defend some version of actualism (see below) all well and good, but I do believe that even a successful version of actualism cannot dispense with modal idioms.

For the reasons outlined above, I do not believe that any of the attempts to eliminate modality are successful. If those attempts cannot succeed with the simplest modal idiom - dispositions - then *a fortiori* they are not likely to succeed for more complex idioms. One reason for wanting to eliminate modal idioms is (an understandable) reluctance to posit non-actual entities. I hope to show that modal idioms may be taken seriously without any such commitment, but before offering my own account I want to discuss two attempts to do this which are not, I think, successful.

(2) Two Kinds of Actualism

Actualism is the view that there neither are nor could be any non-

existent objects, i.e. that the only possible entities are actual entities.

(a) Combinatorialism

Combinatorialism (the C-thesis) is the kind of actualism which says that modal statements, if true, are true in virtue of describing possible rearrangements of actual entities. One way of describing this view is as follows:[8] Suppose that there are metaphysically basic elements out of which our universe is composed. Call them 'atoms'. The actual world consists of these atoms being arranged in a highly complex manner. The actual arrangement of atoms could be taken to be, or represented by, a vastly complex set built up of nothing more than atoms and sets of atoms as members. Now one may construe the phrase 'other possible worlds' as referring to other arrangements of atoms than the actual arrangement. In his paper 'Propositional Objects' Quine suggested that space-time points be taken to be the atoms.[9]

A standard objection to this view is that it rules out what intuitively seem to be bona fide possibilities. It rules out, for instance the possibility that there should have been more atoms (whatever kind of thing they are supposed to be) than there actually are. Claims and counter-claims about what is possible are always possible, but the C-thesis seems to rule out possibilities for the wrong reasons. Goodman sometimes appears to be a combinatorialist so it will be useful to consider one of his examples.

In Fact, Fiction and Forecast Goodman mentions what may be described as a 'possible automobile'.[10] The idea is that there is a chassis on one side of the street and a body on the other. This possible automobile misses being an actual automobile because it lacks a certain relationship

among its parts but, obviously, those parts are actual. Suppose that one generalizes from this example to ask the question, 'how many possible automobiles are there in the world at the time t ?' Imagine that we could discover exactly how many cars there are in world at t. Call this number n. Further suppose that we could also discover how many parts there are in existence. Call this number m. These parts could be assembled in various ways to produce different numbers of possible cars. The combinatorialist will probably want to allow every possible arrangement of parts as constituting a possible car. The number, a function of m, which would result from adding together the numbers of possible cars produced by all of the various arrangements of parts, I denote by f(m).

The C-theorist seems to be committed$_t$ to the view that at time t there are only n + f(m) possible cars. But this just seems wrong - the claim that there could have been another car at t simply does not seem to depend upon how many cars and how many parts there are at t. (This was what I meant when I said that the C-thesis seems to rule out possibilities for the wrong reasons.)[11]

I said that the C-theorist 'seems' committed$_t$ to the claim that there are only n + f(m) possible cars; 'seems' because a possible response at this point would be to say that there are more than m number of parts, for there are not only the actual parts of automobiles but also possible parts which could be made from other kinds of actual things. This response not only increases the number of possible automobiles but seems to introduce what is, arguably, the right kind of indeterminacy into the equation - who can say how many parts could be made from everything which exists ? Nevertheless there is something very odd in taking this line. What, one must ask, has happened to the

actuality of the automobile since, given this response, even the parts are no longer actual ? Indeed one should raise this question about Goodman's possible automobile. If the only possible entities are actual (which is the claim of actualism) then there is not a possible car because the possible car is not actual. According to the C-thesis 'possible arrangements of the actual metaphysical atoms' may not be actual, so I do not see how the possibilities that are allowed, have been explained as only being actual. So, although I do believe that the standard objection to the C-thesis, which says that it is inadequate because it rules out genuine possibilities, does have some force, it is not, I think, the fundamental objection. The fundamental objection is that the C-thesis is not actualist enough: it violates its own assumptions. If the fundamental objection were that it rules out possibilities it would be difficult to argue against Quine's view in his (1965) since it is hard to imagine any meaningful way of calculating the total number of space-time points in existence. What I believe is wrong with the C-thesis is that it has the wrong kind of commitment to the actual, viz. the view ought to be not, that any true modal statement must be about some kind of entity but, more simply, that there is some way of construing it to be about the actual. That, at least, is the view I will attempt to develop in part II.

(b) Constructivism

In his (1975) Rescher has provided an account of modality that may suggestively be called constructivist. Rescher provides a useful summary of this approach in his (1973). He writes,[12]

> "Unrealized possibilities do not exist as such. What exist are minds and their capabilities, and consequently languages and their rules. Unrealized possibilities are generated by minds, and so they can be said to 'exist' only in a secondary and dependent sense, as actual or potential objects of thought. Such possibilities are the products

> of an intellectual construction. The ontological status of the possible is thus fundamentally mind-dependent, the domain of the possible being a mental construct."

The trouble with this approach, I believe, is that it is entirely unilluminating. Rescher quotes Quine where he expresses his doubts about a 'possible man'. Quine writes,[13]

> "Take...the possible fat man in that doorway; and, again,the possible bald man in that doorway. Are they the same possible men, or two possible men ? How do we decide ? How many possible men are there in that doorway ? Are there more possible thin ones than fat ones ? How many of them are alike? Or would their being alike make them one ? Are no two possible things alike ? Is this the same as saying that it is impossible for two things to be alike ? Or, finally, is the concept of identity simply inapplicable to unactualized possibles ? But what sense can be found in talking of entities which cannot meaningfully be said to be identical with themselves and distinct from one another ?"

In response Rescher writes,[14]

> "Quine is seeking for a principle of individuation...for nonexistent, yet possible, items. But - his inclination to the contrary notwithstanding - this problem does not in fact pose any insuperable obstacles. Presumably a nonexistent possible is to be identified by means of a defining description. And on this, the classical approach to the matter, the problems..posed by Quine encounter no decisive theoretical difficulties. How many possible objects are there ? Clearly as many as can be described distinctly - presumably an infinite number. When are two possible objects identical ? When their defining descriptions are 'logically identical', that is, equivalent. The doctrine of possible objects entails no major logical anomalies. With 'nonexistents' everything save existence alone (and its implications) remains precisely the same as it does with objects that 'really' exist..."

Rescher equates possibility with 'conceivability' when he is dealing with what he calls 'hard-core, totally unactualized possibility'. It seems to me that Rescher has to impose some restrictions on what is conceivable. It simply cannot be said that there is an unactualized possible for every description since one can supply descriptions of impossibles, e.g. the round-square. The trouble now is that it is hard to see what restrictions Rescher can impose that will be illuminating. If we are told that the description must be of a logical possibility the analysis only amounts to saying that that which is conceivable is logically possible. What this shows, I think, is that although actualism

maybe a necessary condition for a coherent and informative account of modality, it is not a sufficient condition. A coherent and informative account must, I believe, do two things, (i) avoid commitment to non-actual objects; (ii) indicate how modal statements are related to our theoretical beliefs about the world. Even if Rescher succeeds with respect to point (i), he says nothing helpful with respect to point (ii). In part II I hope to show how one can do better.

(3) Modal Realism

In Counterfactuals Lewis offers the following argument,[15]

> "I believe that there are possible worlds other than the one we happen to inhabit. If an argument is wanted, it is this. It is uncontroversially true that things might be otherwise than they are. I believe, and so do you, that things could have been different in countless ways. But what does this mean ? Ordinary language permits the paraphrase: there are many ways things could have been besides the way they actually are. On the face of it, this sentence is an existential quantification. It says that there exist many entities of a certain description, to wit 'ways things could have been'. I believe that things could have been different in countless ways; I believe permissible paraphrases of what I believe; taking the paraphrase at face value, I therefore believe in the existence of entities that might be called 'ways things could have been.' I prefer to call them possible worlds."

This is a strange argument in many respects. First, it seems to me that it is not uncontroversially true that things might be otherwise than they are. Far from being uncontroversially true this seems to me false; what might be uncontroversially true is that things might have been otherwise than they are. I accept the next claim that 'things could have been different in countless ways.' Lewis then paraphrases this as, 'There are many ways things might have been besides the way they actually are.' A good question to ask here is what work the word 'actually' is doing. Although I have had to use the word 'actual' frequently one must bear in mind its pernicious tendency to presuppose that there are (by contrast) non-actual ways that things are.

Having noted the dangers of the word 'actual', the next major thing to note about the argument is that it is symmetrical between what might be called the inflationary and the deflationary interpretations. These interpretations differ according to what one takes as premise and what as conclusion. Lewis starts from the premise that things might be otherwise than they are. This is then paraphrased as 'there are many ways things could have been otherwise than they are', from which the conclusion is drawn that there are possible worlds. However, if one starts from the premise that 'there are many ways things could have been different from what they are', since this permits the paraphrase, 'things could have been different in countless ways', one could claim that this shows that we do need to posit possible worlds. Those who keep Occam's razor to hand might wonder what justification there is for the inflationary rather than the deflationary reading.

The issue between actualism and modal realism may be expressed by the question, is it better to have modal predications of actual objects, or non-modal predictions of modal objects ? It is tempting to give the short answer to this question that if we do the latter we will not know what we are talking about. I fear that this would beg the question however. I do believe, though, that there is an argument in favour of the former course.

Modal intuitions are notoriously variable; mine, as it happens, are fairly weak. Still, for everyone, they shade off: we are sure that some modal statements are true, whilst there are other modal statements such that we have little idea of what they mean. Take a mind-boggling counterfactual such as, 'if Julias Caesar were prime minister today he would have an incomes policy'. I am inclined to think that the supposition that the historical figure of Julias Caesar could be alive

today does not make much sense. Take a less controversial counterfactual that does not involve problems of personal identity: 'if Regan had been in power at the time the American hostages were taken, he would have got them out quickly.' I do not know (and I don't think that anyone else does) whether that counterfactual is true. It might be true, or there might have been a war, or.... Why are we unsure in cases like these ? Actualism has an answer to this question: since modal statements are about the actual world, how sure or unsure we are about them is bound to vary with the strength of our theories about the world. Our theories about psychological, sociological and political matters, say, are notoriously weak, therefore modal statements that involve these kinds of theory are problematic.

How does modal realism fare when faced with the same question ? Here I think the glibness of the answer reveals the bankruptcy of its explanatory power. For example, we can answer the counterfactual about Julias Caesar as follows: it is true iff there is a possible world in which Julias Caesar is prime minister today, he has an incomes policy, and that possible world is as similar to this one as is compatible with those differences. I contend that this gets us nowhere. Nor is it obvious what a modal realist like Lewis can say about this problem. It is true that within his system Lewis employs the metaphor of 'distance': those possible worlds about which we know little are those that are 'remote' from our own. But what do 'distance' and 'remote' mean here ? It is true that from some of the things Lewis says one might almost expect a <u>physical</u> measure; the greater the distance would correspond to the greater number of light-years clocked up when travelling from one possible world to another. Few, I trust, will want to swallow that. The serious point here is that talk of distance seems more to repeat the question than to answer it. By outlining the view

which I favour I hope to underline this criticism.

Part II: Redescriptivism

"...despite all the scare stories, possibility is all right. But contrary to the glowing prospectuses of unreal estate salesmen, possibility is very much rooted in this world." 16

Redescriptivism is a species of actualism; according to this view we can make sense of modal statements without being commited to non-actual objects. Indeed, according to redescriptivism there are no special ontological problems concerned with modal statements.[17] In this respect redescriptivism contrasts not only with modal realism but also with those other forms of actualism discussed above which attempt to explain away our commitment to non-actual objects; on this view there is no such commitment and, therefore, nothing to be explained away. That is to say, there is no need to find respectable surrogates for non-actual objects (combinatorialism's 'atoms'), nor to show that references to the non-actual are respectable because mind-dependent (Rescher's constructivism). Recent philosophical discussion of modality has, I believe, misconceived the problem as ontological, whereas I take the real difficulties to be epistemic. Acceptance of redescriptivism allows one to understand how our modal intuitions shade off in proportion to the amount of confidence we have in our theories of the world. I hope to provide justification for the claim made by Hacking (quoted above).

I will proceed as follows. First, I will discuss various kinds of of possibility and Hacking's proposals for distinguishing them. I will then employ a (modified) version of this proposal in discussing three kinds of modal statements viewed from the redescriptivist perspective.

Finally, having shown that modality holds no ontological terrors, I will employ subjunctive conditionals in outlining how the concept of relative truth-conditions defined in chapter 2 may be further explicated.

(1) Kinds of Possibility

In his (1975) Hacking has proposed a way of distinguishing distinct kinds of possibility by syntactic means. He distinguishes two forms of sentence as follows,[18]

M : It is possible that p.

M* : It is possible for A to x.

Substitutions for 'p' are sentences in the indicative mood only. Substitutions for 'A' may include the names of agents, inanimate objects, mass nouns, and general terms. These substitutions may be illustrated in turn by the follwing sentences:

'It is possible for Rachel to hold her breathe for a whole minute.'

'It is possible for the kettle to boil dry in five minutes.'

'It is possible for sand to wear down the mountain.'

'It is possible for any python to swallow some monkey.'

Substitutions for 'x' include not only descriptions of actions but also states, for example,

'It is possible for him to be a woodsman.'

Hacking points out that M and M* are not identical since M* does not entail M,[19] "It may be possible for the judge to give the woman a suspended sentence, but it is not possible that he will; he is notoriously mean and will certainly send her to jail." Moreover, 'possible' in M may be replaced by such adjectives as, 'probable', 'certain', 'true', 'doubtful', 'believable', 'alleged', and 'plausible', none of which

fit M*. Adjectives which may replace 'possible' in M* include 'permissible', 'obligatory', 'essential', 'unheard of'. There are others (including 'possible' itself) which can not take the place of the blank in 'It is....of A to x', but others which may take that place and the place of 'possible' in the M construction, for example, 'unwise', 'delightful', 'insulting'.

The importance of the distinction between M and M* sentences may be illustrated by an example Hacking gave in his earlier (1967) paper.[20] The sentences,

(1) It is possible that I shall go.

(2) It is possible for me to go.

are apparently about the same thing - namely, my departure - but (2) may be true when (1) is false. As this example and the list of adjectives suggest, Hacking contends that when not modified by 'logically' M represents epistemic possibility,[21] "To say that it is possible that so-and-so, is to say that so-and-so is consistent with all we can know or readily find out to the contrary." Technical, human, economic, metaphysical possibility and so forth are represented by the M* construction.

I have only one real quarrel with Hacking's grammatical device. As noted, substitutions for 'p' must be in the indicative mood only. This entails that possibilities expressed by subjunctives cannot, by fiat, be epistemic. It seems to me however that many subjunctives are naturally construed as expressing epistemic possibilities. (I shall provide illustration of this claim below). In employing Hacking's device I will therefore drop the restriction that substitutions for 'p' must be sentences in the indicative mood.

There is one further modification of Hacking's device that I wish to make. This is to extend the M and M* forms in order to be able to encompass modal statements in the past tense. This I shall do by adopting the following convention:

M_p : It was possible that p.

M^*_p : It was possible for A to x.

These modifications will be employed in the discussion of 'might' and counterfactual sentences below. First, though, I will say something about dispositions.

(2) Three Kinds of Modal Statements

(a) Dispositions

Dispositional idioms seem to provide the simplest case of modality. Contrast the term 'dissolves' with the term 'soluble'. 'Dissolves' is true of things that actually do dissolve when immersed in water, 'soluble' to things that would dissolve if immersed in water. Although it need not be doubted that things are soluble in virtue of some physical property, even in this simplest case, there are difficulties in straightforwardly identifying the property of solubility with a particular microstructure. It may be, for instance, that what we take to be one natural kind - sugar say - may consist of two types, so that sugar of type a is soluble in virtue of microstructure x, whilst sugar of type b is soluble in virtue of microstructure y. (In this case whether we want to say that there is one natural kind with two different microstructures, or that there are really two natural kinds, surely just depends upon whether we wish to identify natural kinds by their macro or micro properties). It also seems possible that if the world had been different then sugar might be soluble in virtue of a different microstructure

from the one which, in the world, does make it soluble. In that eventuality it might be said that 'solubility is still solubility' and this claim prevents straightforward identification of solubility with a particular kind of microstructure.

The temptation with analyses of disposition terms has been to claim that all disposition terms true of objects are true in virtue of a particular kind of microstructure m. Even ignoring the difficulties raised above this seems too strong a claim. Consider the term 'irritable' and the set of people S of whom this term is true. Now I see no reason to deny (as Mellor does) that for each of the members of S, there is a categorical property P with which the dispositional property is coextensive. It does not follow from this, however, that it must be the same kind of property for each of the members of S. There is a strong analogy here with the view Davidson calls anomalous monism.[22] This is the view that although every mental event is identical with some physical state, the predicates which pick out kinds of mental events need not be coextensive with predicates which pick out kinds of physical events. This view makes compatible two plausible claims that appear inconsistent: (i) that every mental event is identical with some physical state; (ii) that even given complete knowledge of all the initial conditions (described in a physical vocabulary) for all physical theories T, and knowledge of all the covering laws governing all of the events described by the set of theories, it would not follow that one could predict any mental event so described.

My view allows for what seems a reasonable position - that it ought to be an empirical question whether a dispositional predicate is coextensive with a kind of microstructure. This view of dispositional statements becomes even more compelling, it seems to me, when applied

to more complex modal idioms. I turn to the idiom of 'might'.

(b) 'Might'

Consider the statement,

(3) Derby County Football Club might win the F.A. cup in 1984.

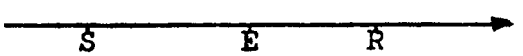
Employing the M and M* constructions nicely disambiguates different grounds that one might have for believing (3) true. Compare,

(3a) M: It is possible that Derby County will win the F.A. cup in 1984.

(3b) M*: It is possible for Derby County to win the F.A. cup in 1984.

(3b) asserts something weaker than (3a). (3b) is very likely to be true since Derby County will be eligible to win the F.A. cup; they will be one of the teams in the competition. Of course this fact is not certain - the club may become bankrupt before 1984 and cease to exist. However, even if (3b) is true (3a) may be false - it is not possible that they will win the F.A. cup since they are not a good enough football team.

It may be useful here to adopt a device employed by Reichenbach when discussing tensed sentences. He represents, for instance, the future perfect tense of 'I shall have seen John' as,[23]



The arrow represents the direction of time; 'E' represents the 'point of event'; 'R' represents the 'point of reference'; and 'S' represents the 'point of speech.'

I suggest that one may represent (3a) as follows,

The F.A. cup competion consists of a series of events (football matches etc.); placing 'R' at the time of the last event represents the

fact that one is referring to the outcome of the competition. The events in question are not yet actual but this poses no problem for actualism; (3a) is true or false at S in virtue of the relative skills of the sides in the competition. That this is so is shown by the fact that the probability of (3a) being true could change over the period of time before the competition begins - the team might acquire more skilful players for example. The difficulties in determining the truth-value of sentences such as (3a) have to do with knowing which facts in the world will determine the outcome. If (3b) is true then it is true in virtue of such facts as the eligibility of the team for the competition.

It is worth considering (3a) more closely. Although there are, as noted, difficulties in identifying microstructure as that in virtue of which dispositional statements are true, 'might' statements are more complex in that the facts which make them true rarely form a kind. Compare (3a) with,

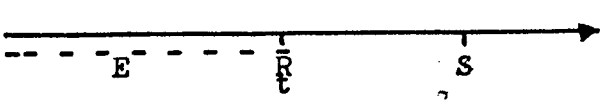
(3a') It is possible for Liverpool football club to win the F.A. cup in 1984.

Assessment of the facts upon which one would assign different probabilities to (3a) and (3a') might require reference to different kinds of facts in the two cases. For example, in the case of (3a) one might consider the motivation and psychological effects caused by being considered unlikely winners of the competition - facts which would not enter into the assignment of probability to (3a'). Following Morton and Mondadori one may speak of modal properties.[24] They suggest that a predicate such as '...might win the F.A. cup in 1984' expresses a modal property and that the same property may be possessed by the teams in question in virtue of different facts about the world. When one describes the causal grounds of, for example, Derby County possibly winning the F.A.

cup, one should not say 'that in virtue of which the F.A. cup might be won' but 'that by virtue of which Derby County might win the F.A. cup.'

In the case of epistemic possibilities it is sometimes possible to give substantive reasons for believing or disbelieving a modal statement. 'Substantive' indicates that our theories about the world may be involved. This seems to me just the sort of connection between the subject-matter of modal statements and our 'theories' that can partially explain why our modal intuitions may be stronger or weaker. It is also just the sort of connection that not only modal realism, but possible world semantics in general, seems to leave aside. Note, however, that I do not claim that for every modal statement it is always possible to make some appeal which gives epistemic justification. (See the discussion below of Dummett's example).

In the above example it is not difficult to find facts about the world which lend support (or lead one to doubt) the claim that Derby County might win the F.A. cup. What should one say though about those cases where the apparent subject of the modal statement does not exist ? It is time to reconsider the case of the possible automobile. What are we to say of the claim that at time t there could have been more than n + f(m) cars ? If actualism is true then this cannot be construed as a claim about a particular car or, indeed, about any car. This does not entail however that the claim is not about the actual world. The claim may be construed as a claim about the causal history of the world up to the time t. One could represent, 'It was possible that there could have been more than n + f(m) cars at t' thus:



Here the 'event' is an (indefinite) part of the history of the world. One is saying of the history of the world up to the time t that it could have been such as to produce a different outcome at the time t. More cars might have been produced, for example, if a factory had gone on overtime before t. Similarly, to say that there might have been another coffee cup on a particular table, can be taken to be the claim that the causal history of the world could have been such to have produced this outcome. It is true, I think, that this talk of the 'causal history of the world' makes it clear how epistemically complex modal idioms are; but the other approaches to modality not only do not fare better, they seem to give more intractable problems, for example, the problem of identifying an object from one possible world to another. Certainly no epistemic advantages are likely to be forthcoming from the view that modal statements are not about the actual ! I shall say more about the contrast between redescriptivism and other approaches to modality below, but first I will say something about counterfactuals.

(c) Counterfactuals

Consider counterfactuals of the form 'if e had occured then f would have occured' where 'e' and 'f' are descriptions of events. Using the M_p construction, consider,

(4) It was possible that if Ian Botham had played for Middlesex then Middlesex would have won the County Championship in 1983.

One could represent this thus:

There are two points worth noting about this example. First, it seems to me a good example of a substitution in the M construction which is naturally seen as epistemic. Not only can 'probable', 'likely' etc.

replace 'possible' in (4), this seems to be the natural interpretation of such a counterfactual. The truth of (4) requires that if Ian Botham had played then it would have been more likely that Middlesex would have won the county championship. Neither the county championship matches (now), nor Ian Botham's possibly having played in those matches are actual. The former was actual, the latter was not. Nevertheless, at the time of the event, there could have been actual facts which would later justify the assertion of (4). Such a fact might have been Ian Botham's superior cricketing skills relative to his replacement.

I said above that it may be the case that for some statements there are not any facts at the time of utterance which would justify asserting it. It is worth reconsidering here Dummett's example discussed in chapter 2, namely,

(5) A city will never be built on this spot.

This statement is analogous to some counterfactuals in that at the time of utterance there may be no actual facts which would justify one either in asserting or denying its truth. This can lead one, if one accepts the realist conviction that there must be something in virtue of which a sentence is true, to the conclusion that such statements cannot be true or false. I believe that this conclusion is unnecessary and unhelpful. (That it is unhelpful will be one of the burdens of the next chapter to show). It seems to me that what one ought to say about (5) is that it is true or false in virtue of a fact about the world, albeit one that can only be described by employing modal idioms. Thus, (5) is false if there will be a city built on that spot or true if a city will not be built on that spot. To repeat: (5) is either true or false according to whether (5) or its negation describes the actual world. Let me put this by saying that either (5) or its

negation describes a modal fact about the world. Now one can imagine a realist objection to this along the lines that there is something objectionable about modal facts since, on the view just stated, one may describe a modal fact without there being anything which exists that makes it a fact. But this misses the point: if it is true that a city will never be built on that spot then it is true in virtue of a state of affairs that will always exist in the world. Conversely, suppose that (5) is false, then the city is not actual (now) but it is something which will be actual. So, one can either say that the negation of (5) is true now in virtue of describing a modal fact about the world, or that it is true in virtue of describing an entity which will be actual. Two points about this discussion seem to me worth emphasizing. (i) That the fact that in the case of (5) there may be no facts which justify asserting it or its negation only signals an epistemic and not an ontological distinction. (ii) The discussion of (5) makes it clear that redescriptivism does not eliminate the need for modal idioms. I will now consider the objection that this view is objectionably circular.

(3) Is Redescriptivism Objectionably Circular ?

Ought one to be able to provide an analysis of modal idioms in non-modal terms ? Morton and Mondadori, whose analysis of modality is very similar to that endorsed here, claim, rightly I believe, that this demand is unreasonable. If one adopts this analysis one finds that one can't describe in non-modal terms the properties which account for different objects satisfying the same modal predicate. On the other hand, a more orthodox modal realist account cannot give any clear sense to the technical terms of the theory, e.g. 'possible world', 'accessible from', except by explaining them in terms of possibility and necessity. Morton and Mondadori analyse the source of the demand for a non-modal

paraphrase of modal statements as follows,[25]

> "The feeling that paraphrase is necessary may come from a subtle confusion. It is natural to suppose that real facts about the world are given by the physical data about the location, motions, and so on of the objects in it. These can be described in non-modal terms; one might therefore suppose that anything that cannot be so described is somehow ungrounded in the facts. But we have seen the mistake in this. Each particular modal prediction - for example, each '<u>a</u> might ϕ' - is indeed grounded in non-modal fact, but the grounding is tied to a particular <u>a</u> and ϕ. We may not be able to find a specification general enough to apply when <u>a</u> is different, let alone when ϕ is different. To say in one breath what properties make an operator like 'might' apply one would have to do both."

As noted above, possible world semantics do <u>not</u> provide a non-modal paraphrase of modal idioms; only the views discussed in part I, section 1, can lay any claim to doing that, but for the reasons discussed such views are not satisfactory.

<u>(4) Redescriptivism and Truth-Conditions</u>

When discussing dispositional statements I did not make explicit appeal to subjunctive conditionals although I might well have done so. To say that some sugar lump which is never destined for water that it is soluble, is to say that if it <u>were</u> to be placed in water it <u>would</u> dissolve. I hope that I may have said enough to show that the fact the truth-conditions cannot be reduced does not entail that either that such statements are not independently true or false, nor that one needs to posit peculiar modal objects. If my discussion of modal facts is convincing, then one may employ modal idioms with a clear conscience. Not only do I believe that the position I call redescriptivism is consistent with a relativist approach to truth and ontology, I am now in a position to employ redescriptivism to further explicate the relativist conception of truth-conditions defined in chapter 2.

Consider the claim,

(6) Mountains existed before human language was created.

I think that good sense can be made of this claim but not in the way absolutists nor, for that matter, commonsense might be said to justify. Let me explain my way first. Consider the sentence,

(7) Mountains exist.

Since sentences are a human creation (7) is not timelessly true. How, then, can one make sense of (6) ? My answer is that we can understand (6) by understanding an appropriate set of subjunctive conditionals. Call the time epoch during which mountains existed, and human language did not, T. (6), I believe, makes good sense if one understands it as the claim that,

(7') 'Mountains exist' would have been a true sentence during T if there had been an English-speaking language community which uttered or could have uttered it. The counterfactual reference to the language community is necessary since an accidental inscription resembling (7) would not have been a sentence.

Why go to such lengths to explain our understanding of such a simple English sentence ? My reason for taking this approach is that only by doing so can we give a coherent account of truth and ontology. Undoubtedly absolutists (and probably many speakers of English) interpret claims about the independent existence of things in a stronger manner. That is, absolutists and commonsense would have one believe that there is something <u>more</u> to metaphysical independence claims than the truth of subjunctive conditionals. I think however that the supposition that there is something more creates insuperable philosophical problems which are quite unnecessary. Given, as I hope to have shown, that modal statements may be true or false independently of our knowing which, I think that this allows a relativistic approach to truth and ontology

If one does not know the colour of the table one cannot specify exactly what visual experiences one would have etc. Suppose, however, that someone correctly points out that according to one part of the language community one ought to be able to have such-and-such an experience, but that according to another part of the language community, that experience is not required. Is there <u>really</u> a table ? That, I should say, is the wrong question. If the situation is as described it shows nothing wrong with my interpretation of 'independent existence' but only points to the open-texture of the predicate '...is a table.'

I should emphasize that there is no reductionist phenomenalism in prospect here - I see no reason for saying that the experiences of which I speak are experiences of sense-data. I believe that they would be what they purport to be - experiences of the table. The point at issue is not <u>phenomenalism</u> but <u>pragmatism</u>; not whether every justified claim about the independent existence of things can be translated into phenomenal terms, but whether we can understand independent existence claims in some sense that is not relative to human cognition.

I have not provided a full analysis of any modal idioms but I hope to have indicated the lines along which analysis should proceed, and that this approach is consistent both with the claim that modal statements may be true or false without our knowing which, and with the claim that only actual objects exist. To end on a placatory note let me say that I do not necessarily believe that possible world semantics have served no useful service. I am, though, inclined to think that they have done us a disservice, not even primarily because they have bloated our ontology, but because they have directed attention and analysis of modal statements away from where it belongs - in the real world.

which avoids idealism.

I can explain my views further by discussing a different example. Consider the claim,

(8) A table exists in that room.

