# A BASIS FOR RELATIVISM

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Doctor of Philosophy.

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# SUMMARY

Taking it as a starting point that the notion of relativism is obscure, and that the ways in which the notion might be developed and used require clarification, this thesis aims to identify a basis on which relativistic theses are possible, and the way such theses might be developed on this basis, and some aspects of the effects of this approach on some standard philosophical issues.

On the basis of a preliminary, negative account of relativism, certain general objections to relativistic doctrines, and their implications for the enquiry at hand, are noted. It is argued that there is no empirical or moral basis upon which an account could be developed; but that certain logical, epistemological and linguistic theses, in particular those offered at times by P. Winch and J.R. Searle, do offer such a basis.

An account is offered of a notion of constitutive and interpretive rules, of "interpreted

games" as systems of such rules, of "ways of acting" as systems of such games, and of "forms of life" as systems of ways of acting. This structure is offered as allowing an account of certain matters, concerning reference and identification, and concerning communicability and commensurability, which have been of concern in the earlier discussion.

The basis of relativism is said to lie in the aspects of the proposed systems which are in certain senses arbitrary.

The structure proposed allows accounts of rationality and objectivity which accept their normal association with scientific contexts without supposing them necessarily to be so related; it allows an account of the "is-ought" gap which shows it arising from the same source as the distinction between observation and theory; and it stresses the relation between relativist theses and a concern with matter of form over matters of content.

This thesis is in the same area as that entitled _Language and Ontology_, which was accepted for the award of the degree of Master of Arts at Macquarie University in 1970. However, the identification and treatment of the problem differ, and no part of the former thesis is used here. The account of individuals used as a basis of discussion in the final section was part of an Honours essay which was accepted for the award of the degree of Bachelor of Arts at Monash University in 1968; it is used, however, as an example, and does not contribute to the present argument. Neither in these nor in other respects does the thesis contain material previously accepted for any award; nor does it contain, to the best of my knowledge and belief, any material previously published or written by any other person without due acknowledgement.

I consent to the thesis being made available for photocopying and loan if it is accepted for the award of the degree.

## INTRODUCTION

'Relativism' is at least as difficult a term as any other in philosophy, suffering as it does from an embarassing profusion of uses. No statement in which 'relativism' appears is open for discussion until it is tied down to some context in which the use of the term is explained. It was claimed, at one stage of the mind-body debate, that while it was clear what central-state materialists asserted and what they denied, it was less clear that their opponents asserted what the materialists denied, or precisely what their claim to deny what the materialists asserted came to. In the discussion of relativism, the case is worse; it is not always clear what is denied on either side, that the other side is prepared to assert.

This may be because 'relativism' collects a variety of positions. It can be found qualified as ontological, linguistic, conceptual, epistemic, cultural, moral and so on; it is not always clear what proponents of such positions have in common. Indeed, the use of 'relativism' seems to serve less to identify a particular thesis or family of theses, than to mark the intersection area of a number of debates. There are well-established areas of debate concerning such matters as scepticism and the regress of reasons;

criticism and justification; rationality and commitment; meaning and truth; belief, action and knowledge; metalanguage and metaphysics; value freedom, fact-value distinctions and the is-ought gap; dogmatism and the onus of proof. Most of these areas have been discussed as though central in the system of related questions; different solutions have been offered, at times involving a substantial reinterpretation of some area of enquiry. Of the emergent more or less schematic over-views of the complex of problems, some have been regarded as being relativistic, more or less. At least, by their opponents.

Some sketch of this spread is shown in the opening passages of a paper by Steven Lukes. He says that "Relativism has had a considerable vogue in recent times, and many thinkers in different fields have, in varying degrees, yielded to its temptations"[1]. He goes on to mention Quine, Wittgenstein, Winch, Phillips, Kuhn, Feyerabend, Sapir, Whorf, Levy-Bruhl and so on, showing the occurrence of relativistic theses in the philosophy of language, social science, religion, science, knowledge, and in linguistics and social anthropology. If what these authors have in common is their yielding to the siren song of relativism, the traces are not easy to find. And it does not help to turn to the recent work of a self- declared radical

relativist, and find that his foreword opens with this passage.

> This book does not run a straight course from beginning to end. It hunts; and in the hunting, it sometimes worries the same raccoon in different trees, or different raccoons in the same tree, or even what turns out to be no raccoon in any tree.... it counts not the kill but what is learned of the territory explored.[2]

Lukes apparently takes the common factor to be an affirmative answer to some question like: ".. are truth and logic, morality, even rationality itself, ultimately context- or culture- or theory-dependent, relative to particular and irreducibly various 'forms of life' or systems of thought?".[3] But even past the problems of complex question that this way of putting the matter raises, there is a question how the, perhaps irreducibly various, answers are to be understood.

Lukes finds it "... striking that few relativists seem able, in the end, to take the theory-dependence of their worlds, and the pluralistic social solipsism it entails, really seriously";[4] and he goes on to record apparent disavowals from some of those he has previously quoted. He might have gone further, noting points like Winch's explicit disavowal at least of crude Protagorean relativism, or Quine's espousal, in the early pages of "Ontological Relativism", of a Deweyan ",. naturalistic view of language and ..

behavioral view of meaning"[5], which might lead to relativism of a sort, but certainly not the sort which, say, Winch is held to support. Hence, when someone speaks generally of relativism - when, say, Popper remarks that it is an objectionable thesis because it leads to war[6] - there is no simple answer to the question what claim is being made.

In the following, I attempt to develop notions of relativism which will be useful across the range of areas of debate mentioned without gross equivocation, and which resist arguments of the kind commonly used to anti-relativist ends. My hope that in so doing, I shall not merely add one more thread to the tangle that already exists, rests on a procedure. I set out, not to define 'relativism', but to identify a source of relativistic theses, and to develop that source position so as to show in what way, and in what form, relativistic theses may emerge from it.

In Part I, I am concerned to identify, and find guidance for the development of, the sort of source position I require. I generally proceed by first discussing a position, then discussing at least one criticism of that position, in order to see what form of further development the position suggests, and what modifications would permit it to support my purpose.

Thus in Chapter 1 I introduce a preliminary, "sight-setting" account of relativism as a negative position, and discuss certain general anti-relativist arguments which at that stage can be brought to bear. In Chapter 2, I offer reasons for thinking that a theory of meaning and of knowledge of the kind proposed by Peter Winch might serve my purpose; and base further discussion first upon Winch's presentation, and then upon criticism of this position offered by John Kekes.

The potential which I suggest is to be found in a Winchian notion of forms of life, or some development thereof, I take to require for its fulfilment a more detailed account of the structure of such a unit. I seek a basis for such an account in two stages: first, I introduce the notion of a "way of acting", based upon an account of the nature and significance of discontinuity in justificatory regresses, and consider objections to the intended function of this notion as expressed by Israel Scheffler; second, to provide a basis for an account of the structure of ways of acting, I consider