Only idealists or verificationists will want to deny that if (8) is true then it is true although no one may be in a position to verify it, and that the table is an enduring spatio-temporal object whose continued existence does not depend upon human presence. Now this last claim . . cannot be verified although there seems no good reason to doubt it. In the light of these beliefs we say that the 'table exists independently of us.' I believe that it does but all I mean by saying that is that some set of subjunctive conditionals is true. These conditionals are difficult to specify without describing a particular situation in detail. Still, the drift is clear enough. To say that the table exists in that room is to hold true some set of conditionals as:[26]

(9) If I were in that room under such-and-such conditions, I would be able to have such-and-such experiences, for example,

- visual: from certain positions, under such-and-such lighting conditions I would be able to see such-and-such colours, shapes etc.
- tactile: I would be able to experience resistence when applying pressure to particular areas of the room.
- acoustic: I would hear certain sounds if I were to hit out in particular directions.

Now it seems fairly clear that this list cannot be completed - there are an indefinite number of experiences which could be imagined as consequences of my interaction with the table. There are also difficulties about allowing that a broken table may still be a table, e.g. how many pieces may a 'table' be broken into and still be said to be a table ?

Chapter 5

Realism, Anti-Realism & Truth-Conditions

I shall argue that once one understands truth-conditions properly, one realizes that truth-conditional theories of meaning have fewer philosophical consequences than either their critics or their champions commonly suppose. Part I will be concerned to show that realism does not have the difficulties that Dummett, in a number of places, has alleged that it does.[1] Section 1 states Dummett's distinction between realist and anti-realist theories and identifies what I take to be the central misconception underlying Dummett's arguments for anti-realism. Section 2 applies this diagnosis to Dummett's conclusions with respect to the reality of the past. Finally, section 3 delivers the coup de grace by showing that so far from being, as Dummett suposes, necessary for language-learning, anti-realism requires assumptions that would show it to be impossible. Part II turns to the task of showing that neither Tarski's theory of truth, nor Davidson's truth-conditional semantics which employs Tarski's theory, vindicate an absolute metaphysics The central point is that acceptance of verification transcendence does not require, nor vindicate, an absolute conception of truth-conditions.

Part I: Dummett's Anti-Realism

(1) The Argument

My first task in this section is to outline Dummett's presentation of the realist/anti-realist debate. Dummett does not suppose that everyone will want to be realist with respect to every kind of statement,

or anti-realist with respect to every class of statement. Someone might want to be anti-realist about mathematical statements, say, but realist with respect to statements about the past. But what makes someone a realist or anti-realist with respect to some kind of statement ? A key passage in Dummett's (1963) reads as follows,[2]

> "The realist and the anti-realist may agree that it is an objective matter, whether, in the case of any given statement of the class, the criteria we use for judging such a statement to be true are satisfied: the difference between them lies in the fact that, for the anti-realist, the truth of the statement can only consist in the satisfaction of these criteria, whereas for the realist, the statement can be true even though we have no means of recognizing it as true."

The burden of my argument will be that that passage lumps together positions which ought to be distinguished and, moreover, that what cements the positions together is a mistaken view of modality. First, I need to unpack some of the connections made in the above passage.

Anti-realism with respect to a given kind of statement really consists for Dummett in two things: (i) that the truth of a statement can only consist in the satisfaction of the criteria we use in judging it true; (ii) that the satisfaction of criteria is to be identified with knowing that the criteria are satisfied. From Dummett's point of view (i) and (ii) are not independent, but from my point of view they are independent. The difference between Dummett and myself really resides in a difference about modality, as I hope to show.

Dummett says that the realist and the anti-realist both agree that it is an objective matter whether or not the criteria we use for judging a statement to be true <u>are</u> satisfied. My difference from Dummett's anti-realism is that I would say that the truth of a statement consists in whether or not the criteria we use in judging a statement to be true <u>are</u> satisified, <u>will be</u> satisfied, or <u>would have been</u> satisfied.

My position allows that a statement may be true or false although we have no way of knowing which it is but this does not make me a straightforward realist in Dummett's terms. To show this, and to clarify that already said, I turn to an example Dummett discusses.

In his (1959) Dummett envisaged a dispute over the statement, 'Either Jones was brave or Jones was not brave', where Jones is imagined to be dead and never to have encountered danger during his life. (Call the disjunction about Jones 'P'). Now the fully-fledged realist as Dummett presents him wants to maintain both that P is true and that for any statement there must be something in virtue of which it is true. As Dummett presents matters this fully-fledged realist is committed to the unattractive view that the truth or falsity of one of the disjuncts of P must have consisted in the presence or abscence of some inner quality or mechanism of Jones that never became manifest during Jones' life. Are there, then, any reasonable grounds for saying that P is true without adopting Dummett's fully-fledged realism ? Well, could we not say that 'Jones was brave' is true if Jones would have acted bravely if he had encountered danger ? Dummett has claimed that we cannot say that because the truth of a counterfactual must depend upon the truth of a categorical. He writes,[3] "...a counterfactual could not be simply true: if it is true, it must be true in virtue of the truth of some categorical statement." The problem for a statement such as P, on Dummett's view, is that there is no plausible candidate for an appropriate categorical statement. Unfortunately, and surprisingly perhaps, Dummett has never, to my knowledge, provided an analysis of modality to support his claim although he speaks of it as being 'intuitively plausible.' It is hard to exaggerate the importance of this difference of opinion concerning modality - directly or indirectly all

of Dummett's presentations of realism and anti-realism seem to depend upon his modal principle. It occurs indirectly, for example, when Dummett says that we all accept that for a statement to be true there must be something in virtue of which it is true. On Dummett's view that means that there must be something which can be described in non-modal terms and I do not accept that view. According to the view for which I argued in chapter 4, Dummett's principle is false - it is not the case that every true modal statement can be 'reduced' to, or expressed by, some true non-modal statement. Ironically, then, I believe that Dummett's inclination toward anti-realism stems from a misguided and overly strong realist principle. I believe that one obtains an attractive realism by rejecting the view of modality Dummett accepts.

(2) Anti-Realism: The Reality of the Past

In his (1969) Dummett says,[4]

> "Statements about the past form a class the application to which of an argument of the anti-realist type seems to be called for. That it has not often been so applied is doubtless due to certain obvious difficulties from applying it: namely, that an anti-realist interpretation of past-tense statements appears incompatible with acknowledging the existence of a systematic link between the truth-values of differently tensed statements uttered at different times. This difficulty is central to the whole issue. The realist has, after all, to meet the anti-realist's challenge to explain how we come by a notion of truth, as applied to statements about the past, considered as applying to such statements independently of our means of recognizing these statements as true. His answer is that this conception is attained precisely via. our coming to grasp the existence of the truth-value link. If I now (2.45 pm. 12 February 1969) say, 'I am in my College room', I make a present-tense statement which is, as I say it, true: let us call this statement A. Suppose now that exactly one year later someone makes the statement (call it B) 'A year ago Dummett was in his College room'. Then it is a consequence of the truth-value link that, as exemplified in such a case, we derive a grasp of what it is for a statement in the past tense, whenever made, for example one made now, to be true."

Dummett believes that the anti-realist cannot afford to give up the truth-value link, but must show that his view is consistent with it.

Dummett says,[5]

> "...the anti-realist will be unable to avoid inconsistency in recognizing the existence of the truth-value link if he formulates his contention as being that a past-tense statement, made at any given time, is true at that time only if there is at that time a situation justifying that statement."

He describes what the anti-realist ought to say as follows,[6]

> "...he must state his general thesis by saying that a statement in the past-tense is (or was, or will be) true just in case there are now, is, or will subsequently be a situation whose existence we can now acknowledge as justifying the ascription to that statement of the value true. Thus a statement in the past tense, made a year hence, will be true just in case either there now is a situation which we can recognise as obtaining and which we now regard as justifying the statement that the past tense statement will be true when uttered a year hence; or else <u>there will be, at some future time, a situation which we can then recognise as obtaining, and whose occurence at that future time we now regard as entailing the correctness of the statement that the past tense statement will be true when uttered a year hence</u>...The thesis thus relates the truth or falsity of past-tense statements, whenever made, not to the evidence available for them at the time of utterence, but to the evidence that is now, or may latter become, available for ascribing to those statements the property of being true when they are uttered."

(My emphasis). Given this response to the claim that the anti-realist violates the truth-value link I cannot see that the anti-realist has any good reason for regarding the past and future as <u>asymmetric</u>. Consider that part of the quotation which is emphasized. In this case the anti-realist accepts that a statement may be true now even though we do not (now) have epistemological access to the situation which will make it true. The statement is said to be true (now) if there <u>will</u> be a situation which we could sometime recognize as justifying it. But if the anti-realist is prepared to accept that, why should he not accept that a past tensed sentence is true now if there <u>was</u> a situation which would have justified someone in asserting it, and to which we do not have access ? It cannot be because he claims that any sentence can only be true if we have evidence for it at the time of utterance since this does not obtain in the case of the future tensed sentence. It is true

that for future tensed sentences we can perhaps wait and see whether or not a particular sentence will turn out to be true or false but I don't believe that this provides a good reson for believing that the meaning of statements about the past are asymmetric to statements about the future. As far as I can see the anti-realist either ought to give up the truth-value link altogether (which, Dummett says, is a[7] ".. fundamental feature of our understanding of tensed statements.."), or admit that his position is ill-motivated.

(3) The Acquisition Argument

A major problem for realism, as Dummett sees it, is how it can[8] "...account for our acquisition of that grasp of conditions for a transcendent truth-value which [the realist] ascribes to us, and to make plausible that ascription." Why should the realist have any difficulty in providing such an account ? Dummett writes,[9]

> "The general form of the argument employed by the anti-realist is a very strong one. He maintains that the process by which we come to grasp the sense of statements of the disputed class, and the use which is subsequently made of these statements, are such that we could not derive from it any notion of what it would be for such a statement to be true independently of the sort of thing we have learned to recognize as establishing the truth of such statements. What we learn to do is to accept the truth of certain statements... which we have been trained to recognize, as conclusively justifying the assertion of a given statement of the disputed class, and the truth of certain other statements, or the occurence of certain other conditions, as conclusively justifying its denial. In the very nature of the case, we could not possibly have come to understand what it could be for the statement to be true independently of that which we have learned to treat as establishing its truth: there simply was no means by which we could be shown this."

One way of describing what appears to be Dummett's argument has been expressed as follows,[10]

"An anti-realist insists that linguistic competence cannot involve a conception of circumstances other than those which a language learner had available to his consciousness in learning the language."

And,[11]

"All that can be imparted, by the training which results in competence with a language, is an ability to suit one's linguistic behaviour to circumstances which impinge on one's consciousness."

These statements are somewhat obscure but perhaps they can be expressed as follows: the meaning of a statement cannot be given by circumstances which we cannot perceive to obtain. 'Perceive' occurs in this statement of the view as one way of representing what may be meant by 'circumstances available to consciousness'. I believe that a number of problems are raised by the question of what we can or cannot be said to perceive but I do not intend to discuss them. I will, rather, concentrate on the problem of how we should understand 'circumstances'. I believe that concentrating on this problem will prove sufficient to undermine Dummett's anti-realism.

I can now explain the strategy of my argument. The main burden of the argument will be to establish that linguistic competence must involve a conception of circumstances not presently available to consciousness. (Call this conclusion C). If established this shows that Dummett can afford neither to accept C nor to reject C. He cannot afford to accept it because it shows that there is nothing amiss with realism. On the other hand, Dummett cannot afford to reject C since its rejection would make the task of explaining language acquisiton impossible.

In order to present the argument for C I want to discuss a traditional picture of how we are supposed to learn the meaning of a present tensed observation sentence. (My argument does not depend upon taking this picture to be correct). Suppose that the sentence is, 'It is raining'. The learner is exposed to rain events on occasions T_1, T_2 ,

...T_n, and eventually, by interrogating his teacher, learns the meaning of the sentence. But how can anyone acquire this capacity if the anti-realist view is correct, since all that has ever been present to the learner's consciousness was the series of events T_1, T_2,...T_n ? Someone who had learnt the meaning of 'It is raining' would know that that sentence could be truly asserted when it is raining at the time T_{n+1} which is a circumstance that has not previously been available to his consciousness. One can interpret the phrase 'circumstances which impinge on one's consciousness' either in a strong or in a weak manner. On the strong interpretation, every experience one has is unique, e.g. the experience of a rain event at time T_1 . In referring to the strong interpretation I will speak of the experience of event tokens. On the weak interpretation of the phrase 'circumstances which impinge on one's consciousness' one may experience the same circumstances on different occasions, e.g. one may experience rain events at the times T_1, T_2,...T_n. In referring to the weak interpretation I will speak of the experience of event types. I believe that a dilemma now faces the anti-realist: either he is taking 'circumstances' to be event tokens or he is taking them to be event types. If he takes the former view then there is a problem not just about explaining linguistic competence with past tensed sentences, but linguistic competence with present tensed sentences. That interpretation gives rise to too many problems about explaining linguistic competence - it leads to a momentary solipism; since all anyone can ever experience are circumstances at a given time one can never experience the same thing twice. It seems clear that this view makes the task of explaining linguistic competence impossible. On the other hand, if the anti-realist takes 'circumstances' to be event types, there seems to be no more difficulty about explaining linguistic competence with respect to past tensed sentences than there is in

explaining it with respect to present tensed sentences. Someone who has learnt the meaning of a present tensed sentence such as 'It is raining' has obtained a conception of circumstances not available to consciousness, viz. they have learnt what would make that sentence true. Now, however, one must ask what is supposed to be the difficulty in learning the meaning of the sentence 'It was raining'. It is said that no one has ever observed the circumstance that it was raining. Since any explanation of linguistic competence must acknowledge the fact that we have the ability to recognize event types, and not just event tokens, one must ask what this supposed to show. To have the ability to recognize event types is to have the ability to recognize circumstances which are not now (but may have been or become) available to consciousness. If we have that ability why should we not be able to know what circumstances would have obtained if a a past tensed sentence were true, even though the circumstances are not now, and even never have been, available to consciousness ?

I can summarize as follows: either the anti-realist takes circumstances to be event tokens or he takes them to be event types. Only if the anti-realist takes the former course can anti-realism be justified; but the argument for conclusion C shows that this would make languageacquisition impossible. Note that I am not offering an explanation of linguistic competence (a task which ought, in part at least,to be a task for psychology). The point is that any theory of language learning must make room for the fact that we can recognize event types. Once one has done that however one has allowed room for conceptions of circumstances not available to consciousness. There seems to be a lesson in the philosophy of language that has constantly to be relearnt: there is always more to learning the meaning of any sentence

than meets the eye. The conclusion, devastating to the anti-realist's case, is that far from it being the case that 'liguistic competence cannot involve a conception of circumstances not available to consciousness', that conception is essential to linguistic competence.

Part II: Realism and Truth-Conditions

The central point I wish to establish in this section is that a Davidson style truth-conditional theory does not require, nor vindicate, an absolute conception of truth-conditions. I shall argue that Davidson's truth-conditional semantics is neutral between the relative conception of truth-conditions which I favour, and the absolute conception of truth-conditions favoured by some Davidsonians (e.g. Platts) rather than by Davidson himself.

I wish to begin by discussing Popper's claim that Tarski's theory of truth vindicates a correspondence theory of truth. I believe that Popper's claims about Tarski's theory are inaccurate. Davidson's employment of Tarski's theory in attempting to develop a truth-conditional semantics for natural language has also been mistakenly supposed to vindicate absolutism. Having explored those points, I conclude by discussing Davidson's own views which seem to me to differ significantly from those of some of his followers.

In his (1960) Popper writes,[12] "Tarski's theory...rehabilitated the correspondence theory of absolute or objective truth." And,[13] "Thanks to Tarski's work, the idea of objective or absolute truth - that is, truth as correspondence to the facts - appears to be accepted today with confidence by all who understand it." It is notable, however, that Tarski did not regard his theory as a vindication of a correspondence

theory of truth. Tarski responded to the claim that his theory commited one to an 'uncritical realism' because a sentence like 'Snow is white' is taken to be true if snow is in fact white by saying he would drop the words 'in fact'. Tarski also said,[14]

> "...we may accept the semantic conception of truth without giving up any epistemological attitude we may have had; we may remain naive realists, critical realists or idealists, empiricists or metaphysicians - whatever we were before. The semantic conception is completely neutral toward all these issues."

Whatever else Popper means by an 'absolute' or 'correspondence' theory of truth he certainly intends it to be inconsistent with, say, idealism. I don't wish to suggest that Tarski's own assessment of his theory must be correct because it is his theory; but there are, I think, good reasons for thinking that Tarski's view is a good deal more accurate than Popper's.

The first question to ask is why Popper believes that Tarski's theory represents an 'absolute' or 'correspondence' theory of truth. Popper's reasons seem to be more concerned with Tarski's proposed material adequacy condition for theories of truth rather than with the definition of truth proper. Popper gives the following formulations of under what conditions an assertion corresponds to the facts,[15]

> "(1) The statement, or the assertion, 'Snow is white' corresponds to the facts if, and only if, snow is, indeed, white.
> (2) The statement, or the assertion,'Grass is red' corresponds to the facts if, and only if, grass is, indeed, red."

Popper then went on to say,[16]

> "The decisive point is Tarski's discovery that, in order to speak of correspondence to the facts, as do (1) and (2), we must use a metalanguage in which we can speak about two things: statements; and the facts to which they refer."

And,

"Once the need for a (semantical) metalanguage is realized, everything becomes clear."

However, the (T) schema is only intended to provide a means of discriminating adequate from inadequate definitions of truth, a definition being adequate if all instances of the (T) schema follow from it. It was not Tarski's intention that the material adequacy condition permit only his definition, and it is clear that rival definitions are also compatible with the material adequacy condition. Indeed, even bizarre theories of truth may be compatible with the (T) schema. Haack points out in her (1976a) that if one defines '"p" is true' as '"p" is asserted by the Pope ex cathedra',[17]

> "...a serious proponent of this definition would..accept all instances of (T) such as:
> 'Snow is white' is asserted by the Pope ex cathedra iff snow is white
> For if he takes his own definition seriously, he will accept/reject the left-hand side just in case he also accepts/rejects the right-hand side."

Tarski's employment of the material adequacy condition would therefore seem to be an inadequate reason for taking Tarski's theory to be a 'correspondence' or 'absolute' theory of truth.

Since I do not believe that the kind of theory of truth Popper desires can be justified by Tarski's theory it is necessary to speculate about what it is Popper desires. Popper speaks of the theory of truth he favours as being 'objective', 'absolute' and a 'correspondence' theory. Now a part of what Popper means by 'objective' is indicated by the statement that,[18]

> "The theory of objective truth...allows us to make assertions such as the following: a theory may be true even though nobody believes it, and even though we have no reason to think that it is true; and another theory may be false even though we have comparatively good reasons for accepting it."

As convenient shorthand, I will call the view that sentences may be true or false independently of our beliefs as to which truth-value they have, verification transcendence. Verification transcendence is one of the elements Popper desires his theory of truth to have. The other element

Popper desires can, I think, be described as a theory independent notion of facts. Now I hope to show both that one can, and that it is desirable to, accept verification transcendence without accepting a theory independent notion of facts. Before I attempt to show that it is worth indicating the similarity between Popper's ambitions and those of other realists. Consider first this statement by McGinn,[19]

> "Realism is the thesis that truth (falsity) is an epistemically unconstrained property of a sentence; there is nothing in the concept of truth (falsity) to exclude the possibility that a sentence be unknowably true (false). This property of truth reflects the realist conviction, embodied in our customary linguistic practices, that the world, or a given sector of it, is determinately comstituted, quite independently of any limitations on our capacity to come to know truths concerning it."

Then consider this statement by Platts,[20]

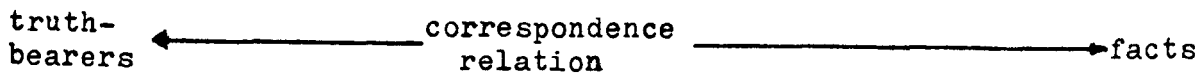
> "Perhaps...we can improve our capacities to understand... the world, to know that our characterizations of it are true. If we succeed in so doing, we do not bring that world into being, we merely discover what was there all along. But that reality will always exceed our capacities: we can struggle to achieve approximately true beliefs about that reality, approximately true beliefs about the entities and their characteristics which, independently of us, make up that reality. But we have to rest with the approximate belief, and resign ourselves to (non-complacent) ignorance: for the world, austerely characterized by our language, will always outrun our recognitional capacities."

Clearly both Platts and McGinn are commited to verification transcendence; less clearly, but I believe implicitly, Platts and McGinn also accept the idea of theory independent facts. Both McGinn's talk of the world being 'determinately consituted' and Platts' emphasis on discovery suggest that this is the case.[21] In any event, I certainly believe that my way of understanding truth-conditions gives a very different emphasis from theirs.

My dissent from Popper, Platts and McGinn does not concern the notion of verification transcendence, which I accept, but the question of whether there are theory independent facts. Moreover, although the view I favour is a relational theory of truth it is not a correspondence

theory of truth.

What the correspondence theorist wants to defend is a picture like this:



What is important about this picture is that there are essentially three independent elements and that the correspondence theorist believes that 'facts' are individuated independently of our theories. That the facts are individuated independently of our theories is what the correspondence theorist believes accounts for the recalcitrance we face in attempting to describe the world truly.

The above picture may appear innocuous because inevitable - it might appear that anyone who supports a relational theory of truth must adopt something like the above view. My suggestion is that one can adopt something like the following picture however:

That is, true sentences will be sentences whose truth-conditions are satisfied. Now it may be said that this picture only differs in the labels used; a different label for the relation ('satisfaction' rather than 'correspondence') and an implied different label for one of the other terms ('satisfiers' rather than 'facts'). But the big difference between these pictures is that the latter picture does not require the idea that the 'satisfiers' are individuated independently of the individuation of the truth-conditions of sentences. The phrase emphasized is crucial and perhaps I can unpack it this way: one can think of truth-conditions as either the truth-conditions of truth-bearers, or one can think of truth-conditions as something like 'states of the world'. Now the latter notion has, I think, a trivial and

innocuous interpretation, but it also has a non-trivial and pernicious interpretation. The latter notion is involved when one thinks: there are facts ('states of the world') and there are the descriptions of possible states of the world provided by truth-bearers - 'truth' is discovered when one is matched with the other. The innocuous interpretation is involved when one thinks that a true statement is a statement whose truth-conditions are satisfied, for this way of putting the matter does not require that the 'states of the world' are individuated independently of our conceptual scheme. One other way of expressing the difference between these views is that the correspondence theorist is inclined to think of 'facts' as though they had ontological import. Popper writes,[22]

> "...a false statement P is false not because it corresponds to some odd entity like a non-fact, but simply because it does not correspond to any fact: it does not stand in the peculiar relation of correspondence to a fact to anything real, though it stands in a relation like 'describes' to the spurious state of affairs that P."

On my view however, a true statement does not stand in some relation to a dubious entity like a fact. Popper also wrote,[23]

> "I am a realist in holding that the question whether our man-made theories are true or false depends upon real facts; real facts which are, with very few exceptions, emphatically not man-made."

Like Popper I believe that truth is not man-made in the sense that one can't make many statements true, but there is a sense in which I believe that truth is man-made in a way that Popper does not; on my view the truth-conditions of all statements are individuated by us (although this does not make them true), but Popper believes that true statements are those which describe 'facts' which are, so to speak, individuated by the 'way the world is.'

If one believes in verification transcendence, what makes the correspondence picture appear inevitable is that it looks as though

what makes statements true or false must be individuated independently of our conceptual scheme. But I believe that is not the case: when communities define the truth-conditions of sentences they don't create truth but only the necessary conditions for truth - to define the truth-conditions for a sentence is not to say whether it is true, for the conditions may or may not obtain (be satisfied); it is to say what would have to be the case for the sentence to be true. Insofar as we define what would have to be the case for the statement to be true then one can account for the epistemic independence of truth not by postulating an independently given set of 'facts' which we have to match, but by saying that the conditions (the 'facts' in scare quotes) are individuated by us (we have defined what would have to be the case for the sentence to be true) but that, of course, does not guarantee that those conditions do obtain.

Where do Davidson's views stand in relation to the above ? The first important point is that Davidson is well aware that his theory does not explain the notion of 'facts' - indeed Davidson refers to the "...failure of correspondence theories based on the notion of fact.."[24] (My emphasis). Davidson also says, surely correctly, that a theory of the kind he favours does not "..really illuminate the relation of satisfaction. When the theory comes to characterize satisfaction for the predicate 'x flies', for example, it merely tells us that an entity satisfies 'x flies' if and only if that entity flies. If we ask for a further explanation or analysis of the relation, we will be disappointed."[25] That point is crucial since Platts writes as though a Davidson style truth-conditional theory does illuminate that relationship; Platts implies that the theory has vindicated the idea of 'merely discovering what was there all along.' I think that a truth-conditional theory

like Davidson's is neutral in the sense that one might want to add to it in the hope of explaining the basic relation of satisfaction that Davidson (like Tarski) treats purely enumeratively. It is quite wrong, however, to suppose that Davidson's theory as it stands justifies the kind of realism that Platts and McGinn favour. There is an irony in the fact that Platts seems to believe that he is expressing Davidson's own views since Davidson himself, in his (1977), makes it clear that he does not believe that one can say very much to elucidate the basic relation of satisfaction beyond giving the kind of theory he himself provides. In that belief I believe that Davidson is wrong, for I believe that I have explicated the relation between language and the world in a way that does clarify an intuitive notion of satisfaction that is consistent with Tarski's technical definition. My way of explicating that relationship, however, far from vindicating the absolutism Platts and McGinn desire, provides reason for thinking absolutism to be incoherent.

Chapter 6

Realism and Science

If absolutism is taken to be the view that, in principle, there is a way of knowing the world which is entirely non-anthropocentric, a way that is entirely independent of our parochial interests, purposes, (etc), could not science plausibly be said to be a vindication of that view ? I think that that claim is indeed plausible but, when examined closely, turns out to be highly problematic. The claim that science is a vindication of absolutism gains credibility by adopting a broad historical perspective; I think it is true to say that modern science has been more successful because less anthropocentric - perhaps the best, and simplest, example being the change from a Ptolemaic to a Copernican world view. There is, however, a large difference between the claim that science has been more successful because less anthropocentric and the claim that science can be entirely non-anthropocentric. Against an absolutist view of science I would urge two arguments: (i) an argument from pluralism - there is not one way of gaining knowledge about the world (the scientific way) but many ways of gaining knowledge about the world. I think that it is a mistake to view commonsense beliefs about the world, for example, as crude proto-scientific beliefs. I present the argument from pluralism in part II. (ii) Even ignoring the issue of pluralism, I believe that a consideration of the issues surrounding the question of realism in science supports a modest realism rather than the stonger version required by absolutism. My argument for this conclusion will occupy part I.

Part I: Realism vs. Instrumentalism

(1) Two Analogies

Analogy 1: Imagine a watch and suppose that for some reason we are unable to open the back to examine the mechanism. Supposing that we knew it to be in working order - we could observe the second hand moving around the face - we would have no doubt that there is a mechanism inside which caused the watch to work. We might speculate about what kind of mechanism was responsible for the observable behaviour of the watch. Suppose that we postulated that the mechanism was clock-work but that later we became aware of other possible kinds of mechanisms, e.g. that a system based on a quartz crystal could be responsible for the movement of the hands. The latter hypothesis might gain support by the observed accuracy of the watch over a long period. The important point of this analogy is that whatever kind of mechanism one postulates there is a fact of the matter as to which hypothesis is the correct one. One 'realist' view of science suggests that the watch analogy is appropriate.

Analogy 2: The second analogy is based upon an impressive visual illusion.[1] The central feature of the set up which creates the illusion is a board which has two shapes 'impressed' upon it. One of the shapes is convex and the other shape is concave (relative to the rest of the flat surface of the board). Both of these shapes are such that they resemble the human face. The visual illusion is that even if one knows that one of the shapes is concave it appears, from a wide variety of angles, to be convex. It is only at the most extreme angles that the concave nature of one of the shapes becomes apparent. A plausible explanation of this phenomenon might exploit the fact that we are so used to seeing the human face which is convex that it is very difficult to see that shape

as concave. One other feature is relevant before drawing the analogy with science. The board with the two shapes is mounted in a shallow box and to the observer the box appears to become distorted as it is tilted one way or the other.

The analogy with science can be brought out as follows: imagine an observer who has no independent way of knowing that one of the shapes is concave. Purely on the basis of that observer's observations it would be entirely natural for him to postulate a mechanism within the box which caused it to become distorted as it was tilted. One can imagine two observers arguing about the best kind of mechanism that would account for the experience. But of course they would both be wrong - there is no 'hidden mechanism' and, in that sense, nothing to be right or wrong about although they would both be misguided in supposing there to be a mechanism at all. Whereas the first analogy was intended to suggest a realist view of science, the second analogy is intended to suggest an instrumentalist view of science.

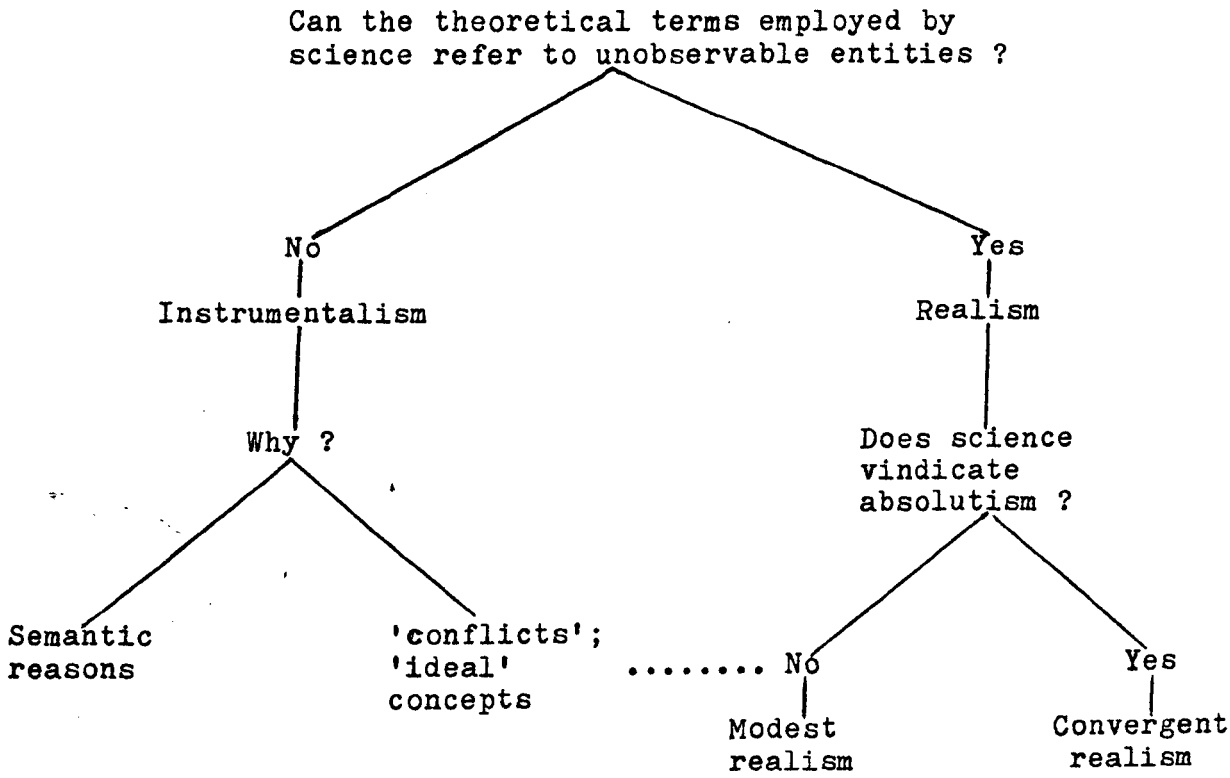
The presentation of two extreme pictures should sound warning bells and that indeed was one of my purposes in presenting both analogies. One good question is whether we have any real reason for supposing that one of the analogies must be appropriate for the <u>whole</u> of science. Might it not be that the watch analogy fits fairly well with some parts of science and that the visual illusion analogy fits better with other parts of science ? Realism and instrumentalism have generally been presented as mutually exclusive views but that may not be reasonable. A second good question is whether or not <u>both</u> pictures might be misleading. One issue is the difference between <u>metaphorical</u> and <u>literal</u> truth. The instrumentalist picture suggests that literally speaking

there are no unobservable entities, the realist picture suggests that literally speaking there are unobservable entities, but could both be wrong ? For instance, because of our macro experience of waves and particles we can't see how both a wave theory of light and a particle theory of light could be compatible, and the realist suggestion is either one is correct or the other is correct (or some third as yet undiscovered theory), but if the talk of 'waves' and 'particles' is really metaphorical, if the talk is really just based on loose analogy with macroscopic experience, both theories may be 'true'. The absolutist cannot accept the suggestion that even scientific truth might be metaphorical for to say that it is metaphorical might be to admit its relativity to our experience. That is, if we cannot rid science of its simile and metaphor, we would merely be saying how the world appears from our point of view, why some aspects of reality seem to us to be like others - but the absolutist cannot rest content with this. An absolutist would consider it infra dig to suggest that scientific truth might, to some degree, be metaphorical; but I for one do not see how to draw the literal/metaphorical distinction clearly. I can explain that point in relation to a watch. Presumably talk of the 'hands' of a watch grew from a metaphor about human hands, as did the idea of the hands 'pointing' to the figures on the 'face' of the clock. I do not see much sense in forcing the question 'do the hands of the clock literally point to the figures on the clock-face ?' I admit, however, that the considerations so far adduced are but dark musings - time to be more specific. I shall be more specific by working through the positions marked in figure 6 overleaf. I shall work from the left-hand side to the right-hand side.

Semantic motivation for instrumentalism arises from the conjunction of two epistemological theses: (i) that terms employed meaningfully in

Figure 6

Some Views of the Status of Theoretical Terms in Science



..... = affinity

some empirical discourse must have a direct relationship to observable experience; (ii) that there is a sharp theoretical/observational dichotomy. Since, by definition, theoretical terms do not have a direct relationship to observable experience the instumentalist concludes that they should not be thought of as referential at all. Point (i) is a variant of the verification principle, against which I have already said much in earlier chapters. In this context I would urge the view that points (i) and (ii) stand or fall together - if there is not a sharp distinction between theoretical terms and observational terms, point (i) cannot be justified. (Certainly the logical positivists would hardly, self-defeatingly, have wanted to deny that relatively observable terms were meaningful). One of the few major points of agreement of much recent philosophy of science has, however, been that no such sharp distinction can be drawn - at best there are degrees to which a term may be said to be more or less theoretical. Two criteria suggest themselves for drawing the distinction between observational terms ('O' terms) and theoretical terms ('T' terms); (i) that 'O' terms may be taught ostensively; (ii) the ease and degree of confidence that we can have in the application of 'O' terms. With respect to the first criterion there does seem to be a prima facie difference between 'T' terms such as 'electron', 'quark', whose meaning can only be learnt by learning a scientific theory, and terms such as 'warm' and 'red' which can be learnt directly. Two things make this a question of degree however. First, 'ostension' is a more indirect process than might appear - there is more to seeing than meets the eye.[2] The distinction between 'seems red' and 'is red' would seem to be theoretical in that it cannot, presumably, be learnt by pure ostension (if there is such a beast). Second, some putative 'T' terms e.g. 'force' can, it would seem, be taught as directly as other putative 'O' terms. One might explain the

concept of force by referring to the experience of feeling the pressure of wind on one's body. The second criterion, proposed by Carnap,[3] would seem to make the O/T distinction explicitly one of degree unless the idea of incorrigible sense experience is taken on board (an idea which attracts few supporters today given the problems of phenomenalism). For these reasons and others it seems better to admit frankly that the distinction between observable terms and theoretical terms is one of degree. Newton-Smith's proposed scale seems sensible enough,[4]

1. The more observational a term is, the easier it is to decide with confidence whether or not it applies.
2. The more observational a term is, the less will be the reliance on instruments in determining its application.
3. The more observational a term is, the easier it is to grasp its meaning without having to understand a scientific theory.

This view of observational and theoretical terms seems to me to undermine a major rationale for instrumentalism: given that the difference between 'T' terms and 'O' terms is one of degree one would not expect a radical difference in their semantic function and, hence, no reasonable reason for denying that 'T' terms may refer.

Turning to the second position under 'instrumentalism' in figure 6, it would seem that in certain parts of scientific discourse a <u>prima facie</u> case can be made for saying that the theoretical concepts employed in the theory do not refer or that the theories in question should not be thought of as literally true. Two kinds of case suggest this view: (i) the use of 'ideal' concepts; (ii) cases where different theories dealing with the same range of phenomena are apparently incompatible. Examples of (i) might include concepts of 'instantaneous velocity', 'perfect vacum', 'perfect elasticity' and so on. In the case of some concepts it seems impossible that, understood literally, they could apply to any existing thing. As Nagel puts it,[5]

> "...we can attribute a velocity to a physical body only if the body moves through a finite, nonvanishing distance during a finite, nonvanishing interval of time. But instantaneous velocity is defined as the limit of the ratios of the distance and time as the time interval diminishes toward zero. In consequence, it is difficult to see how the numerical value of this limit could possibly be the measure of any actual velocity."

Cases of (ii) might include theories which analyse gas as an aggregation of discrete particles and theories which represent the gas as a continuous medium; wave vs. particle theories of light. I shall discuss the problems raised by (ii) in the following section.

In his (1960) Nagel reached the following conclusion concerning the debate between instrumentalists and realists,[6]

> "It is ...difficult to escape the conclusion that when the two apparently conflicting views [instrumentalism and realism] on the cognitive status of theories are stated with some circumspection, each can assimilate into its formulations not only the facts concerning the primary subject matter explored by experimental inquiry but also all the relevant facts concerning the logic and procedure of science. In brief, the opposition between these views is a conflict over prefered modes of speech."

There is, I think, something importantly correct about Nagel's view with respect to some versions of instrumentalism and some versions of realism and I hope in what follows to bring this out. One response to the problem of 'ideal' or 'limiting' concepts is to make a move towards holism. One might argue as follows: we may not, for example, be able to ascertain by overt measurement the value of an instanteneous velocity of the magnitude of some length whose theoretical value is stipulated to be equal to the square root of 2. But unless accessibility to overt measurement is made the criterion of physical existence this does not show that bodies cannot have instantaneous velocities or lengths with real number magnitudes. On the contrary, if a theory postulating such values is supported by evidence, then there is good reason to maintain that these limiting concepts do designate certain phases of things and processes. Since in testing a theory we test the totality of assumptions

it makes, if a theory is regarded as well established on the available evidence, then all its component assumptions must also be regarded as well established. Accordingly, unless we introduce arbitrary distinctions, we cannot pick and choose between the component assumptions, counting some and not others as descriptions of what exists. Much depends upon how we understand the idea of reference. Nagel discusses a number of things that may be meant by talking of the physical reality to which theoretical concepts may refer. The second criterion he discusses is similar in many respects to the holistic view just mentioned,[7]

> "...every nonlogical term of an assumed law (whether experimental or theoretical) designates something physically real, provided that the law is well supported by empirical evidence and is generally accepted by the scientific community as likely to be true."

I take it that by 'likely to be true' Nagel intends something equally epistemic, e.g. likely to recieve further confirmation as the result of further investigation. This criterion could be made more restrictive along the lines of Nagel's third criterion,[8]

> "...a term designating anything physically real must enter into more than one experimental law, with the proviso that the laws are logically independent of each other and that none of them is logically equivalent to a set of two or more laws."

Now I take it that criteria for the physical existence of theoretical entities along the lines mentioned above are liberal and epistemic. Indeed, they are liberal because they are epistemic - no metaphysical constraints enter the picture. For that very reason these criteria would be considered too weak to satisfy many realists (some of whom we shall meet below). However, the kind of realist I call the 'modest realist' may not be far apart from the instrumentalist. As Nagel says,[9]

> "...if the third of the..criteria is adopted for specifying the sense of 'physically real', it is quite patent that the instrumentalist view is compatible with the claim that atoms, say, are indeed physically real."

Or, as he also nicely puts the point,[10]

> "...there are at least some senses of the expressions 'physically real' and 'physically exists' in which an ironically minded instrumentalist can acknowledge the physical reality or existence of many theoretical entities,"

(I think that I may be described as an ironically minded realist !) Hence Nagel's point that the debate between instrumentalists and realists may largely concern a prefered way of speaking. (That is what the dots in figure 6 represent). Nevertheless, even against the ironically minded instrumentalist there are, I think, good reasons for prefering the realist way of speaking. One reason that I have is to reject the implication that because the evidence for theoretical terms is holistic this is not the case with non-theoretical entities, i.e. those macro sized objects that we can observe in a more direct manner. Since I believe that the relationship between experience and the terms employed in everyday discourse, and experience and the terms employed in scientific discourse, is fundamentally similar in both cases, I find no good reason for saying that theoretical entities don't exist and non-theoretical entities do exist. To develop this view further I will discuss the underdetermination of theory by data in the next section and then contrast my views with those of the convergent realists in the final section.

(2) Realism and the Underdetermination of Theory

The question of the underdetermination of theories concerns the question of whether there could be two empirically equivalent theories which were logically incompatible. In his (1970b) Quine expressed the problem thus,[11]

> "Consider all the observation sentences of the language: all the occasion sentences that are suited for use in reporting observable events in the external world. Apply dates and positions to them

> in all combinations, without regard to whether observers were at the time and place. Some of these placed-timed sentences will be true and others false, by virtue simply of the observable though unobserved past and future events in the world. Now my point about physical theory is that physical theory is underdetermined even by all these truths. Theory can still vary though all possible observations be fixed. Physical theories can be at odds with each other and yet compatible with all possible data even in the broadest sense. In a word, they can be logically incompatible and empirically equivalent. This is a point on which I expect wide agreement, if only because the observational criteria of theoretical terms are commonly so flexible and fragmentary."

Quine's expectation of 'wide agreement' suggested that he thought the thesis uncontentious but the major problem the thesis faces can be expressed by asking why we should count as distinct, theories which agree on all possible observations, i.e. can't we say that here we merely have a case of equivocation over the meaning of theoretical terms ?[12] Quine's confidence concerning the thesis was much less in evidence when he returned to the problem in his (1975b) where he comments that, "The doctrine is plausible insofar as it is intelligible, but it is less readily intelligible than it may seem."

Now the main problem I want to consider (because I believe that it is the major problem) is that of equivocation; but first I want to mention two points without exploring them. The first point is that if one accepts that there is no really sharp theoretical/observational dichotomy the thesis will have to be relativized to some particular way of drawing the distinction. Second, as Quine notes, the notions of implication, logical consequence and logical equivalence are "..clear so long as the theory formulations and their consequences are couched in our regimented scientific language, with its explicit logical notation."[14] I will follow Quine in supposing that we are talking only of theories formulated "..in our language with the help of our own regimented logical notation." Taking these points for granted I turn to the problem of equivocation.

As an illustration of the problem of equivocation Quine imagined the following: take some theory formulation and select two of its terms, say 'electron' and 'molecule'. (Suppose that these do not occur in any observation sentences). Transform the theory formulation merely by switching these terms throughout. This new theory formulation will be logically incompatible with the old; it will affirm things about so-called electrons that the other denies. Yet, as Quine has the 'man in the street' say, their only difference is terminological; the one theory formulation uses the technical terms 'molecule' and 'electron' to name what the other formulation calls 'electron' and 'molecule'. Clearly the two formulations are empirically equivalent - they imply exactly the same observation sentences. Quine agrees with the man in the street however: we should individuate theories in such a way that we regard these formulations as formulations of the same theory. Quine proposes that we count formulations as identical if besides being empirically equivalent the two formulations can be rendered identical by switching predicates in one of them. Quine then broadens this proposal to allow for more complex cases than the one mentioned. Three ways of broadening the proposal are mentioned: (i) we should not require that a switching of terms render formulations identical, only logically equivalent; (ii) we should not limit the permutation to a switching of two predicates - we should allow the switching of many; (iii) we should not require the transformation to carry predicates always into simple one word predicates. With this broadening Quine states his criteria for the individuation of theories thus,[15] "...two formulations express the same theory if they are empirically equivalent and there is a reconstrual of predicates that transforms the one theory into a logical equivalent of the other." After discussing some of the implications and complications that arise, Quine finally arrives at the following,[16]

> "...a last ditch version of the thesis of underdetermination would assert merely that our system of the world is bound to have empirically equivalent alternatives which, if we were to discover them, we would see no way of reconciling by reconstrual of predicates."

Having said something about the thesis of underdetermination I want to discuss Newton-Smith's claim that it presents a dilemma for realism. Newton-Smith identifies two major ingredients in realism: (i) the ontological ingredient, "...scientific theories are true or false and which a given theory is it is in virtue of how the world is."[17] (ii) the epistemological ingredient, "...we can have warranted beliefs (at least in principle) concerning the truth-values of our theories."[18]

Newton-Smith believes that there may be underdetermination and, given that, thinks that one must weaken either the ontological ingredient or the epistemological ingredient. Weakening the ontological ingredient Newton-Smith calls the ignorance response: there may be empirically equivalent theories between which we cannot have empirical reasons for choosing but nevertheless one may be true and the other false although we may never know which. Weakening the epistemological ingredient Newton-Smith calls the arrogance response: if there are theories between which we cannot have empirical evidence then there is no fact of the matter as to which is true. In Newton-Smith's view the ignorance response has the consequence of having to embrace the notion of inaccessible facts, whilst the arrogance response has the consequence of having to abandon the law of excluded-middle: empirically undecidable propositions are neither true nor false. Surprisingly, perhaps, Newton-Smith opts for the arrogance response not wishing to adopt the notion of inaccessible facts.[19] My response is to reject the dilemma as posed by Newton-Smith and my reasons may begin to emerge by considering this notion of inaccessible facts.

I believe that the notion of inaccessible facts is ambiguous in just the way that talk of 'truth-conditions' is ambiguous. When one speaks of truth-conditions one may mean the truth-conditions of a sentence or one may mean the conditions which obtain in the world itself. Now there is an important sense in which we always have access to the truth-conditions of any sentence we find meaningful. For instance, I find the sentence, 'A city will never be built on this spot.' perfectly meaningful - I do have access to the truth-conditions of that sentence in the sense that I know what would have to be the case for the sentence to be true. I do not, however, have access to the truth-conditions in the sense that I do not know (and have no way of knowing) whether those truth-conditions are satisfied. If by 'inaccessible facts' one means that there are sentences which have truth-conditions which we cannot discover if they obtain or not then I do believe that there are 'inaccessible facts'. What I believe Newton-Smith had in mind, however, is more like the use of 'truth-conditions' where one thinks of them as 'states of affairs'. 'States of affairs' in this sense can sometimes be thought of as physical states of the world. Consider this notion of inaccessible facts in relation to the sentence 'A city will never be built on that spot.' Suppose that that sentence is uttered at time t. Now, whatever else may be meant by 'inaccessible facts' in this sense, it entails that every state of affairs after time t is inaccessible. Newton-Smith rejects this notion of 'inaccessible facts' and, I think, with very good reason for it is very hard to say much about them which is intelligible. Unlike Newton-Smith however, I do not believe that this should lead one to adopt the arrogance response. On my view there is inaccessibility at time t but it is not the inaccessibility of some mysterious future state of affairs but the epistemic inability to know what will or will not happen. Not knowing what will

or will not happen after time t is not the same as not knowing what would or would not have to happen for the sentence to be true. I claim, indeed, that we do know the latter but not the former.

It seems to me vital to reject the premises that led Newton-Smith to pose the realist dilemma. It is crucially important to our conceptual scheme in general and our science in particular that we retain the notion of epistemic inaccessibilty. In fact, as Quine presents the thesis of underdetermination, epistemic inaccessibility is taken for granted since he talks of all possible evidence and explicitly talks of observation sentences being 'pegged to inaccessible time-places.'[20] As I have argued in chapter 5, the demand for epistemic access threatens to make a mystery of all language learning.

Quine anticipated a dilemma similar to that raised by Newton-Smith and responded in much the way I favour,[21]

> "Perhaps there are two best theories that imply all the true observational conditionals and no false ones. The two are equally simple, let us suppose, and logically incompatible. Suppose further ...that they are not reconcilable by reconstrual of predicates however devious. Can we say that one, perhaps, is true, and the other therefore false, but that it is impossible in principle to know which ? Or, taking a more positivistic line, should we say that truth reaches only to the observational conditionals at most..?
>
> I incline to neither line. Whatever we affirm, after all, we affirm as a statement within our aggregate theory of nature as we now see it; and to call a statement true is just to reaffirm it. Perhaps it is not true, and perhaps we shall find that out; but in any event there is no extra-theoretic truth, no higher truth than the truth we are claiming or aspiring to as we continue to tinker with our system of the world from within."

In other words, Quine rejects the ignorance response because truth is internal to our theory of the world and, that being the case, we have equally good reason for saying that both theories are true. Saying that both theories are true is, however, equally a way of rejecting the arrogance response since that involves saying that (parts of) both theories are neither true nor false. What, then, of 'incompatibiltity' ?

As we saw, in Quine's account this amounts to merely saying that we have no way of reconciling one theory in terms of the other. As far as I can see this really amounts to denying an intuitively appealing but misleading picture of scientific discovery. One kind of scientific realism suggests that there must be one scientific theory of the world to describe the structure of the world. Quine's view really amounts to the suggestion that there may be more than theory to describe the structures (plural) of the world. If someone claims that unless we can discover one to describe the structure of the world this shows that we cannot discover the truth about the world, Quine's response, and it is mine also, is to say that this is a misunderstanding of the concept of truth; insofar as the concept of truth is employed meaningfully we must be talking about truth within our theories. Certainly we can talk about sentences being true or false when we cannot discover which, but the imagined realist must want more than that since we are imagining the truth-values of all sentences settled. Just what more the realist is just that which defies explication. The ultimate outcome of the view for which I am arguing is that metaphysics (or, rather, good metaphysics) and epistemology meet. That is to say that we can't ultimately separate 'what there is' from our reasons for believing in what there is. (Not, however, in the way verificationism drew this relationship). This form of relativism rejects both the idea of an 'extra-theoretic truth', and a theory-independent notion of 'objects' (a point I shall expand upon in the next section). Just because our notion of relatively unobservable entities is theory-dependent we have no way of knowing when there is real or merely apparent conflict between theories. For example, some experimental evidence seems best explained by thinking of light as though it were wave-like, other experimental evidence seems

best explained by thinking of it as though it were constituted by particles. Insofar as we now have good reason for accepting both theories we also, I claim, have good reason for saying that talk of 'waves' and 'particles' is only analogous and that the conflict is more apparent than real. Perhaps we should think of a continuum with the watch analogy at one end and the visual illusion analogy at the other. Insofar as two theories are equally good at explaining the evidence, and insofar as these theories are in apparent conflict, there is good reason for thinking of those theories as being nearer to the 'illusion' end of the spectrum rather than the 'watch' end of the spectrum. If some day a unified theory of light were developed which accounts for all the experimental evidence without apparent conflict that would, perhaps, ex post facto, be reason for saying that the conflict was, after all, more real than apparent. The reason however would not be that we had discovered a non-theoretic truth, but that we had found a better theory-dependent truth. I shall continue this theme in the next section.

(3) Convergent Realism

> "The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle."
> Putnam.22.

In the sense of 'realism' that Putnam had in mind in the above quotation, I believe that his claim is false. That conception of realism has been labeled 'convergent realism' and in addition to Putnam has been advocated by Newton-Smith and Boyd.[23] I believe that a modest realism can achieve what is wanted without incurring the difficulties of convergent realism.

Any advocate of realism with respect to theoretical entities in science will require a distinction between terms which genuinely refer and those which merely purport to refer. This distinction is needed because of such terms as 'aether' and 'phlogiston' which we now believe do not refer. With this distinction the central thesis of convergent realism may be stated as follows: theoretical terms refer if they describe the underlying structure of the world; that is, theoretical terms genuinely refer if there are substances or things in the world that correspond to the ontology of the scientific theory in question. The criterion for saying that terms do genuinely refer is epistemic success; the more successful a theory the more reason for supposing that it does genuinely refer. Moreover, and this is an important claim for the convergent realists, it is said that the supposition that the terms of successful theories do genuinely refer explains their success. Science faces a recalcitrant world and attempts to describe its underlying structure. The progress of science (measured by epistemic success) is evidence that its theories are getting nearer to a true description of the structure. Moreover, it is claimed, any philosophy that does not take this view would be at a total loss to explain why on earth science should be successful. (Convergent realists have to speak of successive approximation to the structure of reality since no one wants to be saddled with the view that today's science, and the ontological commitments of today's science, will be the final one. I think, however, that there are grounds for saying that convergent realists are too committed to today's science. I will return to this point.)

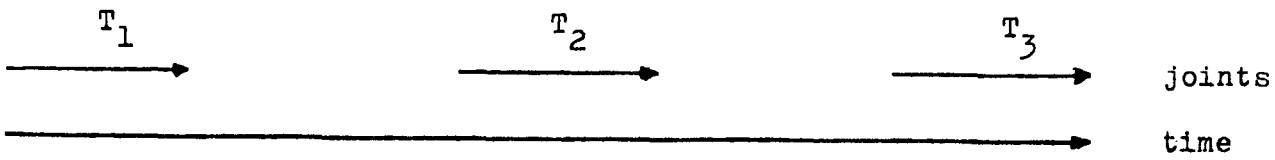
Before I tackle the central claims of this thesis a word on 'epistemic success' is called for. To my knowledge convergent realists have not been very specific about what epistemic success amounts to -

they seem to be operating with a largely pragmatic notion: we would say that a theory is successful if it makes substantially correct predictions; passes a battery of tests; leads to efficacious interventions in the natural order. As Laudan notes, the criteria for success cannot be made too stringent since otherwise the very phenomena to be explained (the success of science) would disappear.[24] However 'success' is to be spelled out exactly I don't wish to question the claim that science has been successful - hence I shall treat the claim about the success of science as common ground. I also won't question some of the theses that convergent realists are committed to that I do believe to be problematic, e.g. the claim that earlier theories are always limiting cases of later theories.

My central objection to convergent realism is that it requires a theory-independent notion of ontology which, as a relativist, I do not believe makes sense. What I see as a <u>weakness</u> in the view of convergent realists has, I think, been claimed by them to be a <u>strength</u>. Underlying Putnam's claim that (convergent) realism is the only view that does not make the success of science a miracle, has been an implicit argument that only a theory which embraces the idea of a theory-independent ontology can give a plausible explanation of why one theory should be better than any other. My first task is to show why I do not believe this to be the case.

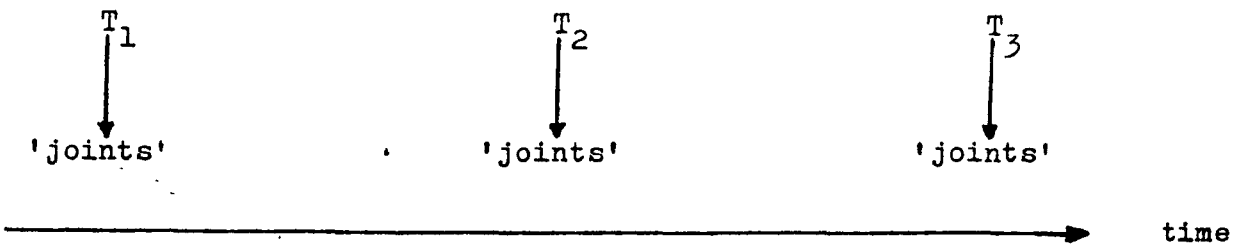
To explain the difference between convergent and modest realism I shall use the word <u>joints</u> (no scare quotes) to represent the convergent realist's notion of a theory-independent ontology; I shall use the word 'joints' (with scare quotes) to represent my notion of a theory-dependent ontology. With that distinction, I think it is helpful to view the difference in points of view as follows:

Convergent Realism



Where: T_3 is an epistemically superior theory to T_2 and T_2 is an epistemically superior theory to T_1.

Modest Realism



(I use separate arrows to take on board Kuhn's point that scientific theories sometimes progress by the adoption of a radically new theory).

The essential difference between these 'pictures' is that whereas the convergent realist has a theory-independent notion of ontology (upon which more successful theories are supposedly 'converging'), modest realism only requires a theory-dependent notion of ontology. This essential difference can also be represented by saying that whereas convergent realism can be defined as a relationship between <u>three</u> elements, modest realism only requires <u>two</u> elements:-

<u>Convergent Realism</u>

(i) theory independent notion of objects (joints);

(ii) theory dependent notion of truth-conditions;

(iii) a correspondence relation between (i) and (ii).

Modest Realism

(i) a theory-dependent notion of truth-conditions ('joints');

(ii) the satisfaction (or not) of the truth-conditions mentioned in (i).

Perhaps the first thing I ought to say concerning these alternative views concerns the idea of theory-dependent truth-conditions. Above I noted that sometimes truth-conditions are spoken of as 'states of the world' rather than as a predicate of sentences. The convergent realist needs the concept of the truth-conditions of truth-bearers no less than the modest realist. The convergent realist hopes to capture what is meant by the idea of truth-conditions as states of the world by the notion a theory independent ontology. Both the convergent realist and the modest realist agree that the truth-conditions of sentences are theory dependent - they are defined by us. Both the convergent realist and the modest realist also agree that merely defining truth-conditions does not create truth; but the difference between them concerns what further conditions have to obtain for a sentence to be true: the convergent realist requires a correspondence between the 'independent way the world is' and the truth-conditions of sentences; the modest realist only requires that the truth-conditions of sentences be satisfied.

Now it seems to me that the supposed strength of convergent realism - its ability to explain why some theories should be epistemically superior to others - can equally well be claimed for modest realism. That is to say, convergent realists are fond of saying that a good explanation of why one theory should be better than another is that the better theory represents a better approximation to the independently given way the world is. I believe, however, that modest realists can equally explain why one theory might be better than another without invoking the need for an independently given way the world is. The modest

realist may do this by saying that one reason why a theory may be better than another is that the truth-conditions of the better theory are satisfied and that the truth-conditions of the other theory are not satisfied. Thus suppose that T_1 is a theory accepted at time t, T_1 is commited to the existence of xs; T_2 is a theory accepted at a time later than t, T_2 is not commited to the existence of xs but to the existence of ys, and that T_2 is more epistemically successful than T_1. (I am also supposing that T_1 and T_2 are attempts to explain the same range of phenomena). In these circumstances the convergent realist claims that a good explanation of why T_2 should be better than T_1 is that it is a better approximation to the theory-independent ontology of the world. What the modest realist can say however is that one explanation of why T_2 may be better than T_1 is that although at time t people believed that the truth-conditions of T_1 were satisfied, (which would of course include the assertion that there are xs), the greater epistemic success of T_2 is reasonable evidence for supposing that the truth-conditions of T_1 were not satisfied and that the truth-conditions of T_2 are satisfied, and that therefore there are ys and not xs. Sofar as the explanation of success goes these theories are equivalent. Note further that modest realists have no need to compromise their fallibilism: the convergent realist will say that in principle even the best theory accepted now may be false because its ontological commitments may not correspond to the independently given ontology of the world, but the modest realist will take that claim to be equivalent to saying that at some time in the future we might arrive at a better theory.

The argument that convergent realism and modest realism are equivalent with respect to the problem of explaining the success of science might suggest that this is another case which falls under Nagel's

dictum that disputes over realism concern a 'preferred way of speaking'. I believe that modest realism is philosphically preferable to convergent realism since it does not require the highly problematic notions of a theory-independent ontology and 'correspondence'.

My definition of realism frankly makes it relative to the current state of scientific theory. I do not believe that this is objectionable since I do not believe that we have any better theory to work with. It implies that our judgements of the kinds of things there are vary with epistemic progress. I hardly think that is controversial. My view does not imply that what there is varies with theory in the sense that what exists is a function of theory production. Should we later withdraw our present belief that a term employed in current science is genuinely referring we will naturally conclude that formerly we held a false belief. Convergent realism, on the other hand, seems to make the mistake of treating our current beliefs as sacrosanct. Suppose that in the future science undergoes a radical conceptual change in such a manner that a good many of the terms we now regard as referring we would then regard as merely purporting to refer.(And who will gainsay that possibility ?) I see no difficulty with this prospect for my modest realism but convergent realists would be embarrassed - at least convergent realists <u>should</u> be embarressed since they would be the terms they were telling us it would be a miracle if they did not refer. One might add that if the argument for convergent realism is good argument <u>now</u>, wouldn't it have been a good argument at any time in the history of science. If it was a good argument in the past hasn't the 'miracle' already occured ?

Part II: Physics and Commonsense

Before discussing the specific topic of how scientific and commonsense beliefs are related it is worth raising the question of whether commonsense beliefs can properly be said to constitute a 'theory' in any sense at all. In one way it would seem at best to be an exaggeration to describe our interrelated system of commonsense beliefs (about people, houses, etc.) as constituting a 'theory', not only because it strains the word 'theory', but because the primary function of such beliefs seems to be practical rather than cognitive. There does seem to be an important difference in purpose served by the ontology of a scientific theory (explaining the events and the nature of the world around us) and the 'ontology' of everyday beliefs. Our primary interests in tables and chairs, homes and gardens, even mountains and lakes, is not basically or usually theoretical. That said, however, it is surely also true that those interests do give rise to claims that may be true or false, and if that is the case why should one put the word 'ontology' as it occurs in the phrase, 'ontology of our everyday beliefs', in scare quotes ? My reason is the one given - an admission that the reasons we (as a community) have arrived at such an ontology are primarily practical. That said, I see no good <u>metaphysical</u> reason for being patronizing about the objects familiar to us all. If we are interested in the question of what there is, the only reason I can see for ignoring the question of the existence of such objects is the normative judgement that it is our theoretical interests that are important. Without questioning that value judgement I would claim that this does nothing to belittle the metaphysical status of tables, chairs, etc.; they exist as surely (in fact surely more surely !) than the glamorous and exotic world of atoms, quarks, etc. However, for someone

who wants to adopt the view that there are tables etc. and there are the particles posited by modern physics, the question arises as to the relationship between these entities.

A colourful statement of the problem was made by Eddington,[25]

> "I have settled down to the task of writing these lectures and have drawn up my chairs to my two tables. Two tables ! Yes; there are duplicates of every object about me - two tables, two chairs, two pens...One of them has been familiar to me from earliest years. It is a commonplace object of the environment that I call the world... It has extension; it is comparatively permanent; it is coloured; above all it is substantial...Table no.2 is my scientific table... it is mostly emptiness. Sparsely scattered in that emptiness are numerous electric charges rushing about with great speed; but their combined bulk amounts to less than a billionth of the bulk of the table itself...when I lay my paper on it the little electric particles with their headlong speed keep on hitting the underside, so that the paper is maintained in shuttlecock fashion at a nearly steady level."

In her (1937) Stebbing did much to dissolve the many confusions to be found in Eddington's work. (It doesn't, for instance, make much sense to talk of the particles hitting the underside of the paper). Nevertheless Stebbing's reply does not seem entirely satisfactory, for two reasons. First, although Stebbing rightly rejects Eddington's talk of two tables, of which the scientific is the 'duplicate' of the ordinary, she talks of 'counterparts'[26] and the only hint of what a counterpart is is that the counterpart of colour is said to be its 'scientific equivalent electromagnetic wavelength'. Aside from vagueness I feel that this choice of term is likely to concede too much to the Eddington view. Second, Stebbing's view of science appears to be an instrumentalist one and therefore won't appeal to those who want to say that there are atoms etc.

Philosophers who have taken the view that the physical objects of everyday life do not exist include Feyerabend, Maxwell, and Sellars.[27] Basically the thought which seems to motivate their view may be put like this: call the language most adequate for scientific purposes L

(ignoring for the moment the fact that there is not one such language), and the ontology to which it is commited O . Call the things ranged over by the variables of L 'basic things'. Then it may be said that other things such as tables do not really exist since they are merely suitably arranged collections of basic things. Mellor calls this the 'super realist's ontological principle' and asks what reason there could be for thinking it true.[28] Mellor says that the view only seems to entail that all statements about tables etc. can be translated into statements about basic things but not conversely, and he objects that this begs the question since,[29]

> "...it is admitted that such translations are not made, and as a matter of fact cannot be made with our present mathematical expertize. If it is insisted that such translations could be carried out in principle, we observe that the possibility of translation is the principle that is supposed to establish the unique adequacy of L. If this possibility is itself only established by assuming the unique adequacy of L, the argument is viciously circular."

Whilst this objection seems to me correct I do not think that it goes far enough. Mellor goes on to say,[30]

> "The point of the claim seems to be that a statement about everyday things is either equivalent to some theorem of physical theory or is false. No doubt, if current physical theory is true, this is true. But this is just what we don't know in any sense strong enough to support the argument."

In making that statement I believe that Mellor concedes too much to the opposition: even if current theoretical physics is true I believe that all it would provide would be necessary conditions for the truth of statements about non-basic things. To explain why I believe this it will be convenient to examine an argument by Unger which is also alleged to show that tables do not exist. Unger applies the sorites paradox to tables. He writes,[31]

> "(1) There exists at least one table.
> But, from our scientific perspective, we may add this second premise:
> (2) For anything there may be, if it is a table, then it consists of many atoms, but only a finite number."

And,

> "(3) For anything there may be, if it is a table (which consists of many atoms, but a finite number), then the net removal of one atom, or only a few, in a way which is most innocuous and favorable, will not mean the difference as to whether there is a table in the situation.
>
> These three premisses, I take it, are inconsistent. The assessment of this inconsistency, I submit, leads one to reject, and to deny, the first premise, whatever one may subsequently think of the remaining two propositions."

Formally, the easiest way of dealing with this argument would seem to be that suggested by Ayer - deny that the relationship of 'being the same x with one less element than on the previous count' is transitive.[32] Although formally adequate this leaves me dissatisfied; I think a more radical approach is called for. Quine claims that the case of the table differs from that of the old paradox of the heap by "...not lending itself to any stipulation, however arbitrary, that we can formulate."[33] Because of this Quine thinks that the table case presents a threat to bivalence but I think this mention of bivalence should cause one to rethink. For the moment I wish to forget about atoms and simply consider the statement, 'Either there is a table in that room or there is not a table in that room.' Now, simply judging by our normal criteria for what counts as a table, subject to open-texture (etc), there seems to be no difficulty in taking that statement to be true. It should also be noted that if there is a table then it will consist of atoms but that is the only kind of implication that science has concerning tables. The incompatibility between our everyday and scientific beliefs only arises when the fact that science does tell us - that tables are composed of atoms - is then used as a basis for asking the question, 'is there a table or not ?' But that seems to me illegitimate and the point where pluralism comes to the rescue. Using ordinary criteria for what counts as a table there is no difficulty either in saying that 'Either a table exists in room or a table does not exist in that room' is true, or in

saying that, 'There is table in that room' must have one of the truth-values true or false. It is not surprising, however, if we cannot answer the question, 'is a table with an atom removed still a table ?' since there is no scientific theory of tables. Tables either exist or not according to the criteria appropriate to them (one point of view), and our scientific theory of the world is irrelevant except for telling us one necessary condition for the truth of a statement asserting that there is a table. The scientific point of view cannot always be used as a basis for questioning our everyday beliefs. The supposition that ultimately there is only one point of view is a supposition of absolutism but this, as we have seen, leads to paradox.

Chapter 7

Mathematics: Epistemology & Metaphysics

> "...because of the multiplicity of possible 'interpretations' or 'models', it came to be recognized that the 'nature' of mathematical objects is ultimately of secondary importance, and that it matters little, for example, whether a result is presented as a theorem of 'pure' geometry or as a theorem of algebra *via* analytical geometry." Bourbaki. 1.

Within the philosophy of mathematics there are three major kinds of questions: (i) *metaphysical* - do such things as numbers, sets, etc. exist ? Are numbers, sets, etc. abstract entities ? What is an abstract entity ? (ii) *epistemological* - is mathematical knowledge certain ? How do we acquire mathematical concepts ? (iii) *applicability* - why should mathematics prove useful in our dealings with the world, either in physics or in our everday calculations ? Roughly I shall take these three kinds of question in the order listed and corresponding to the three sections of this chapter. Only *roughly*, because in practice it is not always possible to sharply separate these issues.

The view of mathematics for which I shall be arguing is *structuralism*. This view of mathematics has been adopted both by some mathematicians (e.g. Bourbaki, Gandy[2]), and some philosophers (e.g. Benacerraf,[3] Resnik,[4]). My version of structuralism inevitably gives a different emphasis from that of others and my reasons for adopting this view are not only that I think it gives a good account of mathematics but also that it is a view consistent with the philosophical views for which I have argued in the rest of the thesis. Given the large nature of the topic I must admit that this chapter can only provide a sketch of the nature of mathematics but I am confident that structuralism provides an interesting and important account of mathematics.

(1) Structures and Structuralism

Reasons for taking a structuralist view of mathematics have been given by Benacerraf in his (1965). Benacerraf begins his paper by imagining the mathematical education of two children named Ernie and Johnny. Unlike most of us Ernie and Johnny began their mathematical education by being taught set theory. Having mastered set theory they were then told how aspects of what they learnt related to what ordinary people called 'numbers'. The point of Benacerraf's story is to present a dilemma: it appears that Ernie and Johnny have been taught all that is necessary for them both to be said to have the concept of number and yet what each of them has learnt may conflict with what the other has learnt. The source of the conflict is that for Ernie,[5]

> "...the successor under R of a number x was the set consisting of x and all the members of x, while for Johnny the successor of x was simply x , the unit set of x - the set whose only member is x. Since for each of them 1 was the unit set of the null set, their respective progressions were,
> (i) [ø], [ø,[ø]], [ø,[ø],[ø,[ø]]],...for Ernie,
> and
> (ii) [ø], [[ø]], [[[ø]]],...for Johnny.

And,[6]

> "...Ernie had been able to prove that a set had n members iff it could be put into a one-to-one correspondence with the set of numbers less than or equal to n. Johnny concurred. But they disagreed when Ernie claimed further that a set had n members iff it could be put into a one-to-one correspondence with the number n itself. For Johnny, every number is single-membered. In short, their cardinality relations were different. For Ernie, 17 had 17 members, while for Johnny it had only one."

The problem is that here are two accounts of the meanings of certain words ('number', 'one' etc.) each of which satisfies what appear to be necessary and sufficient conditions for a correct account. Benacerraf concludes that this leaves us with two alternatives,[7]

> "(A) Both are right in their contentions: each account contained conditions each of which was necessary and which were jointly sufficient. Therefore 3 = [[[ø]]], and 3 = [ø,[ø],[ø,[ø]]].

> (B) It is not the case that both accounts were correct; that is, at least one contained conditions which were not necessary and possibily failed to contain further conditions which, taken together with those remaining, would make a set of sufficient conditions."

Benacerraf comments that (A) is absurd and goes on to discuss (B). The trouble is, however, that few expect that we will discover conditions which show that one version of set theory is the correct one.[8] Relevant here is the fact that Quine does not have this expectation (relevant since Quine believes that there are numbers). Quine writes,[9]

> "...any progression will serve as a version of number so long and only so long as we stick to one and the same progression. Arithmetic is, in this sense, all there is to number: there is no saying what the numbers are; there is only arithmetic."

In a footnote Quine notes that Benacerraf's conclusions "...differ in some ways from those I shall come to." The point at issue between Quine and Benacerraf is what lesson one ought to draw from the view, shared by both, that there is no fact of the matter as to which of the distinct definitions of number offered by various set theories is the correct one. The lesson that Benacerraf draws is clear and, I believe, substantially correct,[10]

> "...any system of objects, whether sets or not, that forms a recursive progression must be adequate [to characterize the numbers].
> "... that any recursive sequence whatever would do suggests that what is important is not the individuality of each element but the structure which they jointly exhibit."
> "'Objects' do not do the job of numbers singly; the whole system performs the job or nothing does."

Benacerraf's view, then, is that numbers are not objects at all because in defining their necessary and sufficient conditions one merely characterizes an <u>abstract structure</u>,[11]

> "That a system of objects exhibits the strucure of the integers implies that the elements of that system have some properties not dependent on structure. It must be possible to individuate those objects independently of the role they play in the structure. But this is precisely what cannot be done with the numbers. To <u>be</u> the number 3 is no more and no less than to be preceded by 2, 1, and possibly 0, and to be followed by 4, 5,...<u>Any</u> object can <u>play the role</u> of 3; that is, any object can be the third element in some progression. What is peculiar to 3 is that it defines that

> role - not by being a pardigm of any object which plays it, but by representing the relation that any third member of a progression bears to the rest of the progression."

Arithmetic, therefore, is seen as the science which elaborates the abstract structure that all progressions have in common merely in virtue of being progressions. It is not a science concerned with particular objects - numbers.

I would now like to place the central idea of structuralism in a broader context by considering Resnik's advocacy of similar ideas in his (1975). In the section of his paper where Resnik advocates a structuralist view of mathematics he begins by noting the various things that could be said about a sequence of dots:

. . . . . .

Of that sequence of dots one could say such things as the following:

(1) No dot has more than one immediate successor.

(2) If one dot succeeds another then the latter does not succeed the former.

(3) There is a dot which succeeds no dot and every dot but it succeeds a dot.

(4) There is no dot between a dot and its successor.

(5) If one dot comes before a second and the second before a third then the first comes before the third.

(6) If one dot comes before another then the second does not come before the first.

(7) Given two distinct dots one comes before the other.

(8) Given any subsequence of dots there will be one in the subsequence which comes before the others.

(1) to (8) would continue to hold even if the talk of dots were to be replaced by a sequence of squares, stars, houses etc. and this is

Benacerraf's point - what is important does not concern the nature of the individual 'objects'.

One question that arises at this point is this: Benacerraf's comments about supposedly individual objects are nominalist, i.e. Benacerraf denies that there are individual mathematical 'objects' such as numbers, but Benacerraf does talk, as he more or less obliged to talk, of abstract structures - so is this view not merely Platonism at one remove so to speak, Platonism with respect to 'structures' rather than 'entities' ? There is some justice in that suggestion and, indeed, Resnik describes structuralism as being a variant of Platonism. However, the attractiveness of structuralism as an account of mathematics seems to me to stem from considerations which are far from those traditionally associated with Platonism. Structuralism, as I envisage it, may be described as a quasi-empiricist view of mathematics and any form of empiricism seems at odds with Platonism as traditionally conceived. (Alternatively, a structuralist view may be described as being quasi-formalist and formalism seems equally at odds with the Platonist picture). Essentially I believe that this is so in two ways: (i) in the epistemological approach encapsulated by structuralism; (ii) in the metaphysical presuppositions of structuralism - in particular structuralism does not presuppose that there is, or can be, a sharp distinction between the 'concrete' and the 'abstract'. These points are closely related and the explication of them will form the substance of the rest of this chapter.

As a first way of suggesting the close relationship between the two points just mentioned, and of the gulf between structuralism and Platonism, it may be instructive to consider the question of infinity.

Consider, again, the pattern of dots. Resnik says,[12] "...by imagining a finite dot pattern as extending indefinitely to the right, we obtain a sequence of dots of order type ω.." That simple statement raises some questions and contrasts between structuralism and Platonism. The contrast with Platonism can in part be drawn by pointing to the difficulties Platonism faces in providing an account of how we acquire mathematical concepts, whereas structuralism claims that we obtain mathematical concepts by abstraction from empirically based observations (the begining of the pattern of dots is empirically inscribed. Compare the argument over the issue of truth-bearers - sentence tokens are inscribed, propositiions are abstract). From a structuralist point of view, empirical inscriptions are not theoretically dispensible. (This is what gives structuralism its affinity with formalism - a point I shall return to). But are we justified in imagining the sequence of dots continued indefinitely to the right, and taking the step intuitonists would disallow, as treating infinity as completed rather than merely potential, as when Cantor decreed that the cardinal number of the natural numbers is $\aleph_0$? A-priori there is no way of telling but having taken that step one can then investigate to what extent infinite patterns emerge as a natural and coherent extension of finite patterns.

I said that the epistemological and metaphysical views of structuralism were closely related. To show why I define a structure to be the overall pattern of relationships between a set of elements. I now want to indicate reasons for thinking that neither the relations, nor the elements, with which mathematics are concerned can be classified as either purely abstract or as purely concrete. I believe that the issue between Platonism and nominalism has been widely misconceived. I examine relations first.

A discussion of the nature of 'abstract entities' faces the difficulty that no adequate characterization of what an abstract entity might be is readily available. The only general point of agreement as to what an abstract entity might be if one existed is negative - something that does not exist in space and time. It hardly seems likely that this characterization can be completely adequate however - I take it that unicorns don't exist in time or space but it is very unclear if should be considered as 'entities' - surely they simply don't exist. (Even if they were said to 'exist' this view would still still have to face the problem of 'impossible objects', e.g. the round-square). Leaving this problem to one side I want to see what one might say about relations. To simplify the discussion I want to begin with a simple case. Imagine two wooden sticks in some piece of ground in a particular relationship - imagine that they are six feet apart. This case simplifies the discussion in that in the circumstances envisaged we would have no doubt that the 'elements', the sticks, exist. Clearly the existence of the sticks is not a sufficient condition for the 'existence' of the relationship - the sticks can exist without being in that relationship. What else is necessary ? The obvious answer is the position in which the sticks are placed. 'Position' is itself a relational concept of course but note that depending upon how the two positions are described they can be characterized without reference to the relation of being six feet apart. Thus if one stick is in a position we call A, and the other stick is in a position we call B, A and B can be characterized by reference to their relationship to other features in the world beside the other stick. Suppose then, for the sake of argument, we say that the existence of the sticks in positions A and B provides both necessary and sufficient conditions for the existence of the relation R. Now, it seems to me it would be a mistake to force the question, 'is R concrete

or abstract ?' The reason I think this is that the question does not make much sense with respect to the concept of position. When the two sticks are in the ground the relationship seems physical; the 'state of affairs' which consists of the two sticks being in the ground could certainly have effects on future events, e.g. a vehicle either could or could not pass between the sticks without disturbing them. Can R be destroyed then ? Well, one could certainly destroy the sticks, but wouldn't the relation still exist between the positions A and B ? Well, can we destroy the positions A and B ? One could dig a hole ! But imagine the farm hand who has been instructed to place posts in positions A and B (to support some fencing) returning to the farmer and saying that positions A and B do not exist in that field ! This in itself suggests an interesting analogy; Platonists are inclined to say such things as 'numbers cannot be created or destroyed' but there may be reason here for saying that positions can't be created or destroyed either; but the conclusion that therefore positions are abstract entities seems unwarranted to say the least. I should not like to be misunderstood - I believe that there is only a problem for someone in the grip of a naive philosophical presupposition. That philosophical presupposition may be described by saying that every true statement about the world can be reduced to either a true statement about a physical entity or to a true statement about an abstract entity. That presupposition may be shared by Platonist and nominalist alike - the latter claiming that true relational statements reduce to statements about the elements, the former claiming that because such statements do not so reduce, relations must be abstract entities. Those not in the grip of this presupposition will, I hope, agree with me that even the simplest case of a relation that I have discussed does not fit happily into either category.

Before I press on to discuss 'elements' there is a nice point to be made about the perception of relations - that is to say that we can see them even though they are not happily thought of as 'things'. Imagine someone moving into a new house and measuring a room to make sure that some furniture will fit. Even knowing that the furniture will fit they might well want to place the furniture in the room to see what the arrangement looks like. Here the people concerned don't want to examine the pieces of furniture (the 'elements'), they want to look at the relationships between them. If it were the pieces of furniture they wanted to look at they could do that without moving the furniture into the room.

I now want to discuss the question of the nature of the 'elements' in mathematical theories by taking as a paradigm case the relationship between numerals and numbers. It is widely agreed that there must be some distinction between numerals and numbers since if there were not it would seem that '2 + 2 = 4' must be false; considered as numerals, what appears on the left hand side of the identity sign is not identical with what appears on the right hand side. However I think it is crucial to notice two things that do not follow from this fact: (i) it does not follow from this fact alone that the relationship between numeral and number is that of name to object; (ii) it does not show that numerals are theoretically dispensible in the mathematical enterprise.

To assert point (i) is to invite the question, 'how else can numeral and number be related ?' The answer, I think, is this: a numeral is a number iff the numeral is understood to mark a place in a numbering system; distinct numerals or number words 'represent' the same number if they mark the same place in a numbering system. Thus, '2', 'two', 'deux', represent the same number since they all represent the same place

in the series of natural numbers 0, 1, 2, 3, 4,... Essentially, distinct numerals represent the same number if they are functionally equivalent. On this view, then, a physical inscription of some kind is not a sufficient condition for a numeral to be a number for it also must be understood by a language community to play a particular role in a numbering system. This does not mean there may not be numbers which we can talk about without being able to give their notation. If, for instance, there is a largest prime number we do not currently know what its notation is but it seems reasonable to suppose that there is such a number iff it could be represented in one of our notations. (The possible difficulty of not having enough time or paper to write the number out can be overcome by our adoption of abbreviations).

Point (ii) above, that numerals are not dispensible, is worth some attention. Körner writes,[13]

> "The strokes on paper and the operations upon them are just as little the subject-matter of metamathematics as figures and constructions on paper are the subject-matter of Euclidian geometry. Both types of marks and constructions are diagrammatic; and diagrams however useful and practically indispensible, are 'representations' which are neither identical nor isomorphic with what they are used to represent."

There is a sense in which I concur with the sentiment expressed by the first sentence but here I wish to register at least some qualifications with respect to the sentiment expressed by the second sentence. Even to express qualifications concerning that sentiment entails that my view is a minority one within the philosophy of mathematics; Platonists, intuitionists and logicists (certainly Frege) all believe numerals to be theoretically, if not practically, dispensible. My views may best be developed by examining Körner's views in more detail.

Körner defends the thesis that logic and (pure) mathematics form two separate, a-priori sciences. Körner also defends the view that

whereas pure mathematics is a-priori, applied mathematics is empirical. Central to his account is the distinction he draws between 'exact' and 'inexact' concepts. This distinction is most easily explained by referring to the point in his book where Körner distinguishes 'Natural number' (capital 'N') from 'natural number' (lower-case 'n'). The former concept is applicable to groups of perceptual objects as when someone says 'There are two apples', but the latter concept, Körner claims, may not be applicable to groups of perceptual objects. He writes,[14]

> "...the Natural numbers..are empirical concepts, characteristics of perceptual patterns, such as groups of strokes or of temporally separated experiences. They and their relations to each other are found, not postulated.
>
> ...the Natural numbers 1,2,etc. are inexact in the sense that they admit of border-line cases, i.e. patterns to which they can with equal correctness be assigned or refused. They share this inexactnes with other empirical concepts. The natural numbers 1, 2, etc. on the other hand are exact."

Körner's distinction between exact and inexact concepts is interesting but does not, I think, provide an entirely satisfactory account of the relation between pure and applied mathematics. Körner notes, I think rightly, that mathematical concepts are frequently idealizations based on empirical experience. It seems reasonable to suppose that the concept of number evolved from repeated experiments in counting sheep etc. There may have been a time when collections of particular kinds of objects were given names before the full concept of number, naming collections as collections, emerged,[15] Körner also notes, again rightly I think, that in applying mathematical concepts to the world it is not only number concepts which have to given an empirical interpretation but also operations such as addition. Here a difference between Körner's view and mine begins to emerge however, a difference I may summarize by saying that the idealization from empirical concepts to mathematical concepts also operates in the other direction. Consider addition for example. 'Addition' as used in informal, everyday contexts has no

precise definition but I can illustrate my view as follows. If someone wanted to count a pile of wooden blocks an appropriate way of proceeding would be as follows: take a block from the pile, place it in a separate location and count '1'; take another block from the pile, place it on top of the other block and count 'two' etc. When completed the last numeral cited will tell one the number of blocks in the pile. If, however, one places one drop of mercury on top of another drop of mercury, one ends up not with two drops of mercury, but with one larger drop of mercury. Now of course we don't regard this as a 'counter-example' to '1 + 1 = 2' - this is my point. Rather, we would say that this does not constitute a proper method of addition. What, then, distinguishes a proper from an improper method of addition ? The correct answer appears to me to be that those methods of addition are correct which confirm the truth of '1 + 1 = 2' ! On this view, what accounts for the apparent certainty of elementary arithmetic as applied to the world is not only our familiarity with the basic physical way the world operates but also the interpretations we give to mathematical operations to ensure that there are no 'counterexamples'. In fact the mercury example is not all that unusual: there is non-commutativity in 'adding' to one's feet shoes + socks rather than socks + shoes; and non-associativity in the 'sums' baking powder + (milk and other ingredients) and (baking powder + milk) and other ingredients.

I am not suggesting, of course, that we are wrong in ignoring such 'counterexamples' but rather that our central concept of addition is privileged because it is the most useful. If noncommutative relations were more important in our lives than commutative ones, we would consider noncommutative 'arithmetic' normal and commutative arithmetic deviant. Against Körner I am urging that there are not two kinds of mathematical concepts, '1 + 1 = 2' as a pure, exact, certain a-priori truth and

'1 + 1 = 2' as an applied, inexact, empirical and fallible truth. I suggest that we arrived at '1 + 1 = 2' on empirical grounds (as Körner admits) and that we then 'read back' from that statement when it comes to applying that statement to the world. It seems to me that Körner is wrong when he claims that exact and inexact concepts bear no relationship to one another.

Körner, as I have already said, takes the view that numerals are in principle dispensible. It seems to me that there are objections to that view which are similar to those I urged against the view that propositions are truth-bearers. If, as Körner urges, numerals are dispensible there are difficulties in attempting to understand how intersubjective agreement in the use of mathematical concepts is possible and difficulties in understanding the relationship between numerals and numbers. On the former point, as with sentence tokens, the public nature of physical inscriptions provides at least a hopeful basis from which one can attempt to understand how individuals acquire mathematical concepts. The second point really goes to the heart of the central objection to a Platonistic or Fregean conception of mathematics, viz. simply saying that numbers are abstract entities which numeral name does not explain anything - it merely leaves us with an unexplained relationship. Frege's jibes against Mill's pebble arithmetic are wonderful rhetoric but should not be allowed to obscure the difficulties in Frege's own view.[16]

As a final comment on Körner's views that will lead into the next section, I would add that his views lead one to expect certainty in mathematical knowledge in the wrong place. I should argue that even if there is certain knowledge in mathematics, Körner's view misloctes it - I feel much more inclined to say that 'one apple + one apple = two apples' is certain than that '1 + 1 = 2' is an a-priori truth. It is,

however, to the issue of certainty that I now turn.

(2) The Loss of Certainty[17]

(a) Background: The Failure of the 'Foundations'

> "Just think: in mathematics, this paragon of reliability and truth, the very notions and inferences, as everyone learns, teaches, and uses them, lead to absurdities. And where else would reliability and truth be found if even mathematical thinking fails ?" 18

The three major philosophies of mathematics which were developed during the later part of the 19th. century and the earlier part of the 20th. century - logicism, intuitionism and formalism - all attempted, in differing ways, to provide firm, certain foundations for mathematics. By way of providing background to a discussion of the epistemological status of mathematics I will briefly discuss the reasons for that failure

Logicism, historically the first of the three major views, had its classical exposition in the work of Frege. Frege believed that the lack of a clear foundation for mathematics was a scandal which he hoped to rectify by showing how mathematics could be derived from self-evident logical principles. In carrying out this programme, however, Frege made an assumption which Russell showed led to a contradiction. Of the assumption in question, 'law V' of the Grundgesetze, Frege had said that it was a principle "..which logicians perhaps have not yet expressly enuciated" but concluded the preface to his work by saying,[19]

> "It is prima facie improbable that such a structure could be erected on a base that was uncertain or defective...As a refutation of this I can only recognize someone's actually demonstrating either that a better, or more durable edifice can be erected upon other fundamental convictions, or else that my principles lead to manifestly false conclusions. But no one will be able to do that."

Sadly for Frege he was proved wrong. Russell (and Poincaré) believed that the source of the paradox which could be derived from Frege's work

lay in the use of impredicative definitions, that is when a set M, and particular object m, are so defined that m is a member of M and the definition of m depends on M. Now the Frege-Russell definition of number went something like this:[20] Frege and Russell identified the cardinal number M of a set M with the set of sets equivalent to M. Then a finite cardinal number can be defined as a cardinal number which posssess the property P such that: (1) 0 has the property P, and (2) n + 1 has the property P whenever n has the property. In brief, a natural number is defined as a cardinal number for which mathematical induction holds. This definition of a natural number is impredicative because the property of being a natural number belongs to the totality of properties of natural numbers, which is presupposed in the definition.

To adopt logicism to the situation arising out of the paradoxes, Russell excluded impredicative definitions by his ramified theory of types. Roughly this may be described as follows: the primary objects or individuals are assigned to one type (say type 0), properties of individuals to type 1, properties of properties of individuals to type 2 etc., and no properties are admitted which do not fall into one of these logical types. (A fuller description would describe the order types for relations and classes). Then, to exclude impredicative definitions within a type, the types above type 0 are further separated into orders. Thus for type 1, properties defined without mentioning any totality belong to order 0, and properties defined using the totality of properties of a given order belong to the next higher order. However, this separation into orders makes it impossible to implement the definition of natural number which contains impredicative definitions. To escape this outcome Russell postulated his axiom of reducibility, which asserts that to any property belonging to an order above the lowest there is a

coextensive property of order 0. But what is the justification for this axiom ? In the second edition of Principia Mathematica Russell and Whitehead said,[21] "This axiom has a purely pragmatic justification: it leads to the desired results and no others. But clearly it is not the sort of axiom with which we can rest content." Weyl said that in the system of Principia Mathematica, "Mathematics is no longer founded on logic, but on a sort of logician's paradise..."[22] Aside from the ad hoc nature of the axiom of reducibility it is not clear that impredicative definitions really are always objectionable. Ramsey stated that he could see nothing wrong with referring to the man with the highest batting average in a team. (Note that that description may be all one can say about some individual if, for instance, one gave that description in response to the general question, 'how should one choose the opening batsman ?') Although today it is generally agreed that mathematics can be expressed in terms of logic plus some version of set theory this 'victory' is an empty one with respect to the initial optimistic epistemological ambitions of the original programme.

Intuitionism differed fundamentally in aim from both logicism and formalism; whereas logicists and formalists were concerned to search for firm foundations for classical mathematics, especially when prompted by the discovery of the paradoxes, intuitionists viewed the discovery of the paradoxes as indicative of something wrong with classical mathematics itself. Far, therefore, from feeling obliged to 'save' the results established by previous mathematicians, they were openly sceptical about many of those results. Intuitionism gave rise to a new kind of mathematics. I need to make it clear from the outset that I have no criticism to offer of intuitionist mathematics but I do wish to criticize intuitionism as a philosophy of mathematics. Intuitionist

mathematics is interesting and important independently of its philosophical presuppositions.

According to the intuitionists (intuitionist) mathematics is absolutely certain because it rests on direct awareness of mental constructions. Brouwer writes,[23]

> "The first act of intuitionism completely separates mathematics from mathematical language, in particular from the phenomena of language which are described by theoretical logic, and recognizes that intuitionist mathematics is an essentially languageless activity of the mind having its origin in the perception of a move of time, i.e. of the falling apart of a life moment into two distinct things, one of which gives way to the other, but is retained by memory. If the two-ity thus born is divested of all quality, there remains the empty form of the common substratum of all two-ities. It is this common substratum, this empty form, which is the basic intuition of mathematics. "

I have already argued in section 1 that there are reasons for not supposing that physical inscriptions are theoretically dispensible from the point of view of explaning how mathematical knowledge is possible. The obscurity of Brouwer's passage hardly encourages me to alter this view; what is a 'move of time' ?, what are the 'two things' refered to ?, what is the 'empty form of the common substratum of all two-ities' ? My failure to understand this passage is so great that rather than press this point I will turn to the other philosophical presupposition of intuitionism - that it provides self-evident truths.

The essential problem with 'self-evidence' is this: many things that people have claimed to be self-evident have been incompatible, therefore either people may sometimes mistakenly claim that something is self-evident when it is not, or something may be self-evident without being true. Whichever option one takes one has no way of knowing when something really is self-evident. In fact the intuitionists did not even agree amongst themselves as to what truths were self-evident.

Brouwer accepted as self-evident the claim that a square-circle cannot exist but Griss (another intuitionist) thought that we cannot have a clear idea of objects which do not exist.[24] It is true to say that my criticism of intuitionism is purely 'philosophical' but it does seem to me that the notion of the self-evident clearly is discredited, and that the intuitionist's account of how mathematics ought to be conducted is so obscure, that the rejection of their philosophy of mathematics is entirely reasonable.

Hilbert wanted to justify the results of classical mathematics (including Cantor's set theory) by finite methods. In the light of the paradoxes of set theory this involved (at least) proving that a formal system adequate for the mathematics in question was consistent. Prior to Hilbert, the method used in proving the consistency of an axiomatic theory was to give a model for that theory, i.e. to provide a system of objects, chosen from some other theory, which satisfy the axioms. That is, to each object or primitive term of the axiomatic theory, an object or term is correlated in such a way that the axioms become theorems of the other theory. If this other theory is consistent than the axiomatic theory must be consistent. This kind of proof of consistency is, however, only relative, it only shows that the one theory is consistent if the other theory is consistent. One possible thought is this: if one can provide an interpretation of a theory in terms of the physical world, one's theory must be consistent since the physical world must be consistent. However, when it comes to the idea of completed infinities it is by no means obvious that this is possible. Hilbert and Bernays argued against the possibility of doing this by considering Zeno's paradox that a runner cannot run a course in a finite time since in so doing he must pass through the infinite series of distances $\frac{1}{2} + \frac{1}{4} + \frac{1}{8} \ldots$

Instead of accepting the usual solution (observing that the infinite series of intervals required to run the successive segments converges) they said that,[25]

> "..there is..a much more radical solution of the paradox. This consists in the consideration that we are by no means obliged to believe that the mathematical space-time representation of motion is physically significant for arbitrarily small space and time intervals; but rather have every basis to suppose that that mathematical model extrapolates the facts of a certain realm of experience..."

And,

> "Closer examination then shows that an infinity is actually not given to us at all, but is first interpolated or extrapolated through an intellectual process."

Therefore if consistency is to be proved for those parts of mathematics which deal with completed infinities it must be by a different method. Hilbert's contribution was to conceive of a method of proving absolute consistency by showing that no logical contradiction can be derived from the theory in question by proving a proposition about the theory itself, i.e. specifically about all of the possible proofs of the theorems in the theory. The mathematical theory whose consistency it is hoped to prove then itself becomes the object of study and this procedure Hilbert called metamathematics or 'proof theory'.

Hilbert drew a distinction between 'real' and 'ideal' statements in classical mathematics. The real statements are those used with intuitive meaning, the ideal statements are those which are not so used. The statements which correspond to the treatment of the infinite as actual are ideal. Above Hilbert and Bernays were quoted as saying that 'infinity is..not given to us but is extrapolated' and that idea can be expressed in terms of real and ideal statements: one hopes to show how, starting from real statements, using methods which are unobjectionable (finitary), one can derive the ideal statements. As an example of this one might start with the natural numbers, then adjoin the negative numbers, then

the fractions, then the irrationals, then the imaginary numbers etc.

Taking the concept of the metamathematical point of view, together with the distinction between real and ideal statements, one can describe Hilbert's programme thus: to show that the ideal statements of classical mathematics are justified by proving metamathematically that a formal system or systems in which the ideal statements are expressed, is consistent. How did this programme fare ? In short, it failed, but its failure was perhaps more interesting and intellectually exciting than success would have been. The most interesting way in which it failed was demonstrated by Gödel. At the International Congress of Mathematics in Bologna in 1928, Hilbert stated two problems of completeness: the problem of showing that every universally valid logical schema is derivable by the rules of the predicate calculus, and the problem of showing the completeness of formalized number theory in the sense that the formal system of number theory contains no formula which, together with its negation, can be shown to be underviable in the system. Gödel proved completeness for the predicate calculus but he also proved the incompleteness of formalized number theory in the strong sense that no strictly formal system is possible in which each true number-theoretic proposition is derivable.[26]

Godel's result and others (e.g. those of Church and Skolem[27]) meant that the formalist programme could not be completed. What are the philosophical lessons of this failure for formalism ? One of the important problems for formalists is posed by Kleene,[28]

> "The delicate point in the formalist position is to explain how the non-intuistionistic classical mathematics is significant, after having intitially agreed with the intuitionists that its theorems lack a clear meaning in terms of which they are true."

The thought which underlies Kleene's point can be expressed in terms of

the distinction between real and ideal statements. The terms 'real' and 'ideal' suggest a metaphysical distinction: the real statements employ terms which denote, ideal statements employ concepts which are really nothing more than convenient fictions or idealizations. However, the distinction between 'real' and 'ideal' statements should, I suggest, merely be taken to represent an epistemological distinction. (There could be a happier choice of terms in my view). It is true that any analysis must begin somewhere, it must take some concepts for granted, and then hope to explain the concepts which seem more problematic in terms of the concepts which seem less problematic. The intuitionists were prepared to take the concept of natural number as primitive and that might seem a reasonable starting-point, but this should not be taken to imply that a natural number is not itself an idealization. For the reasons given in section 1 natural numbers are not happily thought of simply as some kind of abstract entity or thing.

At this point it is possible, and useful, to compare formalism, intuitionism and logicism with respect to the epistemological status of mathematics. Formalism failed to establish a certain foundation for mathematics because the metamathematical programme to which it gave rise could not be completed. With the important qualification concerning real and ideal statements just made above however, its general metaphysical stance seems reasonable to me. Unlike logicists and intuitionists, formalists take the inscriptions used in the activity of mathematics seriously and that fact augurs well for the possibility of explaining the public and intersubjective nature of mathematical knowledge. Epistemologically logicism failed in a different way: like the formalists logicists wanted to explain the more problematic concepts in terms of simpler concepts. One might well say, however, that as it turned out the

explanation of number in set-theoretic terms explained the conceptually simple in terms of the complex ! With that outcome its attempt to provide certain foundations tottered. With respect to the philosophical basis of intuitionism I can best explain my view by means of an analogy. My analogy concerns the arguments creationists raise against evolutionary theory. These arguments begin by pointing out, quite correctly, that there are some difficulties in the theory of evolution. (There are always difficulties with any major scientific theory). The next step, however, is to propose a radically different theory ('creationism') where there seems to be good reason for saying that the problems of the new theory make the problems of the old theory appear insignificant. It is not merely that one set of problems have been exchanged for another set of problems but that they have been exchanged for a much bigger and more intractable set of problems. Philosophically speaking, intuitionists start by raising worries someone might reasonably have - worries about non-denumerable infinities say - but philosophically the notions of self-evidence and mental constructions upon which the cure is based seem worse than the disease. What I think the intuitionists have done, contrary to their intentions, is to show that there can be interesting and significantly different kinds of mathematics.

The loss of certainty was experienced by some philosophers and mathematicians as a loss of <u>faith</u>; it would not be exaggerating, I think, to say that for some it amounted to a personal tragedy. (I think of Frege, Russell and possibly Cantor in this respect). Today we should, I believe, view the 'loss' as gain; the failure of the 'foundations' has left mathematics and metamathematics in a fascinating position and led to 'Gödel's proof' - surely one of the outstanding achievements in the history of ideas.

(b) Structuralism and Epistemology: Some Speculations

Péter recounts a rather charming and possibly instructive anecdote,[29]

> "One of our professors began his first lecture by asking one of the ladies: 'Madam, have you ever seen a point ?' This was rather unexpected, but the answer came: 'No, I have not.' 'Have you ever drawn a point ?' came the next question. 'I have' came the reply, but the lady in question quickly changed her mind and said, 'I mean I have tried but never succeeded.' (It is this answer that endeared our year to our professor for the rest of his life !)"

One can understand why the professor was pleased since an important part of acquiring mathematical knowledge consists in becoming aware that one is dealing with abstractions and it is probably impossible to progress very far beyond elementary arithmetic if one constantly demands concrete interpretations. The anecdote suggests something else however:one can imagine a mathematics teacher introducing the concept of a mathematical point by saying of a physical point, 'well, it is like that but it does not occupy space.' Now that simile raises a thought that will seem shocking to some - could mathematical knowledge ultimately depend upon such similes ? One motivating thought against my suggestion might go something like this: if we say that something is like something else in certain respects we may just be making a statement which is 'merely' relative to human experience. A long tradition in philosophy has it that (real) knowledge tells us how the world is not merely relatively to human experience but how it is absolutely. Philosophers who subscribe to that tradition would therefore find my suggestion unacceptable - mathematical knowledge is generally (unlike, say, poetry) taken seriously as knowledge and therefore, for philosophers of that persuasion, can't depend on ineliminable similes or metaphors. Note that nominalists, no less than Platonists or realists, share this tradition. Indeed, the whole universal/particular debate seems to depend upon taking this tradition seriously for if one is prepared to rest content with an analysis which does not necessarily eliminate the human

perspective there does not seem to be a problem about universals. The problem about universals comes from wanting to say what universals are non-relatively to human experience. Since throughout the thesis I have been arguing for the view that the human perspective is ineliminable I take it that it is clear that I am not engaged in special pleading on behalf of mathematics.

In section 1 I refered to my view as being quasi-empirical and I want to be a little more specific about what this means. Perhaps the first thing I ought to emphasize is that I do not believe that any mathematical statement is merely an empirical generalization from experience. Talk of empiricism in mathematics is bound to bring to mind the views of Mill. Mill does seem to have held that mathematical statements are generalizations from experience. Mill wrote,[30]

> "The science of number is...no exception to the conclusion...that the processes even of deductive sciences are altogether inductive and that their first principles are generalizations from experience."

My difference from Mill may be expressed in two ways: (i) that mathematical statements are not simply generalizations from experience but rather idealizations of experience (as with the points that don't occupy space). (ii) that it may be the case that sometimes our experience of the world is interpreted in terms of mathematics. The positivists argued that an arithmetical statement like '2 + 2 = 4' could not be empirical because there could be no evidence to disconfirm it; Lehman in his (1979), defending an empiricist view of mathematics, envisages situations where one might want to question such statements. I, however, would rather point to the cases such as the 'addition' of one drop of mercury to another where we do not obtain two drops of mercury. My point is that we have become so adept at interpreting elementary mathematical statements in terms of physical experience that we cease to notice that

interpretation is involved at all. That is the point I would urge against Mill for I believe it shows that mathematical statements aren't simply 'read off' from experience; we interpret physical statements in terms of mathematics and vice versa. (Compare Goodman's comments on the justification of deduction in terms of making mutual adjustments between rules of inference and inferences yielded.) I would, however, support Lehman in one contention he makes. To say that some statement is 'empirical' is not exactly unambiguous. I take it that most of us believe, as lay-physicists, that a perfectly normal table, in perfectly normal circumstances, won't explode. If a table does explode we would need a lot of convincing that there were not abnormal circumstances involved. Now if one is prepared to say that the belief that normal tables, in normal circumstances, don't explode is empirical (a usage that seems perfectly acceptable to me but others might count that belief as 'metaphysical') then it seems to me that '2 + 2 = 4' is empirical. Certainly we would need a lot of convincing that the addition of two 'normal' objects ('objects' unlike drops of mercury) with two other 'normal' objects does not equal four objects but that, surely, is because of familarity based on experience - ultimately there is no gainsaying what will happen.

If mathematics is empirical at all, one might expect that our degree of confidence in the truth of mathematical statements would vary. Physicists, presumably, don't have the same degree of confidence in some well-confirmed, long established experimental result as they do in the latest recherché theory about black holes. Yet one peculiarity about most philosophies of mathematics has been that they have presupposed that we are equally certain about all mathematical statements. Partially I suppose that has been because of the domineering dichotomy of the

analytic/synthetic distinction. When this is pointed out it does, I think, give one cause to pause and wonder whether most philosophy of mathematics has really been along the right lines.

If one of the problems with the claim that mathematics is empirical is that elementary arithmetical statements seem to be absolutely certain, the converse of the problem is that for relatively sophisticated mathematical statements the only potential falsifiers would seem to be logical inconsistencies. The interesting suggestion that Lakatos makes is that where a formal theory is the formalization of an informal theory, then a formal theory could be said to be 'refuted' if one of its theorems is negated by the corresponding theorem in the informal theory. Lakatos calls such an informal theorem an heuristic falsifier of the formal theory. As an example Lakatos imagines the situation where some machine churns out a formal proof in a formal set theory of a formula whose intended meaning is that there exists a non-Goldbachian even number. At the same time someone might prove informally that all even numbers are Goldbachian. If this proof can be formalized within the system of the set theory then the theory will be inconsistent. But if it cannot be thus formalized, the set theory will not have been shown to be inconsistent but only a false theory of arithmetic (whilst still being a possibly true theory of some mathematical theory that is not isomorphic to arithmetic). Then the informally proved Goldbach theorem may be called an heuristic falsifier of the formal set theory. Similarly, a demonstration of the so-called 'ω-inconsistency' of a system of arithmetic (that is, a demonstration of $\exists x P(x)$ & $-P(0), -P(1)\ldots$, for some predicate 'P') would be a heuristic falsification of it. Lakatos goes on to note that a heuristic falsifier is only a falsifier in a Pickwickian sense, it only suggests a falsification. But this does not sharply separate mathematics from physics. Lakatos claims, plausibly to

my mind, that "One can show that most classical refutations in the history of science and mathematics..are heuristic falsifications."[31]

I should like to emphasize two points implicit in the above. The first point is that the discovery and acceptance of mathematical statements is a tentative process. Even if it now appears that some mathematical statements are very unlikely to be rejected or refuted that is partially because they have stood the test of time and this does not distinguish them from other kinds of statements. Gandy expresses the tentative process of mathematical discovery very nicely,[32]

> "...much of a worthwhile mathematical journey will be spent in a fog of incomplete analogies, half-worked-out examples, and hazy intuitions. By trial and error, by exploring blind alleys, by hard and imaginative thought, the mathematician finally emerges on the sunlit summit. The clouds disperse and a direct route is seen and retraced. Traditionally - though perhaps wrongly - the published account describes only this direct route; the gropings and false trails are ignored."

The second point is that mathematical statements are not tested in isolation but holistically. As early as 1928 Hilbert wrote that,[33] "It is by no means reasonable to set up in general the requirement that each separate formula should be interpretable by itself..." In theoretical physics "only certain combinations and consequences of the physical laws can be checked experimentally - likewise in my proof theory only the real statements are immediately capable of a verification." I should only add that even the so-called 'real' statements do not confront the world in isolation.

Lakatos quotes an impressive array of mathematicians and philosophers who have reached the conclusion that the epistemological status of mathematics is quasi-empirical in the sense that mathematics is fallible and ultimately dependent upon experience. The conclusion gains credibility from the fact that it is a conclusion very many have been very reluctant

to reach. I won't reproduce the quotations but the list includes, Frankenel, Curry, Carnap, Church, Gödel, Rosser, Weyl, Von Neumann, Bernays, Kalamor and Russell. The conviction that mathematics provides absolutely certain knowledge has taken a long time to overcome. Partially that was because dogmatism with respect to other areas of inquiry had to be overcome first - mathematics seemed to provide the last bastion of certainty. Partially it was also because it seemed that mathematics must have a very different relationship to reality because of the theories that were held about non-mathematical knowledge. That is to say that so long as the analytic/synthetic distinction was unquestioned; so long as the 'theory-laden' nature of perception had not been realized; and so long as the relation between language and reality has an 'atomistic' rather than an 'holistic' model, it was inevitable that mathematics must appear to have a very different epistemological status and a very different relation to the world from other areas of inquiry. With the withering of the shibboleths of traditional epistemology the vista of a unified account of knowledge has opened up and I hope it is clear that the conclusions toward which I am moving do not justify a sharp distinction between mathematical and other kinds of knowledge.

My final speculative suggestion is that if we concentrated upon the question of the acquisition of mathematical concepts and their practical application, we may be enabled to make faster progress in the philosophy of mathematics than has been evident to date.

(3) Mathematics and the World

Why mathematics should prove useful in our dealings with the world, whether it be at the prosaic level of counting objects, or at the highest level of physical theory, is a fascinating question. To explore

this issue in the light of the preceding sections, it will be convenient for me to compare and contrast my views with those expressed by Field in his book Science Without Numbers (1980). It is a particularly appropriate work for my purposes because on the one hand I find Field's answer to the question of why mathematics is useful interesting and suggestive, but on the other hand I do not share his overall philosophic ambition. I will begin by describing what Field attempts to do, proceed to the important points of agreement, and conclude by discussing where we disagree.

The position for which Field argues is nominalism - the view that there are no abstract entities. To argue for this view Field proposes to show that mathematics needed for application to the physical world need not include anything which even prima facie contains references to (or quantifies over) abstract entities like numbers, functions or sets. To the major part of mathematics which does contain references to abstract entities, Field adopts a fictionalist attitude - that is he does not regard it as being true. He proposes to justify this fictionalism by showing that there is a reformulation of science that does not require the use of any mathematics that refers to or quantifies over abstract entities. Clearly this view commits Field to a large programme. He does not claim to have completed this programme in this work but he does claim to have gone as far as to provide a sketch of how a nominalist version of the Newtonian theory of gravitation could be given.

How does Field propose to implement this programme ? The basic claim that Field makes is as follows: suppose N is a nominalistic formulation of a scientific theory (i.e. a formulation that does not quantify over abstract entities) and S is a mathematical theory, then

one couldn't get any nominalistically statable assertions from N + S that one couldn't get from N alone. The mathematical theory however is useful in making inferences much easier and quicker to draw than they would be if one were to avoid the use of the mathematical theory. One caveat must be entered however. Since N is a nominalistic theory it may say things which rule out the existence of abstract entities and this could make N + S inconsistent. To deal with this Field first introduces a one place predicate M(x) meaning 'x is a mathematical entity'; second, for any nominalistically statable assertion A, A* is the assertion that results by restricting each quantifier of A with the formula 'not $M(x_i)$'(for the appropriate variable 'x_i'); third, for any nominalistically stated body of assertions N, let N* consist of all assertions A* for A in N. N* is then an agnostic version of N. If N says that all objects obey Newton's laws, then N* says that all non-mathematical objects obey Newton's laws but allows for the possibility that there are mathematical objects which do not.

Field says that the key to using a mathematical system S as an aid to drawing conclusions from a nominalistic system N lies in proving in N*+ S the equivalence of a statement in N* alone with some other statement (which he calls an abstract counterpart of the statement in N*) which quantifies over abstract entities. Then, to determine the validity of an inference in N* (or of an inference in N) one need not proceed directly; instead one can 'ascend' from one or more statements in N* to abstract counterparts of them, then use S to prove from these abstract counterparts an abstract counterpart of some other statement in N*, and 'descend' back to that statement to that statement in N*. I will outline an illustration Field gives of this procedure.

The illustration is where the mathematical theory to be applied is the arithmetic of natural numbers plus some set theory. Suppose N is a theory which contains the identity symbol and the usual axioms of identity but does not contain any terms or quantifiers for abstract entities. N does not contain singular terms like '87'. It is convenient to suppose that N contains quantifiers like '$\exists_{87}$' (there are exactly 87) and '$\exists_{\geq 87}$' (there are at least 87). Tiresome though it would be we do know how we could dispense with these quantifiers in favour of the standard universal or existential. We are now invited to consider the following argument in N:

(1) there are exactly twenty-one aardvarks,

(2) on each aardvark there are exactly three bugs,

(3) each bug is on exactly one aardvark; so,

(4) there are exactly sixty-three bugs.

To discover whether this is a valid argument in N would take a great deal of work but if one has a mathematical system S, which includes the arithmetic of the natural numbers plus some set theory, things are simplified considerably. As an abstract counterpart of the first claim we have:

(1') the cardinality of the set of aardvarks is 21.

(1') is an abstract counterpart of (1) because the equivalence of (1) and (1') is provable in N + S. Abstract counterparts of the other premises and of the conclusion are:

(2') all sets in the range of the function whose domain is the set of aardvarks, and which assigns to each entity in its domain the set of bugs on that entity have cardinality 3.

(3') the function mentioned in (2') is 1-1 and its range forms a partition of the set of all bugs.

(4') the cardinality of the set of all bugs is 63.

Now in S one can prove:

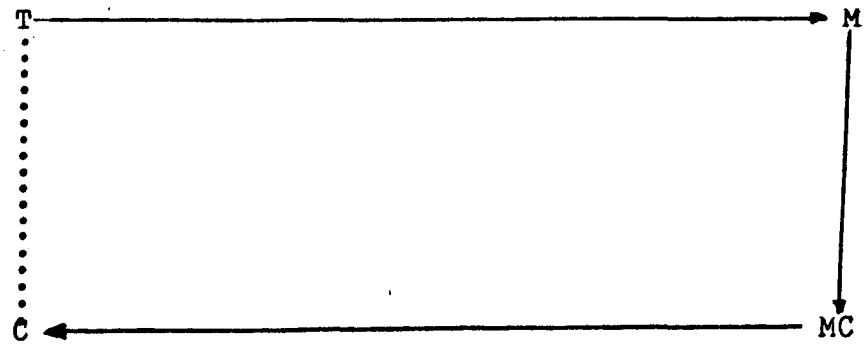
(a) if all members of a partition of a set X have cardinality α, and the cardinality of the set of members of the partition is β, then the cardinality of X is $\alpha.\beta$.

(b) the range and domain of a 1-1 function have the same cardinality.

(c) $3.21 = 63$.

Since (1'), (2') and (3') in conjunction with (a)-(c) entail (4'); and since (1') - (4') are abstract counterparts of (1) - (4), (4) has been proved from (1) - (3) in N + S.

Field's approach to the usefulness of mathematics is, I think, interesting and suggestive. One might 'picture' the view as follows:



Where:

'T' = some informal fragment of everyday discourse; a scientific theory or a mathematical theory.

'M' = some mathematical theory (formal or informal).

'MC' = the conclusion reached in some mathematical theory.

'C' the conclusion reached in the original theory.

The horizontal arrows represent mappings from T into M, or from MC into C.

This picture is no doubt oversimplified but one can view a great deal of mathematical activity in its terms. Sometimes mathematicians define and study the structures represented by M with the intention of

dealing with T, and sometimes not. The theory of real numbers and the theory of differentiation of real numbers was developed in order to deal with physical space and time and various theories in which space and/or time play a role such as Newtonian mechanics. On the other hand it is sometimes discovered that a mathematical theory can be an interpretation for some other theory as, for example, when Shannon showed that the states, switches and relays in an electric circuit is an interpretation of abstract Boolean algebra.[34] As an example of the interpretation of one mathematical theory in terms of another one might mention analytic geometry, where Descartes showed how to express Euclidian geometry in algebraic terms. This is a precise example of the view Field suggests - having expressed geometric figures in algebraic terms, proof can be carried out more easily by the use of algebra before being reinterpreted geometrically.

I hope it may be clear that this view of the application of mathematics fits nicely with structuralism. One example of a structure described by Gandy has two elements 0 and 1, and two operations called 'plus' (+) and 'times' (.). The structure is defined completely by the following equations:

$$0 + 0 = 0,\ 0 + 1 = 1,\ 1 + 0 = 1,\ 1 + 1 = 0.$$

$$0 \cdot 0 = 0,\ 0 \cdot 1 = 0,\ 1 \cdot 0 = 0,\ 1 \cdot 1 = 1.$$

If one reads '0' as even and '1' as odd, then the equations above give the laws of the odd and the even. The equations also give the rules of binary arithmetic as used in computing. Further, the structure is the simplest example of a finite number field investigated by Galois, and plays a fundamental role in the theory of numbers and algebraic geometry. This brings me back to the point stressed in section 1, that the elements and relations that the mathematician is interested in may be

studied independently of their concrete interpretations.

In the 'picture' I represented the line from T to C as broken to indicate that I am not commited to the thesis Field is concerned to argue - that where T is a nominalistic formulation of a scientific theory there is a direct route to C which does not require one to quantify over abstract entities. The importance of Field's work, I think, resides in its suggestiveness with respect to the usefulness of mathematics rather than in his nominalist ambitions. The argument Field gives for the usefulness of mathematics could, I believe, profitably be developed further to the benefit of both the philosophy of mathematics and the philosophy of science. Given my views, however, it hardly seems likely that Field's desire for a pure nominalism can be satisfied; whatever else physics and mathematics may be able to do without it hardly seems likely that they can do without relations, and relations, I have suggested, are not happily thought of as either purely abstract nor purely concrete. The difference between Field's view and mine ostensibly concerns mathematics since that is the topic under discussion but the source of those differences lie much deeper - they lie deep in the conflict between my espousal of pragmatism and Field's acceptance, albeit in a modern guise, of absolutism. To bring these differences to the fore it will be useful to contrast Field's views with Quine's, and both of their views with mine.

Quine's position on mathematics and abstract entities has been, for most of his career at any rate, clear:[35] scientists quantify over abstract entities, there is no known way of eliminating those references to abstract entities, therefore abstract entities exist. Field accepts that argument in the sense that he would accept the conclusion if he thought that he could not show that the reference to abstract entities

can be eliminated. Now although it is nice to have some concord in philosophy I can't, as I said in chapter 3, entirely accept this argument. From my point of view both Quine and Field have effectively started from the wrong question; they both started from worrying about whether or not there are abstract entities rather than starting, as I would urge, from trying to explain how mathematical knowledge is possible. (Note that in effect this is what Benacerraf did and, it seems to me, the outcome was very suggestive). So reasonable does the Quinian view appear that it comes as something of a shock to realize that although we are confidently being told that there are abstract entities we have not been given an account of what they are. As I lamented earlier, the only general characterization of what abstract entities are is negative - that they are not observable etc. From my point of view, an even more important lacuna in Quine's position is that he nowhere explains why reference to abstract entities should be useful. Effectively I have been arguing that 'abstract entities' are neither purely 'abstract' nor purely 'concrete' - we can't, for instance, simply say that numerals <u>are</u> numbers but the other conditions that have to obtain to make a numeral a number (essentially that a numeral must be understood by a language community to represent a place in a numbering system) can't themselves be neatly characterized as either abstract or concrete. Given the apparent respectability of the Quinian view it may be said that I am fudging the issue, that I am not taking my 'ontological commitments' seriously, that I am engaged in double-think. In reply I would say this: if I am fudging then it is only because this is a place where fudging is quite properly called for. To justify this claim I would make the following comparison: in the respects relevant to these concerns numbers (for instance) are rather like possibilities. Given the analysis in chapter 4, and faced with the question, 'do you

think that there are possibilities ?' I should have to give a qualified answer. On the one hand there are many sentences of the form, 'There is a possibility that _____.' where, in my view, one could fill in the blank and obtain a sentence with truth-conditions as clear as we generally have any hope of obtaining. That would amount to the case for saying that I do believe that there are possibilities. On the other hand, a philosopher asking the question, 'do you think that there are possibilities ?' might mean, 'do you think that possibilities are <u>things</u> - do you think that there are possible worlds ?', to which my answer would be 'no'. Asked whether there are numbers my answers would be structurally similar: 'yes, I believe that there are numbers if by 'numbers' you mean that there are numerals which are understood by a language community to mark a place in a numbering system'; or, 'no, I don't believe that there are numbers if by 'numbers' you mean abstract entities which exist outside of time and space and independently of us.' In the light of those remarks I hope it will be clear what I mean when I say that my view is <u>resolutely</u> <u>non-reductionist</u>.

I should like to draw another parallel between the analysis of modality and the analysis of mathematics. It seems to me that many analyses of modality have erred because philosophers have approached the issue with ontological questions foremost in their minds. That entailed that either one had the option of 'reducing' modal statements to non-modal statements (thereby supposedly showing that they weren't really committed to dubious kinds of things), or of biting the bullet and saying that they were committed to a certain kind of entity - a possible world - and then worrying like mad about what it might be. I suggested, however, that if one approaches the analysis of modality with more appropriate questions in mind, questions about how we understand

particular kinds of modal statements, what kinds of evidence we accept as supporting kinds of modal statement, both of the previous options would seem bizarre. Much the same can, I believe, be said of the philosophy of mathematics. To approach the philosophy of mathematics with ontological questions at the forefront is to start from the wrong place. Once one approaches the philosophy of mathematics with, roughly speaking, epistemological questions to the fore the whole picture changes and becomes far less mystifying. I think it is significant that it has been philosophers and not mathematicians who have worried about the ontological status of abstract entities. In my view we do well if we understood - epistemologically understood - how a given area of inquiry works and let the ontological questions turn out as they will.

Chapter 8

Philosophy and the Norms of Inquiry

My aims in this final chapter are ambitious, they are: (i) to criticize the three dominant conceptions of morality to be found in analytic philosophy; (ii) to attack the basis of the fact/value distinction (to be more precise I am going to criticize the usual model of the relationship between facts and values supposed to obtain in moral thought and the humanities); (iii) to sketch an alternative conception of morality and inquiry. My aims are necessarily ambitious since I believe that these tasks are intimately connected.

It is a simplification, but not, I think, a gross over-simplification to say that there have been three dominant conceptions of morality in the recent analytic tradition: theories of personal preference, Kantian theories and utilitarian theories. Section (1) begins my attack on personal preference theories and the basis for the fact/value distinction; section (2) continues the attack on personal preference theories, begins to sketch an alternative and attacks Kantian and utilitarian theories; section (3) draws these themes together and presents an alternative conception of the nature of inquiry to that dominant within recent philosophy and partially embodied in 'common-sense' views. I conclude with some reflections on philosophy and pragmatism.

(1) Facts and Values

In his article "Value and Valuation", Frankena makes the following statement,[1]

> "Philosophers from the time of Plato had discussed a variety of questions under such headings as the good, the end, the right, obligation, virtue, moral judgement, aesthetic judgement, the

> beautiful, truth, and validity. In the 19th. century the conception was born - or reborn, because it is essentially to be found in Plato - that all these questions belong to the same family, since they are all concerned with value or what ought to be, not with fact or what is, was, or will be."

To modern eyes this statement appears inconsistent since truth has been placed in the list of value concepts when we think of truth as essentially concerned with what is, not what ought to be. But there is a difficulty when one probes the apparently sharp contrast between what is, was, or will be, on the one hand, with what ought to be on the other. For many of us there are, no doubt, many things that we think ought to be that are not - a fact which invites the idea that the concept of what is is mutally exclusive of what ought to be. For most of us however that is not so - it is surely an unhappy person for whom everything that ought to be includes nothing that is.

Endemic in modern philosophy and contemporary (Western) culture has been a view of morality which claims that moral statements can have no justification beyond the personal preference of the individual who makes the statement. Underlying that view has been the distinction between facts and values. Whilst I do not want to deny that there is something worth calling 'facts', and that there is something worth calling 'values', I do want to deny either the explicit or the implicit theories which have been taken to justify personal preference theories (and the model of the relation between facts and values upon which they depend) is tenable for most areas of inquiry.

When I speak of personal preference theories I have in mind such theories as the following: subjectivism (e.g. Hume); emotivism (e.g. Stevenson, Ayer [2]); existentialism (e.g. Sartre[3]). For my purposes the differences between these theories are less important than what they have in common. The classical source for personal preference theories

in modern philosophy is Hume - arguably Hume had a direct influence on the logical positivists and the development of emotivism, and an indirect influence (via. Nietzsche) on Sartre and existentialism. For these reasons, and because it will facilitate the presentation of my alternative view, I will attack Hume's version of personal preference theory.

A famous passage in the Treatise reads as follows,[4]

> "Take any action allowed to be vicious: wilful murder, for instance. Examine it in all lights, and see if you can find that matter of fact, or real existence, which you call vice. In whichever way you take it, you find only certain passions, motives, volitions and thoughts. There is no other matter of fact in the case. The vice entirely escapes you, as long as you consider the object. You never can find it, till you turn your reflection into your own breast, and find a sentiment of disapprobation, which arises in you, towards this action. Here is a matter of fact; but it is the object of feeling not of reason. It lies in yourself, not in the object. So that when you pronounce any action or character to be vicious, you mean nothing, but that from the constitution of your nature you have a feeling or sentiment of blame from the contemplation of it. Vice and virtue, therefore, may be compared to sounds, colours, heat and cold, which, according to modern philosophy, are not qualities in objects, but perceptions in the mind..."

I believe that there are essentially two things wrong with this view: (i) the epistemology; (ii) the theory of emotion upon which the theory depends. To enable me to make my criticisms it will be useful to recall another famous passage,[5]

> "A passion is an original existence, or, if you will, modification of existence, and contains not any representative quality, which renders it a copy of any other existence or modification. When I am angry, I am actually possessed with the passion, and in that emotion have no more reference to any other object, than when I am thirsty, or sick, or more than five foot high. It is impossible, therefore, that this passion can be opposed by, or be contradictory to truth and reason; since this contradiction consists in the disagreement of ideas, considered as copies, with those objects, which they represent."

At this point both in this thesis and in contemporary philosophy generally I need not labour the epistemological difficulties: the idea of 'copying' and the difficulties of finding any 'real existence' even

when there is a true factual statement (upon what 'real existence' does the statement, 'The earth is not flat' depend ?); and the dubious claim that we can make a sharp distinction between properties which depend upon us (colours, sounds) and those that do not.

The second aspect of Hume's theory that I wish to criticize, his theory of emotions, stands in a more interesting relationship to contemporary philosophy since whilst this theory has been criticized (see, for example, Pitcher [6]) I have not seen those criticisms embodied in an attack on Hume's moral subjectivism. What is essentially wrong with Hume's theory of emotions is that he treats them as though they were merely bodily sensations when emotion is a much more complex matter. It seems just false that emotions do not have reference to other 'objects'; emotions have 'intentional objects', one is angry at someone or some state of affairs, in love with some person, and so on. Hume anticipates this objection and, in effect, claims that it is not the emotion which 'refers' but the accompanying belief. Hume makes this point when he claims that emotions cannot be said to be reasonable or unreasonable,[7]

> "...it is only in two senses that any affection can be called unreasonable. First, when a passion such as hope or fear, grief or joy, despair or security, is founded on the supposition of the existence of objects which do not really exist. Secondly, when in exerting any passion in action, we choose means insufficient for the designed end, and deceive ourselves in our judgements of causes and effects."

Not all cases that fall into Hume's first category can be said to be cases of unreasonable emotion. Suppose someone has read in a local newspaper that a lion has escaped from the zoo and that when they are out walking they hear what they think is the roar of a lion, then their emotion of fear would be reasonable whether or not there was a lion. The more general problem is whether one can say something as follows:

emotions can only be said to be reasonable or unreasonable to the extent that the accompanying belief is either true or reasonable; or false or unreasonable, respectively. Even this general model does not work however: suppose that some minor incident happens, e.g. someone breaks one of my coffee cups, and I fly into a fit of rage; here it would seem that my belief about what has happened may be true but my emotion unreasonable. It is true, I think, that in those circumstances one might be inclined to think that the breakage of the cup wasn't the real or the sole cause of the emotional reaction (one would be tempted to say something like 'He got out of bed the wrong side this morning') but I do not think that is of much comfort for Hume's view since it is the discrepency between what happened and the unreasonableness of the emotion that would justify the inference. More generally, what a Humean view of emotion requires is that one make a sharp distinction between the cognitive element of someone's reaction (the belief) and the non-cognitive, but that distinction cannot always be drawn. We cannot always distinguish emotions purely by reference to their phenomenal properties - the very difference between annoyance and indignation depends upon what the individual believes about the situation. Moreover, contrary to what Hume claims, it seems that some emotions must have an intentional object - one cannot be in a state of grief without grieving over something.

Consider the following three distinctions:

belief	emotion
applicability of predicates of rational appraisal (e.g. reasonable, unreasonable).	inapplicability of predicates of rational appraisal.
cognition	non-cognition

Hume's view is that the three distinctions in the list on the left-hand side are coextensive and mutually exclusive of the three distinctions in the list on the right-hand side. So far I have provided some reason for thinking that the first two distinctions are not mutually exclusive but what of the third distinction ? Can emotion play a cognitive role ?

Consider the sentence 'All men are mortal' uttered in two different contexts. On the one occasion the person uttering the sentence is a logic lecturer taking a logic class, uttering the sentence as he writes it on the blackboard, intending to complete the famous syllogism. On the other occasion the sentence is uttered slowly, almost inaudibly, by a patient in a doctor's surgery who has just been told that they only have six months to live. To make the case vivid suppose that it is the same person involved in both cases - the lecturer went straight from the logic class to the doctor's surgery. Now the question is: do the logic teacher and the patient know the same thing ? One response to this is as follows: 'well, of course, the patient knows something that the logic teacher doesn't - that he is going to die in six months time. However, in the sense that the logic teacher and the patient know 'All men are mortal' is a true sentence they do know exactly the same thing. It would, after all, be a pretty poor logic teacher who did not know that he was included in the class of all men.' Against this I think one can say the following: in the situation as described it would be entirely natural to say of the patient that as he uttered the words in the surgery the words took on a new meaning, a new significance for him. Moreover, it is the knowledge that he is going to die in six months time that put the patient in a new relationship to the sentence, 'All men are mortal'. I am inclined to think that it is, admittedly, a moot point as to whether they know something different but it is on a

point as moot as this that the whole idea that emotions are non-cognitive depends. I won't claim therefore that this distinction amounts to a difference in ways of knowing - call the difference between what is involved in the two cases the difference between the evaluative significance and the non-evaluative significance of a statement.

One response to the above might be to question whether the distinction between the evaluative and the non-evaluative significance of a statement could have any general application; if it can't then its possible significance for moral philosophy would be in severe doubt. It seems to me that the distinction does have general significance. For example, it was only recently in Britain the the wearing of seat-belts in cars was made compulsory. Now one liberal argument against the wearing of seat belts being made compulsory might have gone as follows: 'Accepting that the wearing of seat-belts saves lives we ought to have a publicity campaign to make sure that every driver is aware of the facts. Once they are aware of the facts however, it ought to be up to them to decide what to do.' One response to this might be: 'in the sense in which people know that wearing seat-belts saves lives, the general theoretical case, everyone already knows that. What is more relevant is that when people get into a car they do not know (and can have no way of knowing) whether an accident will happen to them. In this sense, most people unconsciously believe, 'It won't happen to me.' We can't, and it would be undesirable to try to convince them that it will happen to them (inculcating a fear of life) but we can give them an extra incentive to wear seat-belts by making it a legal requirement.' The second argument is intended to suggest a parallel between the way in which the logic teacher knows that all men are mortal and how drivers know that they might be involved in an accident, and the importance of the

fact that there isn't generally a sense which parallels the way that the patient knows that all men are mortal and knowing that it really could be oneself involved in an accident. If this point is correct then the distinction between the evaluative and the non-evaluative significance of statements is of practical importance and morality, I take it, is a practical business.

I shall return to the distinction between evaluative and non-evaluative significance in section (3) but for now I should like to sow the seeds of an idea that I hope may germinate before the end of the chapter - that the following parallel between Quine's attack on the analytic/synthetic distinction and my view of the cognitive role that emotion may play is reasonable:

"Science has its double dependence upon language and experience; but this duality is not significantly traceable into the statements of science taken one by one."[8]

Moral statements have their dual dependence upon emotion and belief; but this duality is not significantly traceable into the statements of morality taken one by one.

(2) Towards a Pragmatic Conception of Morality

Hume's personal preference theory was expressed in another famous passage in the Treatise,[9]

> "In every system of morality, which I have hitherto met with, I have always remarked, that the author proceeds for some time in the ordinary way of reasoning, and establishes the being of a God, or makes observations concerning human affairs; when of a sudden I am surprised to find that instead of the usual copulations, is, and is not, I meet with no proposition that is not connected with an ought or an ought not. This change is imperceptible; but is, however, of the last consequence. For this ought, or ought not, expresses some new relation or affirmation, it is necessary that it should be observed and explained; and at the same time that a reason should be given, for what seems altogether inconeivable, how

> this new relation can be a deduction from others, which are entirely different from it."

Attempts have been made to meet Hume's argument directly (e.g. by Foot and Searle[10]) but the line of resistence I wish to explore starts from a view MacIntyre has recently been defending.[11] This point of view starts by noting that certain concepts - functional concepts - do allow one to derive evaluative conclusions from factual premises. For instance, from 'this watch keeps time very accurately' and 'this watch is very convenient to carry around and is easily read' it follows that this is a good watch. What allows this derivation is that we have some idea of the purpose or function of a watch against which, given factual premises, we can deduce that the watch is good for serving those purposes. The question is whether any reasonable parallel can be made for the view that the concept of man is a functional concept. That is a large question but to begin to ask it suppose that one asks someone to make a list of the qualities they admire in a person. Suppose that they come up with the following list (they admire people who are): honest, kind, sympathetic, unassuming, unpretentious. One point about this list is that it is simultaneously evaluative and a list of qualities for which we know, roughly at least, what kinds of evidence, what kinds of behaviour, would lead us to attribute or refuse to attribute these qualities to some individual. But are any of those moral qualities ? Is there a sharp distinction between moral and non-moral qualities ? I'm afraid that to answer that question I must raise the general question of what morality is. I think that it is important to distinguish between two ways of trying to define morality:[12]

Autonomous	Contextual
Moral beliefs or moral values represent a special class of beliefs or values to be con-	Morality or moral values represent the ordering of an individual's or a societies priorities. In this

trasted for instance, with scientific beliefs or aesthetic values. In this sense someone may be said to place more importance on aesthetic values than moral values.

sense, if an individual or society placed great emphasis on aesthetic values that would be a part of the individual's or that societie's morality.

As things stand, of course, what I have placed beneath the 'autonomous' label does not define the moral at all, for the question arises as to what characteristics moral beliefs or moral values share that scientific beliefs or aesthetic values do not. I think it is very much open to doubt whether that question can be answered if one is attempting not merely to define one's own morality but attempting to define a concept of morality neutral between all individuals or societies. Certainly I see little promise in the attempt to give such a definition by invoking grammatical criteria (e.g. that moral statements take the form of universal prescriptions). What I have written under the 'contextual' label might be said to suffer a similar problem for one may ask, 'place greater emphasis on aesthetic values in preference to what ?' The answer moral values would obviously take us back to square one. I think, however, that things are a little more promising in this case, It is true that not every ordering of priorities that an individual or a society makes can be described as moral: if someone decides to read a book rather than go to the cinema that is an ordering of priorities but hardly a moral choice. Perhaps one can say something as follows: with respect to an individual their morality consists in their overall ordering of priorities that are important for their overall aims, projects, and character; with respect to a society, its morality consists in its overall ordering of priorities partially embodied in

its legal system and partially embodied in its political actions and policies. Even these qualifications are likely to leave many ambiguities but since, for reasons I shall come to, I do not believe that the distinction between the moral and the non-moral is necessarily always very sharp or very clear this may suffice for my purposes.

As a partial specification of how we talk of moral beliefs and values I take it that both the autonomous and the contextual descriptions capture something but I want to suggest that things look very different according to whether one approaches normative ethics via the autonomous or the contextual conception. The large difference between these approaches (as the labels are intended to suggest) is that an approach to normative ethics via the autonomous conception implies an approach that operates at a high level of abstraction and is compatible with the search for very general moral principles and a moral decision procedure. The approach to normative ethics via the contextual conception is less likely to yield general principles since if one considers how an individual orders his priorities in the course of their life we are likely to find that those priorities are inextricably bound up with specific historical circumstances, the nature of their personality, and their projects and commitments. Now an adequate decision procedure, if one could be found, would be very desirable but it seems to me that not only are we not liable ever to find one but also that the attempt to find one has damaged moral philosophy by encouraging gross over-simplification. To explicate and defend this claim I will first examine Kantian approaches to morality.

As Williams notes, some of the elements in a Kantian outlook are these:[13] that the moral point of view is essentially different from a non-moral, self-interested point of view; that the moral point of view is specially characterized by its impartiality and its particular

relations to particular persons; that moral thought requires abstraction from particular characteristics of the parties, including the agent, except insofar as these can be treated as universal features of any morally relevant situation; and that the motivations of a moral agent involve an application of moral principle and thus are different in kind from the sorts of motivations that a moral agent might have for treating some particular persons differently because he or she happened to have particular interests towards them.

The abstraction of moral motivations and the moral point of view from the level of particular relations to particular persons, obtains even when the moral point of view is explained in terms of the self-interest of contracting parties under conditions of ignorance as it is in the work of Rawls, since the contracting parties are entirely abstract persons making this choice in ignorance of their own particular characters, interests and so forth. This perspective is also explicitly embodied in Singer's book The Expanding Circle which precisely identifies (as have theorists of 'moral growth') the ability to abstract from one's own particular circumstances and character as constituting the essence of morality.

Kant's own moral philosophy drew a sharp distinction between acting in conformity with duty and acting from the motive of duty which seems to me as unattractive as it is difficult to apply,[14]

> "To help others where one can is a duty, and besides this there are many spirits of so sympathetic a temper that, without any further motive of vanity or self-interest, they find an inner pleasure in spreading happiness around them and can take delight in the contentment of others as their own work. Yet I maintain that in such a case an action of this kind, however right and amiable it may be, has still no genuinely moral worth."

Susan Wolf suggests, to my mind rightly, that such phrases as 'I cannot tell a lie' and 'He couldn't hurt a fly' are not exemptions from

praiseworthiness but testimonies to it.[15] She makes the point in the course of defending a compatibilist view of the relation between freedom and causality but the point is equally telling, it seems to me, against Kant's view. It seems to me that we should (both normatively and descriptively) not withdraw moral praise on the grounds that someone acted from a 'sympathetic temper'.

Another aspect of the demand for impartiality is raised by Fried,[16]

> "...surely it would be absurd to insist that if a man could, at no risk or cost to himself, save one or two persons in equal peril, and one of those in peril was, say, his wife, he must treat both equally, perhaps by flipping a coin. One answer is that where the potential rescuer occupies no office such as that of captain of a ship, public health offical or the like, the occurence of the accident may itself stand as a sufficient randomizing event to meet the dictates of fairness, so he may prefer his friend or loved one. Where the rescuer does occupy an official position, the argument that he must overlook personal ties is not unacceptable."

One question this view immediately raises is what the person who does occupy an official point of view is supposed to do - do they flip a coin ? The more general point is that there does seem a reasonable point of view from which someone could say that they rescued one person rather than another because that person was their wife. Moreover, that point of view does seem to be deeply entrenched in our present moralities since most people believe that they have obligations and responsibilities to family and friends that they do not have to others. Partially some of those commitments can be accomodated within a Kantian perspective; but only partially. The Kantian view would seem unable to accomodate the claim that it would be morally justifiable to save one's wife because she was one's wife. Note the difference here between the motivating thought 'It was my wife' and the different motivating thought which might be available to a Kantian, 'It was my wife and in situations of this kind it is permissible to save one's wife.' In discussing this case Williams argues that somewhere deep attachments to other persons

will express themselves in the world in ways that cannot at the same time embody the impartial view. Moreover, unless there are such commitments there would not be enough conviction in one's life to sustain allegiance to life itself. He concludes,[17]

> "It follows that moral philosophy's habit, particularly in its Kantian forms, of treating persons in abstraction from character is not so much a legitimate device for dealing with one aspect of thought, but is rather a misrepresentation, since it leaves out what both limits and helps define that aspect of thought."

The charge that moral philosophy has oversimplified the issues by making the wrong kinds of abstraction can also be leveled at utiltarianism. This may seem surprising in the sense that utilitarianism does at least allow room for empirical considerations insofar as it is recognized that the course of action which will maximize utility (however defined) will vary with the distinct patterns of desire to be found in different social settings. Crucially, however, utilitarianism does abstract from the character of the agent making the moral decision - according to this view, anyone who happens to be near the appropriate causal levers should take that action which will maximize utility. To show the significance of this I will discuss a case considered by Williams. The story goes as follows: Jim finds himself in the central square of a small South American town. Tied up against the wall are a row of twenty Indians and in front of them several armed soldiers. The captain in charge questions Jim and establishes that he got there by accident while on a botanical expedition. The captain explains that the Indians are a random group of the inhabitants who, after recent acts of protest against the government, are just about to be killed to remind other possible protestors of the advantages of not protesting. However, since Jim is an honoured vistor from another land, the captain is happy to offer him a guest's privelge of killing one of the Indians himself.

If Jim accepts, then as a special mark of the occasion, the other Indians will be let off. If Jim refuses there will be no special occasion and all the Indians will be killed.

I take it that the answer utilitarianism gives is clear - Jim should kill the Indian. The point of discussing this case however is not that that is the wrong answer but that utilitarianism cannot accomodate considerations which are, arguably, morally relevant considerations. These considerations lend some weight to the contention that utilitarianism cannot treat the concept of integrity seriously. To bring these considerations to the fore imagine (Williams doesn't) that Jim has been a life-long pacifist; that he has spent a large part of his life articulating, defending and working for pacifist goals. This adds a factor to the situation which, it might be said, would make a difference to the utilitarian calculation, since one would have to calculate the amount of disutility to Jim (however that could be done). That is, at best, marginally relevant since it seems clear that Jim's feelings can't outweigh the lives of several people. The real question is whether this is the right way of looking at things. The concept of integrity is related to the idea that each of us is specially responsible for our own actions and the idea that someone might be morally right in refusing to perform an action even if someone else would is, at the very least, comprehensible. If Jim had not been a pacifist but a long standing utilitarian it would perhaps be more accurate to describe Jim as facing the task of performing a morally unpleasant action rather than a moral dilemma; that is so since for a utilitarian there would be no moral consideration to weigh against the course of action the theory recommends. But for Jim the pacifist there is a dilemma between his long standing commitments on the one hand and the appalling consequences of staying true to them on the other. In this case I want

to say that although utilitarianism may give the right answer it does so by employing an oversimplified conception of morality that cannot accomodate cruical, relevant, considerations. Of course, a utilitarian might conclude that utilitarianism cannot accomodate the concept of integrity but to say 'so much the worse for integrity'. That response, I think, won't do. There are hints throughout Smart's defence of utilitarianism that a part of the theory's attractiveness for him stems from its compatibility with a scientific and materialist conception of the world. But since integrity is certainly an indispensible value for the pursuit of science that response would be self-defeating for a utilitarian such as Smart.

In his defence of utilitarianism, Smart discusses a counter-example posed by McCloskey. The point of the counter-example is that a utilitarian may, in unfavorable circumstances, be commited to killing an innocent person in order to maximize utility. Smart accepts the conclusion but bites the bullet and says that, unhappily, the utilitarian would be commited to this action. In his comments Smart says,[18]

> "It is...true that we should probably dislike and fear a man who could bring himself to do the right utilitarian act in a case of the sort envisaged by McCloskey. Though the man in this case might have done the right utilitarian act, his act would betoken a toughness and lack of squeamishness which would make him a dangerous person."

One might wonder whether there ought to be some sort of connection between someone being the kind of person we admire and the kind of person who would do the morally right thing. Should we not fear a moral philosophy whereby the kind of person who could do the morally right thing is simultaneously the kind of person we would have reason to fear ? Smart's comment also makes one wonder whether we really know what a society would be like if it were governed by utilitarian policies and feelings.

It is now time to draw some of these themes together. I began this section by quoting Hume's argument that one cannot derive 'ought' from 'is'. I then went on to point out that for certain functional concepts, factual premises can entail evaluative conclusions. I then raised, without answering, the question of whether there is a perspective from which man can be seen as a functional concept. Now it might be said that from a secular point of view one can't treat 'man' as a functional concept for a functional concept requires reference to a purpose, and there simply is no overall purpose one can describe given a naturalistic view of man's place in nature. What, though, if we ask not for an overall purpose but for the purposes people find in the full context of their lives ? Don't people find purposes as artists, scientists, philosophers, novelists ? Don't people find purpose in their relationships as lovers, husbands, wives, friends ? The suggestion is that if one considers people in their full historical context the gap between 'is' and 'ought' looks less foreboding: to be a scientist is to be someone who will not knowingly falsify the evidence; to be a lover is to be someone who will not willingly inflict suffering on the person loved. Of course we know that people do do these things but we, and they, know that insofar as they do they have failed as a scientist or as a lover. This way of meeting the challenge of personal preference theories is far from being that embodied in either Kantian or utilitarian theories. Those theories have equally, in important respects, treated morality in abstraction from social particularity. It is true that what I have termed a contextual approach to morality has the consequence that moral philosophy would be seen as a much more difficult enterprise than before but that, it seems to me, is a price we must pay. With respect to this contextual approach to morality I am only able to offer a view of what I believe will be some of the concepts required in its

further development.

Those who dissent from personal preference theories have two options: (i) to argue that moral statements may, like other statememts, simply be true or false; (ii) argue that at least with respect to many moral decisions, non-arbitrary reasons can be given for claiming that one course of action is superior to another. The second option promises to be easier than the first, and is the option I favour, but it is worth saying why these options are not so very different. One view in the philosophy of science, a view with which I have considerable sympathy, would say that whilst scientific theories are not true or FALSE simpliciter (since they depend upon many statements some of which may be true and others false) nevertheless non-arbitrary reasons can be given for prefering some theories rather than others. My suggestion is that moral statements are in a similar position - if they are true or false they are true or false in the same way as scientific theories are true or false but that in both cases it is easier, and sacrifices nothing important, to make the more modest claim that although not true or false non-arbitrary reasons can be given for prefering some to others. (This is a point I shall return to in section (3)).

The second issue of importance concerns the problem of moral diversity. I take it that the prima facie sociological and psychological evidence suggests that, both cross-culturally and within one culture, different people have different opinions as to the morally correct course of action in relevantly similar circumstances. Those opposed to to personal preference theories have the option of denying that the apparent diversity is genuine or of explaining how that moral diversity is compatible with the view that there are non-arbitrary reasons for some courses of actions rather than others. First I want to examine the

reasons that might be given for denying that the diversity is genuine.

Perhaps the strongest reason for denying the existence of moral diversity may be given by concentrating upon the phrase 'relevantly similar circumstances'. The more emphasis one places upon the relevance of the historical situation the more reason there will be for denying that cross-culturally, or even within one culture (if one counts someone's personal circumstances as a part of the historical situation) that the circumstances which give rise to apparent moral diversity are relevantly similar. Pettit and MacDonald take this view in their discussion of the Ik. They point out that when one takes into accoumt the harsh environmental conditions under which the Ik live, actions which from our point of view appear cruel may rather be a reflection of the necessity for their adjusting to those conditions. That is an important point and raises another: the extent of moral diversity appears differently depending upon the generality of the level of descriptions one is prepared to employ. For example, we are now liable to suppose that the practice of cutting off someone's hand is hard to justify but many people in our culture would find nothing difficult about the idea that theft should be punished - yet, of course, both of those descriptions may be given of the same action. Important though both of those points are they should not, I think, lead us to deny the existence of all moral diversity. Pettit and MacDonald, who do seem to want to deny the existence of all moral diversity, seem to me to be forced to some unconvincing argument. For instance, when discussing whether the treatment of a woman called Lo'ono can properly be called cruel they say that the action,[19]

> "...need not involve a misperception: a belief to the effect that it is not cruel to treat Lo'ono in the way described. All that may be involved is inattentiveness to the cruel aspect of such

actions, or underweighing of the cruel aspect in relation to other considerations."

Is that a distinction with a difference ? If there is genuine moral diversity however, does this not support the central tenet of personal preference theories ? The best way of reconciling these tensions, I think, is as follows: first, one should note that the essential function of morality is social, to answer the question 'how shall we (a given community) live ?' That question arose as a consequence of evolution, i.e. simply out of the fact that communities of beings with ceratin capacities for appraisal, choice and decision, emerged at some point in evolution. Therefore the essential limits on what is morally possible are finally set by the fact that certain kinds of actions, if allowed to prevail, would destroy the community as a community, e.g. arbitrary murder or wholesale dishonesty. Members of a community have an interest in preserving the community and hence of limiting those actions that could lead to its destruction. Although one can find in history almost any practice 'justified' in someway it is hard to imagine a society where practices such as murder or mutililation were not thought to stand in any need of justification at all. Clearly, however, although this view gives one some grip on the idea of a naturalistic basis for the concept of the morally possible, those limits are nevertheless compatible with wide variations in the sets of values adopted within a community. Although at one level of description one would expect to find similar lists of the virtues (e.g. one would expect to find honesty, sincerity, kindness, courage, valued to some degree in any community) there is still room for endless permutations and emphasis with respect to the ordering of those values. That is the more to be expected if recognizes the fact that the overall patterning of values in a society

will be inextricably linked with the cognitive beliefs and the envi mental conditions within which an individual or society functions.

Much of moral philosophy has been insensitive to the kind of moral diversity that I have in mind for the reason stated earlier, i.e. that it has abstracted from most kinds of social particularity. To discover moral diversity one need not consider exotic cultures or even two people in our culture. This is wonderfully expressed in a passage by Anthony Powell,[20]

> "Trapnel wanted, among other things, to be a writer, a dandy, a lover, a comrade, an eccentric, a sage, a virtuoso, a good chap, a man of honour, a hard case, a spendthrift, an opportunist, a raisonneur, to be very rich, to be very poor, to posses a thousand mistresses, to win the heart of one love to whom he was ever faithful, to be on the best of terms with all men, to avenge savagely the lightest affront, to live to a hundred full years and honour, to die young and unknown but recognized the following day as the most neglected genius of the age. Each of these ambitions had something to recommend it, from one angle or another."

Mary Midgeley, who also quotes this passage, comments, I think rightly, that the only difference between Trapnel and ourselves is that he is even a little less clear than the rest of us about the consistency of his values.[21] The culture of modern western society is more diverse than most having inherited fragments from many diverse traditions (e.g. Christian, Greek, utilitarian). MacIntyre emphasizes this point in his (1981) but laments the fact more than I.

I can summarize the upshot of the above in two points: first, that the limits of what is morally possible are naturalistic, set by our need and desire to operate as members of a community; second, those limits woefully underdetermine the set of values one must live by to live in a morally permissible way (and, hence, cannot be captured by some simplistic set of moral rules). But now one can imagine the personal preference theorist becoming still more insistent: suppose they

concur that the ultimate limits of morality are non-arbitrary, does that not at least prove their point for the very wide range of options which I have admitted remain open ? They might add that this is the more so since I have also said that no one set of abstract moral principles will be sufficient for determining one uniquely acceptable set of values. What I think this objection overlooks is how large a part of our culture and inquiry (in addition to that explicitly recognized as moral reasoning) is an attempt to give or to find non-arbitrary reasons in favour or against particular constellations of values and beliefs. The explication of the claim will occupy much of the next section. When articulated it will make apparent how very unclear I believe the moral/non-moral divide to be.

(3) Truth, Values and Inquiry

> "'Beauty is truth, truth beauty', that is all
> Ye know on earth, and all ye need to know." Keats.22

> "The human mind always has and always will be able to interpret facts in accordance with its moral interests." James. 23

> "In this life, we want nothing but Facts, Sir; nothing but Facts !"
> Gradgrind. 24.

Although only one of the above quotations mentions the word 'truth', indirectly they all concern the concept of truth, its nature and importance. In one way the statement by James represents a position in the middle of a continuum between Keats and Gradgrind. It will come as no surprise that it is James' position that I wish to defend but in so doing I wish to point to the virtue and vices of the other positions.

It is perhaps mischievous, but suggestive, to think of analytic philosophy has having gone from a conception of truth not unlike that

of Keats (e.g. in Plato) to one not unlike that of Gradgrind (e.g. on one interpretation of the Tractatus) - a process of going from the sublime to the ridiculous. Even in making that comparison I don't wish to suggest that there are not good (rational) reasons why philosophers have trodden that path but it is, perhaps, time to retrace a few steps. Partially the resons why philosophers have trodden that path are technical - philosophers have found it hard enough to give the semantics for 'A cat sat on the mat' let alone anything more ambitious. More fundamentally however, (although these reasons are not ultimately separable) the reasons are cultural. Plato thought of philosophy as the pursuit of wisdom and since philosophers no longer see that as a realistic aim, students and others sometimes seem to think (I know because I used to think) that philosophers have abrogated their responsibility by giving up that role and, like social security investigators, they want to know the reason why. What they overlook is that the role is no longer vacant - the job has been abolished. The reasons why would really amount to a history of western culture so here I will simply note the fact and rush to the other end of the spectrum. (See, however, my comments in section 4).

In deference to Gradgrind's view it must be admitted that there are facts of the kind he had in mind - millions of them. 'Facts' in this sense refers to the kind of statement Gradgrind wanted drummed into the heads of children - the nine times table, the capital cities of the countries of the world etc. What characterizes all such facts is a tough philosophical question and the only answer that seems adequate to me is that there are agreed and uncontroversial methods for their discovery and confirmation. I don't believe that such facts can be characterized either by the way they are learnt, their being observable,

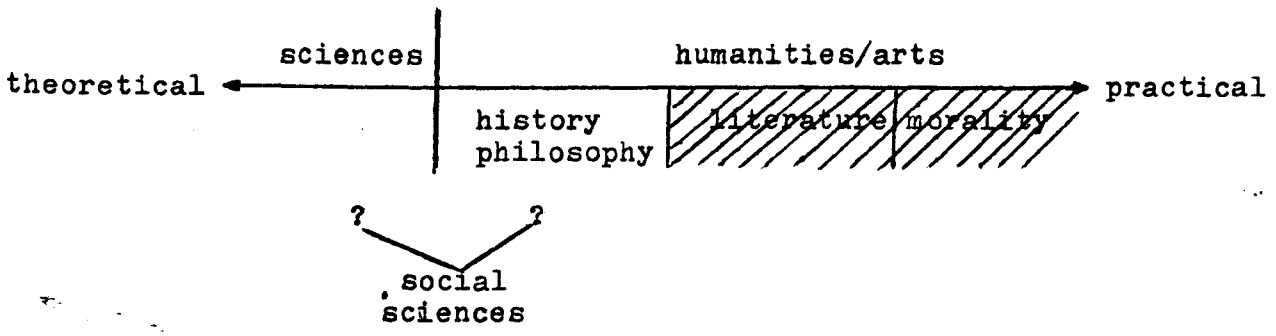
or in their 'corresponding' to reality. Another philosophical question is whether we should restrict the predicate '..is true' to those statements where we do have agreed methods for their discovery or confirmation. If we do then we can't speak of scientific theories being true or false since scientific theories are certainly more than mere collections of such facts. That need not invite epistemological scepticism however since one may still argue that even though scientific theories are not strictly true or false they are subject to rational methods of appraisal, argument, and evidence. Although there is no very clear dividing line, I do incline towards restricting the predicate '...is true' to those statements where we do have agreed methods for their discovery or confirmation. (Thus moving away from the Keats end of the spectrum). This is, in a broader context, the point that underlay my distinction between a direct and indirect pragmatic theory of truth in chapter 1. Favouring an indirect theory, I said, makes it clear that other things than truth do matter. That point brings me to the first of Gradgrind's mistakes, viz. that in this life we do want many more things than facts even for science. The second of Gradgrind's mistakes lay in supposing that the existence of facts justified his miserable philosophy of life. That is a point I shall come back to.

To raise the next question I want to discuss I need to employ the distinction between the context of justification and the context of discovery. As the labels suggest, this is a distinction between the way in which the results of an inquiry may be discovered and the ways in which the results of an inquiry may be justified. In the recent philosophy of science it is now commonplace to recognize that the values of the scientist may, quite legitimately, play a role in the context of discovery that they may not play (or should not be allowed to play) in the context of justification. My question is whether this view

of the role that values play in inquiry can be upheld for all areas of inquiry. To examine this question I propose to discuss the following model of inquiry.

Figure 7

A Model of Inquiry



A few words of explanation: 'theoretical' implies that the primary purpose of the inquiry is explanatory; 'practical' that the inquiry is addressed to the question, 'how shall we live ?' The shading indicates that in those areas evaluative significance plays a greater role than in the unshaded areas. The most important point of the model however is to suggest that the humanities have a more or less direct role to play in answering both the theoretical and practical questions. How far the social sciences fit into the natural sciences is, for reasons I shall come to, ambiguous. I dare say that I hardly need add that I do not see the ends of the spectrum running from the cognitive to the non-cognitive. I shall now try to defend and explain what may seem to be my arbitrary model by working from the right-hand side to the left-hand side.

Morality, in the sense of the autonomous definition, obviously occurs near to the right-hand side since it is explicitly concerned with the question, 'how shall we live ?' (In this sense, of course, my

placement of philosophy further to the left is uncomfortable since moral philosophy, or a part of it, belongs here). However, the merit of the model, I shall try to show, resides in its relationship to the contextual approach to morality.

Literature and morality occur next to one another since I believe that evaluative significance plays an important role in understanding either. In section (1) I tried to show that even a statement such as 'All men are mortal' could have an evaluative significance when placed in a particular context. Note that this is ambiguous between the evaluative significance for the person concerned - the patient - and the evaluative significance for the reader - someone who did not understand that people do not generally want to die would not have understood my point. I tried to convey what the evaluative significance was by hinting at how the patient felt - in effect I was making the first crude steps in story-telling. Emotion, I suggest, plays a cognitive role in morality and literary experience. Emotional insensitivity in these areas prevents cognition. If this is true then evaluative significance plays a role in the context of justification and not merely in the context of discovery. There would appear to be a similarity between the significance of evaluative significance and the distinction between form and content. Mathematical and scientific theories are relatively insensitive to transformations into logically equivalent forms - one can understand Einstein's theory of relativity without ever having seen any of his original papers but Shakespeare's plays, for instance, are very much more sensitive to their means of expression. (It is worth noting, however, that other parts of the humanities are ambiguous on this point. It is unclear, for instance, whether the content of Wittgenstein's philosophy can be fully understood independently

of the form in which he expressed it. That makes the business of 'extracting arguments' from Wittgenstein's work a sensitive enterprise).

Literature appears to the left of 'morality' and the right of the other humanities since, on the one hand, it is certainly not its purpose to tell us how to live (although didactic tracts are not unknown) but, on the other hand, it explores the issues which are highly relevant to the question of how we shall live. Moreover it does so in a way that is a great deal more senstive to the issues involved than moral philosophy has been. For an understanding of moral life, novels offer a deeper understanding than moral philosophy has yet attained.

How does the statement by James stand with respect to the humanities ? I want to suggest that it gives a very good description of their purpose and that this provides an important link with the contextual conception of morality. The way in which this is true is that in the humanities the value of the inquirers (be they the producers or the critics of some work) play a crucial role in the context of justification: they play the role of being a partial criterion of success. That is a large and controversial claim - to begin to defend it, and to help define when a piece of work counts, on my view, as belonging to the humanities, I will first consider the situation with respect to social sciences.

There has been a long-standing and inconclusive debate over the question of whether the social sciences can be 'value-free'. Now one of the important ways of understanding that question is to ask whether in the social sciences one can restrict the role played by the values of the inquirers to the context of discovery, i.e. of whether values can be held to play the same role in social sciences as they do in

natural science. It seems to me that the answer to this question is not the same for all parts of the social sciences now classified as such. For instance, one thing social scientists do is to study the methods for carrying out social surveys; in particular they attempt to find methods of eliminating the various complex problems that stand in the way of discovering what people really believe about some issue. Here I see no difficulty in principle in claiming that values play no more fundamental role in the study of those techniques than they do in the natural sciences. Similarly for experiments in social psychology: the values of the experimenter may affect what issues or questions the experiment is designed to investigate but given careful methodology (e.g. the use of control groups) there does not seem any greater role played by values here than in the natural sciences. Suppose, however, one turns to a much more complex case, e.g. Marx's analysis of capitalism. It is, I take it, uncontroversial that Marx's analysis was intended to be politically revolutionary, and only slightly more controversial to suppose that it could only be revolutionary if it were saying that there is something fundamentally and morally wrong with capitalist society. Now suppose that someone sets out to discover how much of Marx's analysis is true independently of any value commitments that Marx had. I rather doubt that this is likely but suppose that there is a considerable amount of Marx's analysis which is true and which does not presuppose any value commitment pro or anti capitalism. Call this analysis 'm' (Marx in a low key). As I said, I have no idea how far such an analysis could succeed but in any case there is a very real doubt as to whether m could really be called Marxist at all. More importantly perhaps, there is even more doubt as to whether this analysis would settle anything for what we would then get would be Marxist and non-Marxist analysis of m ! The point is that whether one has pro or anti

Marxist values one wants a theory which is consonant with those values, so that for this kind of enterprise a 'value-free' analysis would not achieve what is wanted. More generally, a point I shall be trying to vindicate in much of what follows, a large part of our inquiries, I claim, derive their purpose and point from attempting to interpret the world in such a way that that interpretation both articulates and develops our values. That such inquiries are interpretations, that there are facts to which they have to be true, is a partial vindication of the claim that such values are not arbitrary.

Before proceeding with my main theme it may be worthwhile to digress slightly on the issue of whether the social sciences should be classified as belonging to the natural sciences. A part of the trouble, I believe, is that there have implicitly been two kinds of criteria a discipline must satisfy (in some intuitive way) for it to be considered a part of the natural sciences and that those criteria are logically independent. On the one hand there is, as I have said, the question of whether values play a role in the context of justification. On my view, parts, perhaps even large parts, of the social sciences may reasonably be classified with the natural sciences by this criterion. On the other hand, there are epistemic criteria: because of the very success of the natural sciences there has come to be a reluctance to classify any theory as scientific which does not attain a considerable degree of power and economy which would enable it to make accurate predictions. By that criteria the social sciences, to put it mildly, seem to fare badly. Some arguments in sociology and philosophy have tried to show that the reason why the social sciences have not produced powerful theories is either because the subjects of the inquiry have values, or because of the involvement of the investigator's values. In my opinion

none of those attempts to argue that view have yet been either very clear or very cogent. Davidson (and Fodor) seem to me to have produced compelling reasons why, even if a materialist view of mind is correct, it does not follow that we must therefore be able to obtain 'covering laws' which would enable one to predict human behaviour.[25] I don't, however, understand Davidson's further argument for the conclusion that it is in principle impossible to obtain covering laws applicable to human behaviour.[26]

Returning to my major theme, that in the humanities values play a significant role in the context of justification, it seems to me that it is a significant fact that disciplines such as philosophy, literary criticism, and history have a different relationship to their history than does, say, physics. That is so in two ways: (i) that in these disciplines the original texts are still of fundamental importance; (ii) the each new 'age' needs its own philosophy, literary criticism or history. I don't mean merely that as time passes new problems arise, or in the case of history that there is a new period to study (although that is true), I mean rather that as the culture in which we work changes we acquire new values and the need for new values. We need, therefore, a new philosophy, a reinterpretation of past literary texts, or period of history consonant with those values. One can't, of course, quantify 'new age' - a new age in this sense arises when, for whatever reasons, the current or traditional views don't fully meet the demands of the situation. In philosophy, for instance, the need for a <u>fallibilist</u> epistemology began to emerge as soon as the apparent certainties of Newtonian mechanics, Euclidian geometry, and Christian religion began to be undermined. That meant not only that a new set of 'technical' issues arose but also that there had to be a reworking of values and

beliefs to enable us to live with the new kinds of uncertainty. Lest it be thought that this is only true of very broad generalizations, true only of past changes in philosophical climate, I should like to press this view of philosophy a little further.

Consider an issue currently at the centre of philosophical research - the philosophical implications of artificial intelligence (A.I.) It seems to me evident that a part of the reason why that issue is currently under discussion is that the results of A.I. threaten, or appear to threaten, our image of ourselves as people. It is not, I think, adequate to say that the problems posed by A.I. are confined to the context of discovery for the philosophical problems are <u>defined</u> by the tension between between our value impregnated picture of ourselves as free, rational, responsible beings and work which may appear to show that we may not be any of these things. The <u>very problem</u> is whether we have to modify our value impregnated picture of ourselves or not. One way of trying to reconcile these tensions is to argue that the results of A.I. can't possibly show we aren't any of the things we think we are (e.g. Searle[27]); another way is argue that our picture of ourselves is incoherent (e.g., albeit in a different context by Skinner.[28] It seems to me that Skinner's views would remain unpopular even if he had written much better philosophy than he did); another way is to show that our picture of ourselves may only need modifying somewhat (e.g. Dennett[29]). If someone such as Dennett can really convince us that the potential results of A.I. don't really threaten anything essential to our image of ourselves then we could breathe a sigh of relief and move on. In certain respects the debate over the significance of A.I. resembles the 'crises' prompted by evolutionary theory: given the values of the participants some were prepared to accept the new theory; some

were prepared to reject it; and others sought a reconcilation with religion. Now although I think arguments can be produced in favour of some views rather than others it is important to notice that there is not a neutral standpoint in the sense that there is a standpoint that does not involve the values of the inquirer. Skinner and Dennett have a value commitment to science which will not be easily overridden. (Even if one finds Skinner's view of science naive). Searle has a value commitment to the image we have of ourselves. (Dennett, unlike Skinner, is fully aware of the importance of that commitment). The point, though, is that they all do have a value commitment, a commitment which in part defines the very nature of the problem.

At this point I want to consider two possible objections. The first objection is that whilst the tensions between our values and philosophical arguments may be evident with respect to a large issue such as that concerning A.I. it is not evident with respect to many other relatively 'technical' issues. I accept that point and would add that with respect to minor technical issues philosophers with different values may accept the same conclusions. One needs to be cautious however: on the face of it the analytic/synthetic distinction was an arcane technical distinction but Quine's attack on it threatened what was then a prevalent conception of philosophy in Oxford associated with a particular constellation of values. As Quine put it, his attack implied a shift towards pragmatism and a shift towards pragmatism was as unwelcome in some senior common rooms then as, I suspect, it is now. Arguments over technical issues in philosophy seem to me like battles in a campaign where the campaign consists in attempting to develop a coherent picture of the world and man's place in it that articulates and defines a 'cultural space' where values and beliefs can he held in reasonable

harmony. In that sense Quine's cultural space is very different from Goodman's, and both are very different from Russell's, in spite of the issues on which they agree and the lack of animosity between them.

The second objection I want to consider might come from a personal preference theorist who agreed on the importance of values in the manner I have indicated and then claimed that this was a vindication for their view. This claim would be that any appearance of rationality in philosophical argument was a sham; that at the end of the day one merely has the imposition of different wills. This, I believe, is simply wrong. In the course of any inquiry standards of success and failure do get developed, standards which to some extent are independent of values, and this is true in philosophy. Philosophers can and do recognize that someone's argument is good or bad philosophy independently of whether they agree with the conclusion or find in the work values to which one is sympathetic. Given that there is this independence to some degree, this gives four kinds of response one philosopher might have to another's work: (i) the ideal case where the work is technically competent (or better) and incorporates values to which one is sympathetic; (ii) technically competent but incorporating values to which one is not sympathetic; (iii) not technically competent but incorporating values to which one is sympathetic; (iv) neither technically competent nor incorporating values to which one is sympathetic. Now although life would be depressing if one never came across work which falls into category (i), the real challenge comes from work which falls into category (ii). By definition the work contains no obvious mistakes or flaws so the challenge is either to find some subtle flaws or mistakes or to amend one's own values (or some combination thereof).

That last picture was explicitly about philosophy but it seems to

me to incorporate the appropriate model not just for other areas of inquiry but also for much of our 'moral life' in general. Two people discussing a television programme or a film are not terribly likely to be having an explicitly 'moral' argument, but it is quite likely that they are having, in various degrees, an argument over aesthetic values, political views and theories, and are attempting to alter each other's values and/or beliefs. It is here that my attack on dominant conceptions of morality, my advocacy of a contextual approach to morality, and my conception of the nature of inquiry meet. By ignoring the substance and structure of our everyday life, utilitarian and Kantian theories leave the way open for the personal preference theorist to claim that there can be no non-arbitrary choice of values. But if we consider people in their social context one can surely understand how and why people value what they do for non-arbitrary reasons.

When it comes to the natural sciences my view of the fact/value distinction, for once, albeit briefly, follows conventional wisdom. I think that implicitly we are inclined to make it a necessary condition of some inquiry counting as a part of the natural sciences that the values of the inquirer can be limited to the context of discovery. But now the question arises as to how James' statement that we can always interpret the world according to our moral interests stands with respect to the natural sciences. Does the fact that values in natural science only play a role in the context of discovery entail that the view of James is false for the natural sciences ? I think not. The idea that the discoveries of science dictate what our moral values shall be is Gradgrind's mistake writ large. The way I view these matters is that when one starts to ask what the results of science imply about oursleves, the metaphysical nature of the universe, or what is morally possible,

one ceases to be engaged in science and becomes engaged in something like the philosophy of science, i.e. the question shifts back into a question within the humanities. On the view I have advocated, in the humanities values play a role in the context of justification - 'success' in constructing an adequate metaphysics or moral system is partially judged by how well it encapsulates and defines our present values or leads us to want to adopt new ones.

(4) Philosophy and Pragmatism

By way of a coda to the previous sections of this chapter and to the entire thesis, I want to compare and contrast my views with those of Kekes and Rorty. Within the last few years Kekes and Rorty have produced works which reach diametrically opposed conclusions; Rorty has argued that philosophy as traditionally conceived ought to disappear, whilst Kekes has argued that philosophy ought to return to its traditionally defined role as the pursuit of wisdom. That such works, reaching such opposite conclusions, should appear within a short period of time, raises the question of whether a view of philosophy between the extremes posed by those authors is tenable: can the centre hold ? In addition to that important question, reading Kekes and Rorty I find an additional, ironic, reason for discussing their works: from my point of view I find that Kekes is more of a pragmatist than he realizes, and Rorty is less of a pragmatist than he claims.

I find much to admire in Kekes (1980) (The Nature of Philosophy). Kekes claims that the central task of philosophy is the development and justification of a world-view, where a world-view is understood to consist of five elements: a theory of the nature of reality or metaphysics; an account of the human significance of the nature of

reality or an anthropology; a system of ideas or a culture; an explanation of the discrepancy between the ideal and the actual state of affairs or a diagnosis; and a programme for overcoming or mininimizing the discrepancy or a policy. The examples of world-views which Kekes gives are: the philosophies of Plato, Spinoza and Hegel; the theology of Christianity; the pragmatism of Dewey; the existentialism of Sartre; the liberal utilitarianism of Mill; the positivism of Comte, Spencer and the Vienna Circle.

Much of Kekes' work is concerned with arguing that rational justification of the five elements that constitute a world-view can be given; that is, non-arbitrary reasons can be given for supposing one world-view to be superior to another with respect to the elements mentioned. Very often I find that Kekes reaches similar conclusions to those that I reach although we come to those conclusions via different routes. It is not appropriate to review these similarities and differences here, but it is appropriate to discuss the one major difference of opinion between us.

I can begin to articulate the major difference of opinion between Kekes and myself by examining Kekes' claim that philosophy ought to return to its traditional role as being the pursuit of wisdom. I find that claim, at this stage of our cultural history, too strong; I find myself unable and unwilling to look someone in the eye and say that I am engaged in the pursuit of wisdom ! That is not, I think, a mere idiosyncrasy of my psychology; I believe that it is a realistic reflection of what is and what is not culturally possible at this stage of history.

One of the difficulties I find with Kekes' work is what exactly

his central thesis amounts to. Although I believe that Kekes provides much convincing argument to show that some world-views (or parts of world-views) can be rationally justified over others, he seems to be suggesting that philosophers should assume that there is one uniquely best world-view which it is their task to discover. Certainly Kekes cannot claim to have demonstrated that there is one such uniquely best world-view for in practice he only produces arguments to show that in principle some world-views can be justified in preference to others - he produces no substantive argument to show which world-view(s) is (are) best. I am sceptical of the suggestion that philosophers should assume that there is a uniquely best world-view and attempt to discover what it is. Partially for the reasons given earlier in this chapter I believe that whilst the underdetermination of theory by data is an interesting theoretical possibility in physics, it is an almost incontrovertible fact with respect to our 'moral life'. My view of what philosophers can hope to achieve is therefore less optimistic than the view taken by Kekes. The task of articulating a world-view, as conceived by Kekes, is a formidable one, one such that it seems philosophers would be doing well if they were even partially successful. (It seems to me salutory to bear in mind that even the greatest social thinkers have rarely, if ever, developed a world-view in the sense defined by Kekes; very few thinkers have had a fully-articulated theory of all five elements that constitute a world-view). One way of accomodating the tensions between my views and Kekes' would be to employ a distinction between a philosopher's personal vision and his public utterance. That is, one could agree with Kekes that the personal ambition of philosophers ought to be to develop a world-view, but, being more realistic with respect to the sheer complexity of our cultural situation, recognize that it is unlikely that many philosophers will complete the task in such a manner that every

part of their personal vision will receive, or stand the burden of, public utterance.

Above I said that from my point of view Kekes was more of a pragmatist than he realized. At one point in the book he says,[30] "It would be a radical misunderstanding of my view...to suppose it to be a version of pragmatism." Toward the end of the book, however, Kekes writes,[31]

> "...my view of philosophy implies that a long-standing philosophical, and also religious, mystical, scientific, ambition is misdirected. If the human perspective is inescapable, then all attempts to form a view of the world independently of the anthropocentric view are doomed. The ambition to understand reality sub specie aeterniatis is unrealizable."

The apparent difference between Kekes and myself here seems to me largely verbal - a question of how broadly or narrowly one defines pragmatism. In the introduction to the thesis I characterized pragmatism as the rejection of both absolutism and subjectivism and Kekes certainly also rejects both of those views. From my point of view the second quotation could well be a motto for all pragmatists (supposing it is not taken as an endorsement of subjectivism).

My feeling towards Rorty's (1980) and (1982) is just the opposite to my feeling toward Kekes - a feeling that Rorty's kind of pragmatism is certainly a very significantly kind of pragmatism from mine if, indeed, it is a variety of pragmatism at all. The theme of Rorty's (1980), that knowledge can never simply be a pure reflection of reality, (that there is no 'mirror of nature') is, of course, one to which I am sympathetic. However, although Rorty explicitly mentions three major philosophical heroes (Dewey, Wittgenstein and Heidegger) the overall conclusions of his work are much nearer to Wittgenstein than to Dewey; Rorty, like Wittgenstein, reaches the conclusion that what we need is

not <u>better</u> philosophy but the <u>end</u> of philosophy. Rorty seems to think that the conclusion that philosophy cannot provide or attain theoretical knowledge (as opposed to therapuetic enlightenment) follows from the rejection of the 'mirror of nature'. I do not believe that that conclusion really does follow, and I do believe that philosophy can and should attempt to provide theoretical understanding of the world. Rorty has a characteristic way of arguing that I find unsatisfactory; this way of arguing consists in presenting two extreme positions and then claiming that since the one position is wrong the other must be right. For example, in his paper, "Philosophy as a Kind of Writing: An Essay on Derrida", Rorty begins by contrasting two views of science. According to (part of) the first view of science,[32]

> "...there are some invisible things which are parts of everything else and whose behaviour determines the way everything else works. Physics is the search for an accurate description of those invisible things, and it proceeds by finding better and better explanations of the visible."

According to the second view of science,[33]

> "...the physicists are men looking for new interpretations of the Book of Nature. After each pedestrian period of normal science, they dream up a new model, a new picture, a new vocabulary, and then they announce that the true meaning of the Book has been discovered. But of course, it never is."

Rorty then goes to draw two opposed pictures of morality and philosophy, claiming that in each case the second view is the view that is more or less correct. In each case, however, I want to say that there is something right about <u>both</u> views and that the philosophical problem is to show how that can be the case. I really can't believe that Dewey would have accepted those dichotomies. I think that we need philosophy without the mirror of nature, but we still need philosophy. Kekes' (1980) seems to me to provide (unintentionally of course) a very good reply to Rorty's (1980) for Kekes, no less than Rorty, rejects the absolutism which underlies the mirror of nature metaphor.

The appearance in the same year of works which reach such diametrically opposed conclusions naturally raises the question of whether the philosophical status quo is stable. For my part, I believe that philosophy should move in the direction favoured by Kekes - that it should be more ambitious rather than less. I have already said that I don't believe that philosophy can achieve all that Kekes demands; but that is not a case for not trying. Contrary to Rorty, I believe that if the current state of analytic philosophy is unstable that is not because there can be no philosophy after the rejection of the mirror of nature (for the concerns of philosophy are perennial), but rather because there has been a failure of nerve and a tendency to 'play it safe'.

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Appendix 1

The Development of Quine's Philosophy

It would hardly be surprising if, over a distinguished career spanning more than fifty years, Quine had had occasion to reject or to modify radically some of his views; what is surprising is that Quine has nowhere ennummerated or admitted to any significant change of mind. It is true, I think, that there is a remarkable continuity in Quine's philosophy but I do detect a very significant change of emphasis - a change of emphasis which leaves one wondering about the general import of Quine's philosophy. The change of emphasis to which I refer concerns the central concept of Quine's relativism to which my comments will be restricted.[1]

In his seminal collection of essays From a Logical Point of View Quine made the following statements,[2]

> "The fundamental-seeming philosophical question, How much of our science is merely contributed by language and how much is a genuine reflection of reality ?, is perhaps a spurious question..."(1950)
>
> "...it is meaningless..to inquire into the absolute correctness of a conceptual scheme as a mirror of reality. Our standards for appraising basic changes of conceptual scheme must be, not a realistic standard of correspondence to reality, but a pragmatic standard." 3. (1950)
>
> "...there is in principle no separating language from the rest of the world..." 4. (1951b)
>
> "The lore of our fathers is fabric of sentences....It is a pale grey lore, black with fact and white with convention. But I have found no substantial reasons for concluding that there are any quite black threads in it, or any white ones." 5. (1954)

These views seem to constitute a radical relativism; they clearly seem to imply the rejection of absolutism. Yet by the time of his (1960) Quine was speaking of "...limning the true and ultimate scheme of reality..."[6], and in his (1975c) of his "...fully-realistic attitude

toward electrons and muons and curved space-time..."[7] It sounds like a different philosophy but is it merely a change of emphasis ? My explication of relativism has employed the central concept of internal justification and it is true, I think, that Quine, both early and late, has been an advocate of that view. In his (1960) Quine wrote,[8]

> "It is..when we turn back into the midst of an actually present theory, at least hypothetically accepted, that we can and do speak sensibly of this and that sentence as true. Where it makes sense to apply 'true' is to a sentence couched in the terms of a given theory, complete with its posited reality."

Even later in his (1975b) Quine still emphasized that we always work within our conceptual scheme,[9]

> "...there is no extra-theoretic truth, no higher truth than the truth we are claiming or aspiring to as we continue to tinker with our system of the world from within."

There are, I think, two related problems here. The first is that as I understand it any non-trivial version of internal justification denies that there can be any way of separating our contribution to knowledge from that of the world. (A part of the motivation for a theory of internal justification, in my view, is to avoid having to try). Yet in his (1973) Quine wrote,[10]

> "...we ought to be able to see just to what extent science is man's free creation; to what extent, in Eddington's phrase, it is a put up job."

That, to my mind, is straightforwardly inconsistent with the views expressed in From a Logical Point of View. Moreover, and this leads to the second problem I mentioned, in Words and Objections, Quine replied to Smart's suggestion[11] that there had been a change from the 'pragmatism and instrumentalism' of From a Logical Point of View to the 'realism' of Word and Object by saying that the "...appearance of vacillation is a misunderstanding."[12] I think that there is a way of agreeing with Quine but at a certain cost.

An absolutist like Williams is sensible enough to admit that we always have to work within our conceptual scheme in the sense that we always have to use concepts which are ours, we always have to use a language which is ours, but goes on to say that if we are lucky we may discover true statements which will correspond to 'what is there anyway'. One might call Williams' view a trivial version of internal justification since it concedes a minor point but still demands a correspondence theory of truth which it is the purpose of the non-trivial version of internal justification to avoid. The problem for the interpretation of Quine's philosophy now becomes: does Quine merely uphold a trivial version of internal justification ? The emphasis in From a Logical Point of View suggested that he upheld a non-trivial version of internal justification, but much of what Quine has written since suggests that either (i) he does uphold the trivial version, or (ii) that he has not specified how his version of internal justification differs from the trivial version.

The suggestion that Quine's view of internal justification may, after all, be the trivial version, gains support, I think, from reflecting upon the importance Quine attaches to the ontological enterprise. Campbell, in his (1976), represents Quine's ontological programme as an attempt to list the distinct categories of things that there are. There are two points to be made about this. First, I find nothing in Campbell's representation of Quine's ambitions which seems like a misrepresentation. Second, there is point to Campbell's ontological programme only for someone who either rejects internal justification or only accepts the trivial version. (See chapter 3 for my reasons for this assertion). One could agree with Quine that there was no vacillation between From a Logical Point of View and Word and Object in the sense

that he still upholds a form of internal justification but claim that there was vacillation in that there was a shift from a non-trivial version of internal justification to a trivial version of internal justification. Certainly I find it hard to avoid the feeling that at some point in his career Quine wanted to uphold more than the trivial version of internal justification, but very hard to read his later work (from Word and Object on) as upholding the strong version of internal justification.

Appendix 2

Davidson on Conceptual Schemes

I find Davidson's (1973b) somewhat enigmatic; I find it difficult to decide if or why I disagree with his argument. The problem is twofold: (i) that Davidson's argument is conducted at a very high level of abstraction; (ii) that some of Davidson's targets are views that I would not care to defend, e.g. that there is a scheme/content dichotomy; that cultural relativism consists in the complete untranslatability of one language into another. It may be the case that the kind of cultural differences I believe to exist, Davidson would not challenge. Davidson discusses supposed exotic and radical differences between one conceptual scheme and another, but the kind of cultural differences that concern me are the kind that one might find charted in almost any history of any subject. Where we may disagree is that I believe that even at that level the differences raises questions about the concept of truth.

One example will suffice to illustrate the problem of the level of abstraction. Davidson says,[1] "Charity is forced upon us; - whether we like it or not, if we want to understand others, we must count them right in most matters." At the very least there is trouble with the word 'most' here for if, for example, I want to understand a Nazi it seems that I must translate his words in such a way that his beliefs come out wrong in 'most' matters. A translation of a Nazi's words that made his values and mine similar would be a mistranslation. Now of course the Nazi is (happily) a rare and extreme case, but the kind of case that I believe is important is like that but closer to home. People may have values with which one may sympathize without sharing.

One example might be arguments over the claim that a foetus is a person. Now I do not have in mind the arguments between people radically opposed to one another, where it may be the case that even if they could agree on all the 'facts' they would still reach different conclusions. What I have in mind is the kind of argument that might occur between two pro-abortionists over whether it is proper to classify a foetus as a person or not, for here we have a case, I think, where the decision about which way to 'organize' the 'facts', about how to 'classify experience', is both significant and not open to Davidson's objections to the scheme/content dichotomy. What makes the decision important is its potential ramifications for many of our other beliefs and practices. I believe that this kind of example provides an instance of the kind Davidson seems to imply cannot occur: a case where we might have a significant choice between 'schemes'. I think it may also be true that that kind of case is far more typical that we suppose because of our natural tendency to take our assumptions for granted; a tendency not threatened, it must be said, by the a-historical nature of most analytic philosophy.[2]

Appendix 3

Extensionality as a Philosophical Commitment: Some Questions

Quine and Goodman may, reasonably I think, be spoken of as extensionalists meaning, roughly, that they favour extensional languages. To say that they favour extensional languages seems far too weak but there are great difficulties in describing what else extensionalist philosophers have in common that does not sound wildly implausible. Taking the less problematic case first, Quine's view might be described as the view that extensional languages are adequate for the whole of science and mathematics. There are, however, very powerful objections to that view. Marcus, in her (1960), makes the important point that extensionality comes in degress, but even if one takes the definition of extensionality for granted, the problems for the Quinian view soon multiply. There are very good reasons, it seems to me, for doubting whether extensional languages are adequate for science itself. I argued in chapter 4 that modal idioms are not dispensible in science, and modal idioms, of course, create intensional contexts. Even if one grants that extensional languages are adequate for science itself, the question should be asked as to whether extensional languages can be adequate for philosophy and, if not, where this leaves the 'extensionalist philosopher.' That extensional languages are not adequate for philosophy seems almost obvious. A number of problems have been raised about Quine's criteria for ontological commitment, problems which essentially turn on the non-extensionality of most of Quine's criteria of ontological commitment or the inadequacy of his extensional versions of ontological commitment.[1] To my knowledge these problems have never been satisfactorily resolved. In any case the general claim would give rise to some apparent

paradox: the only philosophy of mind that, presumably, would be acceptable would be some version of behaviourism but surely, whether behaviourism is right or wrong, behaviourism has to be an issue within philosophy. Would that mean that it was acceptable to talk about intensional languages but not to use them ? Suppose, then, that it is claimed not that philosophy can be expressed in extensional languages but only that science can. That, I think, would be a very uncomfortable position for Quine to adopt given his stress on the continuity of science and philosophy. One would also want a clear justification for this double standard - if philosophy can be intelligible when it employs intensional languages, why can't science ?

If Quine's position seems difficult, Goodman's seems impossible. Goodman argues that art is cognitive but an 'extensional version' of, say, War and Peace, seems to be even a theoretical impossibility yet, by Goodman's own lights, ought to be counted a contribution to knowledge.

I find it surprising that, to my knowledge, these very general problems about what a commitment to extensionality might amount to have never been discussed, but I raise them with some trepidation, feeling like the boy who claimed that the emperor is naked.

Notes and References*

1. Allen (1966), Chapter 2.

Introduction

1. Peirce (1878), p.124. I have misgivings about always using this quotation as an introduction to Peirce because most readers (in this country at any rate) are likely to be more familiar with logical positivism than with pragmatism and are likely to read the pragmatic maxim in that light. Read in the light of logical positivism the passage may be taken to imply, or portend, phenomenalism and the verifiability principle, and with those concepts in mind the rest of Peirce's paper may misleadingly suggest that pragmatism had the same sort of relation to science as logical positivism. Murphey's description of the origins of pragmatism place it in a very different historical context, "...pragmatism in American philosophy...was introduced as part of an idealistic idealization of science and in the interests of harmonizing science with religious and metaphysical views.It was an outgrowth of post-Kantian romanticism..." Murphey (1968), p.14.
2. Peirce ibid., p.133
3. James (1909), p.117
4. Peirce (1905), in Thayer (1968), p.495.
5. Williams (1978), p.64.
6. Peirce (1878), p.133
7. Peirce, comparing himself with James, wrote, "Who..could be of a nature so different from I ? He is so concrete, so living; I a mere table of contents, so abstract, a very snarl of twine." Collected Papers, VI.184.
8. James wrote that pragmatism "..agress with nominalism..in always appealing to particulars; with utilitarianism in emphasizing practical aspects; with positivism in its disdain for verbal solutions, useless questions, and metaphysical abstractions." (1907), p.32.
9. Quine's views are discussed throughout the thesis but for my view of the difficulties of providing an overall interpretation of his philosophy see the appendix.
10. For my views of Davidson see chapter 5 and appendix 2.

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* All page references are to the particular editions of works cited in the bibliography.

Chapter 1

1. I do find Goodman's views on sets difficult to reconcile with the rest of his philosophy. I discuss this problem in chapter 3.

2. Goodman (1978), p.2.

3. ibid., p.4.

4. James (1909), p.40.

5. Campbell (1976), p.108.n.2. Whether these views can be attributed to Quine himself is a difficult question. See appendix 1.

6. Clifford (1877), p.205.

7. Kennedy (1958), p.580.

8. See lecture 1 of his (1907).

9. See Goodman (1973), Chapter 2, section 1, 'On the Philosophic Conscience'.

10. Quoted by Perry (1935), Vol.2., p.27. My own way of defending this statement will be given in chapter 8.

11. See, for example, Reichenbach (1951), p.312 ff. Reichenbach there says, "The scientific philosopher does not want to belittle values... but he refuses to muddle emotion and cognition, and likes to breathe the pure air of logical insight and penetration." p.312. Compare that statement with this by James, "Pretend what we may, the whole man within us is at work when we form our philosophical opinions. Intellect, will, taste, and passion co-operate just as they do in practical affairs; and lucky it is if the passion be not something as petty as a love of personal conquest over the philosopher across the way...It is almost incredible that men who are themselves working philosophers should pretend that any philosophy can be, or ever has been, constructed without the help of personal preference, belief, or divinination." (1896), p.77.

12. Goodman (1976), p.127. Sessions (1962), p.31.

13. Quoted by Goodman (1976), p.33. See Gombrich (1960).

14. Goodman (1978), p.107.

15. Goodman (1976), p.264.

16. See Read (1960). Chapter 1 of this work is explicitly directed against Reichenbach's (1951). See Langer (1953) and the quotation from Otto Baensch which states, "...the function of art is not to give the percipent any kind of pleasure, however noble, but to acquaint him with something which he has not known." p.19.

17. Goodman (1976), p.248.

Chapter 1 (cont.)

18. ibid., p.264

19. Lakatos (1970), p.20.

20. Williams (1978), p.244.

21. James (1907), p.122.

22. id.

23. Goodman (1976), pp.7-8.

24. ibid., p.8

25. I use the terms 'framework', 'vocabulary' and 'purpose' to allow for the distinct emphases that occur in James' and Goodman's relativisms. The term that I employ to describe what it is that truth and ontology are relative to is 'linguistic context'. See chapter 2.

26. James (1912), p.136.

27. James (1907), p.96.

28. ibid., p.106.

29. In Perry (1935), Vol.2., p.475.

30. James (1907), p.97.

31. ibid., p.108.

32. James (1909), p.54.

33. Scheffler ((1974), p.113) uses a similar example to criticize James' relativism. Scheffler seems unable to make much sense of James' relativism at all but ironically I believe that the key to understanding James - the theory of internal justification - employs a central point made by Scheffler himself in his (1967). See chapter 2.

34. Goodman (1973), p.64.

35. A view for which there are a variety of reasons. See Haack (1979).

36. Goodman (1978), p.18

Chapter 2

1. Lewis, C.I. (1929), p.191.

2. ibid. p.37.

Chapter 2 (cont.)

3. id.

4. ibid., pp.38-9.

5. ibid., p.55.

6. ibid., p.66

7. ibid., p.49.

8. ibid., p.50.

9. ibid., p.52.

10. id.

11. ibid., p.53.

12. ibid., p.52.

13. Scheffler (1967), p.22. My discussion and criticism of Lewis is indebted to this work.

14. Scheffler, ibid. pp.22-3.

15. Scheffler, ibid. pp.37-8.

16. The phrase used by Williams in his (1978).

17. Kripke argues that the private language argument essentially arises from the 'rule-following considerations' preceding the passages which are normally taken to constitute the private language argument in the Philosophical Investigations.

18. Wittgenstein (1958), sec.124.

19. ibid., sec.202.

20. Putnam (1975a).

21. Quine (1951a), p.20.

22. ibid., p.38.

23. ibid., p.41.

24. id.

25. Hofstadter (1954), pp.409-10.

26. Mellor (1965), p.111.

27. See his (1975a).

Chapter 2 (cont.)

28. The choice between statements and sentences is sometimes linked to what some see as an opposition within the philosophy of language between an approach which takes semantics as fundamental, and an approach which takes pragmatics (the study of language in relation to its use or users) as fundamental. Strawson, for example, in his (1979), argues in favour of taking pragmatics as fundamental. My talk of 'linguistic contexts' rather cuts across this distinction since whilst my major focus of attention is semantic, linguistic contexts are partially individuated by their purpose. I am inclined to think that an approach which does not set up an opposition between semantics and pragmatics is desirable.

29. Recently, for example, by Grayling (1982), chapter 2.

30. Pitcher (1964), p.5.

31. Cargile (1978), p.105.

32. ibid., pp.105-6.

33. Quine (1951a), p.42.

34. Hofstadter, ibid., p.410.

35. In a letter published in Harding ((1976), p.132) Quine wrote, "...my holism is not as extreme as those brief vague paragraphs at the end of 'Two Dogmas of Empiricism' are bound to sound."

36. Quine (1951a), p.43.

37. Haack (1978a), p.234.

38. Lewis,C.I. (1923), p.365.

39. Similar views are developed by Goodman in his (1964) tribute to Lewis.

40. This is not proposed as a serious test since I have not defined who counts as a 'competent speaker'. In fact 'competence' might have appeared on my list of linguistic phenomena - a fact which, I believe, supports rather than casts doubt on my holistic approach.

41. McGinn (1976) in Platts (1980), p.19.

Chapter 3

1. Quine (1975c), p.293.

2. Russell once said that philosophy "...must make only such assertions as would be equally true however the actual world were constituted." (1914) in his (1953), p.107. Generally I think it would not be unreasonable to define the beginings of contemporary analytic

Chapter 3 (cont.)

as being identical with the rejection of the idea that philosophy can possibly have such a role independently of contingent experience. That view would be of a piece with the idea that logical positivism and pragmatism were the first movements of contemporary analytic philosophy.One might speculate that Russell's influence over Quine (especially via the theory of descriptions) might be responsible for Quine's somewhat ambivalent attitude to the idea that philosophy has an ontological role to play that is independent of science.

3. One other area of philosophy this doesn't fit is the philosophy of mind. My view here would be that philosophy can legitimately be concerned with both the philosophy of psychology and with the nature of everyday explanations of actions.

4. Ultimately it is for each philosopher and, more generally, each inquirer to decide what counts as a 'legitimate' inquiry. The community of inquirers will, however, impose constraints: a scientist whose view of scientific method is extremely bizarre will find himself unable to obtain research grants etc. (Sometimes, I suppose, the community may be 'wrong' and the individual 'right'). The difficulty in philosophy, as I said, is that what a philosopher will count as a reasonable approach to what questions is inextricably bound up with their substantive philosophical beliefs; in science, it would seem, there is a larger gap between methods and results.

5. I see an important difference between the so-called ontological argument for the existence of God and the 'argument from design'. I don't see how purely a-priori arguments can establish the existence of anything. The argument from design is, I think, acceptable in principle (feeble though I think it is) being a kind of induction based on experience. If one rejected the argument from design I think one would have to reject the argument from the problem of evil - and that seems to me a perfectly good argument.

6. Dewey (1929), p.309.

7. Alston (1958), pp.9-10.

8. Quine (1950), p.78.

9. Quine (1973), p.3.

10. Quine (1951a), p.45.

11. Campbell (1976), p.108n.

12. Cambell ibid., p.108.

13. Quine (1968b) in his (1968a), p.69.

14. In this section I have treated pluralism only in the context of ontological questions but ultimately much bigger issues are at stake. In Quine's (1951a) there is a passage which speaks of physical objects and Homer's gods where he says, "..in point of epistemological

Chapter 3 (cont.)

footing the physical objects and the gods differ only in degree and not in kind. Both sorts of entities enter our conception only as cultural posits." p.44. Whilst I welcome the epistemic liberalism of this passage there is, I think, a dangerous assumption that necessarily the 'cultural posit' of Homer's gods was intended to serve precisely the purpose that all cultural posits are intended to serve in our culture. An anthropologist has said recently that a mistake of earlier anthropology was to assume that what 'primitive people' are doing is what we are doing but doing it badly. (See Geertz (1983)). That, it seems to me, is the view that Quine implies in the passage referred to and the view explicitly, and unashamedly, adopted by Jarvie in his (1970). There are very considerable difficulties in classifying the beliefs of peoples of different cultures but if, for example, we accept some description of some peoples' beliefs as 'magical', I think that it is wrong to assume that if they are rational then they must at some time abandon those beliefs. I do incline to the view that so long as they stick to a magical belief system their explanations of the physical world and their metaphysics will be poorer than ours, but their art, system of socialization and their moral behaviour, say, may be better than ours. In a reasonable sense of 'rational' the rational thing to do is to develop the overall best culture but it is not necessarily the case that overall our culture is superiour to any culture with a magical belief system. There is a sense in which, if their actions were primarily motivated by their desire to preserve (or improve) their moral belief system, they would be choosing a non-scientific way of life - one which has a different motivation from that which has been behind a great deal of Western achievement. This is clearly a large and contentious topic but see chapter 8 and appendix 2 for discussion of related themes.

15. Hilbert (1925), p.376.

16. Goodman (1956), p.156.

17. Goodman ibid., p.158. Compare the tone of these remarks with my view stated above that the length of an inventory is irrelevant.

18. Haack (1978b), p.492.

19. Goodman (1956), p.168.

20. In certain respects it seems to me that my conclusions are not very different from those for which Carnap argued in his (1950). Carnap was, for instance, sceptical about the importance of the nominalist/realist debate. However, Carnap's way of reaching those conclusions depended upon his distinction between 'internal' and 'external' questions - a distinction which is, at best, unclear. (See Haack (1976b) for criticism).

Chapter 4

1. Quine (1960), p.224.
2. Mellor (1974).
3. Mellor ibid.p.167.
4. Quine op.cit., p.223.
5. See Davidson (1970), (1973) and (1974).
6. Quine (1973), pp.11-2.
7. Campbell (1976), pp.171-2.
8. This description of combinatorialism is due to Lycan (1979) in Loux (1979), pp.304-5.
9. See his (1965a), p.147 ff.
10. See Goodman (1973), Chapter 2.
11. Goodman has consistently and explicitly repudiated possible worlds. Perhaps his views, in general, fit better into my category of 'redescriptivism' but in that case the example he provided was misleading.
12. Rescher (1973) in Loux (1979), p.179.
13. Quine (1948), p.4.
14. Rescher (1973), p.177.
15. Lewis,D., (1973), p.84.
16. Hacking (1975), p.322.
17. This view is indebted to a paper by Morton and Mondadori, (1976) in Loux (1979).
18. Hacking employs 'L' and 'M' to represent what I have denoted by 'M' & 'M*'. I have changed the notation in order to avoid confusion with the use of 'L' for necessity in the standard modal logics.
19. Hacking (1975), p.323.
20. Hacking (1967), p.150.
21. Hacking (1975), p.325.
22. See references in note 5.
23. Reichenbach (1947), p.290.
24. See Morton and Mondadori (1976), p.247 ff.

Chapter 4 (cont.)

25. Morton and Mondadori (1976), pp.250-1.

26. There are, as Goodman points out in his (1951), problems about defining optimal conditions, "Exactly what is involved in ascribing ..a property to a thing has been the subject of some controversy. Some maintain that to say that a thing is carmine is to say that all presentations of the thing that occur under pure daylight and otherwise optimal conditions exhibit the quale carmine. This optimimum theory has some initial plausibility since it is under optimum conditions that the most acute distinctions can be made and a property most precisely determined...But there are serious difficulties. Optimum conditions may not always be uniquely determinable... A better theory has been proposed by C.I.Lewis. He holds that to ascribe a certain property to an object is in effect to describe the complete pattern of qualia (of the kind in question) exhibited under all sorts of conditions. This pattern theory meets the difficulties of the optimum theory but perhaps may err in the opposite direction...Perhaps the truth is that to apply any ordinary property predicate to a thing amounts to describing its appearance under those sorts of conditions that are regarded as critical or standard." pp.95-6. Although there are, therefore, questions of detail, what is important from my point of view is the general approach. This approach enables one to distinguish between, say, "...looks green" and "...is green" by reference to complex patterns of usage of those predicates in a language community. This approach only explains such distinctions relative to patterns of language usage within a language community - but that is all relativists require; absolutists, presumably, can't rest content with such an approach.

Chapter 5

1. See, for example, Dummett (1959), (1963), (1969), all in his (1978). All page references, unless otherwise stated, are to his (1978).

2. Dummett (1963), p.147.

3. ibid., p.148

4. (1969), pp. 362-3.

5. ibid., p.368.

6. id.

7. ibid., p.364.

8. (1973b), p.318.

9. (1969), p.362.

10. McDowell (1978) in Pettit and MacDonald (1978), p.129.

Chapter 5 (cont.)

11. ibid., p.128

12. Popper (1960), p.223

13. ibid., pp.224-5.

14. Tarski (1944) in Linsky (1952), p.34.

15. Popper (1960), p.224.

16. id.

17. Haack (1976a), p.325.

18. Popper (1960), p.225

19. McGinn (1976) in Platts (1980), p.19.

20. Platts (1979), p.238.

21. In his (1979) McGinn says, "The external world of material bodies and events has the characteristic of objectivity; i.e. it is to be conceived in an absolute way, as not owing its intrinsic nature to the relative and subjective sensory modalities and conscious experience of the sentient beings that inhabit it." p.131.

22. Popper (1970) in his (1972), p.46.

23. Popper (1971) in his (1972), p.328.

24. Davidson (1969), p.759.

25. Davidson (1977) in Platts (1980), p.133.

Chapter 6

1. This illusion was demonstrated on television by R.L.Gregory in the series "States of Mind", BBC television.

2. This phrase is Ziff's in his stimulating discussion of Quine's notion of stimulus meaning. See Ziff (1972), p.99.

3. See Carnap (1953) in Feigel and Brodbeck, pp.63-4.

4. Newton-Smith (1981), p.27.

5. Nagel (1960), p.131.

6. ibid., p.152.

7. ibid., pp.146-7.

Chapter 6 (cont.)

8. ibid., p.147.

9. ibid., p.151.

10. id.

11. Quine (1970b), p.179.

12. This was Dummett's reaction in his (1973a). There he wrote, "Quine's argument for indeterminacy...is based on the claim..that there can be empirically equivalent but logically incompatible theories. Even if this claim is granted, the argument is not compelling, since theories might diverge irreconcilably in internal structure: but the claim is absurd, because there could be nothing to prevent our attributing the apparent incompatibility to equivocation." p.617n.

13. Quine (1975b), p.313.

14. Quine ibid., p.318.

15. ibid., p.320.

16. ibid., p.327. Quine's way of saying that there are bound to be alternative, empirically equivalent, logically incompatible theories lends itself to a modal misunderstanding. If we speak of branching theories whenever there are alternative theories, a way of misunderstanding the thesis would be:
(1) It is unavoidable that there will be branching.
But what Quine intends is that the modal operator has smaller scope:
(2) It is possible that there will be unavoidable branching.
If the branching were avoidable the thesis would lose its theoretical interest (see pp.322-3). I think Newton-Smith failed to appreciate Quine's intention and was led to distinguish between strong and weak versions of the underdetermination thesis (roughly corresponding to 1 and 2 above) whereas it was only the latter Quine intended.

17. Newton-Smith (1978), p.71. He also mentions what he calls the 'causal ingredient' but adds that this is 'arguably not independent.'

18. Newton-Smith (1978), p.72.

19. Surprising in view of his avowed commitment to realism.

20. Quine (1975b), p.324.

21. ibid., p.327.

22. Putnam (1979), p.73. Later Putnam (1978, part 4) abandoned this form of realism.

23. See, for example, Putnam (1978); Newton-Smith (1978); R.Boyd (1980).

24. See Laudan (1981).

Chapter 6 (cont.)

25. Eddington (1944), pp.xi-xii.

26. Stebbing (1937), p.51.

27. See Feyerabend (1962); Maxwell (1968); Sellars (1961).

28. Mellor (1969).

29. ibid., p.182.

30. id.

31. Unger (1979), pp.237-8.

32. Ayer (1979), p.324.

33. Quine (1981a), p.36.

Chapter 7

1. Bourbaki (1968), p.316. 'N.Bourbaki' is the pseudonym for a group of French mathematicians who became active shortly before the second world war.

2. Gandy (1972).

3. Benacerraf (1965).

4. Resnik (1975).

5. Benacerraf op.cit., pp.54-5.

6. ibid. p.55.

7. ibid. p.56.

8. Church (1966) wrote, "...if a choice must in some sense be made among the rival set theories, rather than merely and neutrally to develop the mathematical consequences of the alternative theories, it seems that the only basis for it can be the same informal criterion of simplicity that governs the choice among rival physical theories when both or all of them equally explain the experimental facts." p.18

9. Quine (1968c), p.45.

10. Benacerraf op.cit., p.69.

11. ibid., p.70.

12. Resnik op.cit., p.36.

Chapter 7 (cont.)

13. Korner (1960), p.104.

14. ibid., p.60.

15. For interesting speculations on this see Goodstein (1949) and (1960).

16. In his (1884).

17. This is the subtitle of Kline's (1980) survey of modern mathematics.

18. Hilbert (1925), p.375.

19. Frege (1893), p.xxvi.

20. My discussion of logicism and formalism is indebted to Kleene (1952) chapter 3.

21. Whitehead and Russell (1925), Vol.1., p.xiv.

22. Weyl (1946), quoted by Kleene (1952), p.45.

23. Brouwer (1952), quoted by Körner (1960), p.122.

24. This dispute was reported by Heyting (1956) in Benacerraf and Putnam (1964), p.63.

25. Quoted by Kleene (1952), pp.54-5.

26. See Godel (1931).

27. The generalized version of Church's theorem states: there exists a predicate such that there is no correct formal system which contains a decision procedure for both the predicate and its negation. The generalized version of Skolem's theorem states: there is no consistent categorical formal system having the natural numbers as its intended interpretation. See De Long (1970), p.195 ff.

28. Kleene op.cit., p.57.

29. Péter (1957), p.215.

30. Mill (1843), pp.165-6.

31. Lakatos (1967), p.40.

32. Gandy (1972), p.139.

33. Quoted by Kleene op.cit., p.57.

34. See Kramer (1981), p.129.

35. Quine changed his view sometime after a paper written with Goodman (1947) which began with the sentence, "We do not believe in abstract entities."

Chapter 8

1. Frankena (1968), Vol.8., p.229.
2. See Stevenson (1944); Ayer (1971).
3. Sartre (1957).
4. Hume (1740), p.203.
5. id.
6. Pitcher (1965).
7. Hume op.cit., p.157.
8. Quine (1951a), p.42.
9. Hume op.cit., p.203.
10. See Foot (1958); Searle (1964), both in Foot (1967).
11. See MacIntyre (1981), pp.55 ff. My general approach to morality has a good deal in common with MacIntrye and with views expressed by Williams in his (1976) and (1981a) both in his (1981b); and with Hampshire in his (1978a) and (1978b) both in Hampshire (1978c).
12. This distinction is similar to one drawn by Cooper in his (1968) but I have altered the terminology and I use the distinction to different ends.
13. See Williams (1981b), p.2.
14. Kant (1785), Paton's translation (1948), pp.63-4.
15. See Wolf (1980), p.156.
16. Fried (1970), p.227.
17. Williams (1976), p.18.
18. Smart (1973), p.71
19. Pettit and MacDonald (1981), p.178.
20. Powell (1971), pp.144-5.
21. Midgeley (1978), pp.187-8.
22. Keats "Ode to a Grecian Urn".
23. Quoted by Perry (1935) vol.2., p.27.
24. Dickens (1854), p.1.

Chapter 8 (cont.)

25. The essential point of the argument is that there is no reason to expect the relation between mental states and neurophysiological states to be type-type. If, on the other hand, the relation is token-token then even knowledge of the covering laws couched in physical vocabulary need not produce knowledge of, or produce any, covering laws couched in mentalistic vocabulary. See Davidson (1970); Fodor (1975), chapter 1.

26. I am not alone in failing to understand this argument in Davidson's (1970). Nagel in his (1974) in his (1979), p.178n expresses the same incomprehension as have other philosophers in conversation.

27. See the argument in his (1980).

28. Skinner (1971).

29. See the arguments in the papers throughout his (1978).

30. Kekes (1980), p.108.

31. ibid., p.196.

32. Rorty (1982), p.90.

33. id.

Appendices

Appendix 1

1. One other major issue where there would seem to be unequivocal evidence that Quine has changed his views is that of fallibilism with respect to logic. In his (1951a) Quine argued that there was no difference in principle between a change in logic and a major change in scientific theory (See (1951a) p.43), but in his (1970a) he asks rhetorically "If sheer logic is not conclusive, what is ?" p.81. Quine's change of mind stems from considering the problem of translation.(See Haack (1974) chapter 1). It is arguable that the more conservative viewpoint I detect in Quine's attitude toward relativism is implied by the change of view over the epistemological status of logic for there is good reason, as Haack says, for supposing that acceptance of the view that there cannot be a real change of logic commits him to "...admiting a distinction between linguistic change and factual change.." (Haack (1974), p.15). Significantly, perhaps, this change of view can be traced roughly to his (1960) - which is the point where I detect a most significant change of emphasis in Quine's general outlook.

Appendix 1 (cont.)

2. Quine (1950), p.78.
3. id.
4. Quine (1951b), p.61.
5. Quine (1954), p.132.
6. Quine (1960), p.221.
7. Quine (1975c), p.303.
8. Quine (1960), p.24.
9. Quine (1975b), p.327.
10. Quine (1973), pp.3-4.
11. Smart (1969), p.3.
12. Quine (1975c), p.293.

Appendix 2

1. Davidson (1973b), p.19.
2. Rescher (1980) also seems to me make some telling points against Davidson's views but they are not the points I have raised here.

Appendix 3

1. See Church (1958); Scheffler and Chomsky (1958); Cartwright (1954).

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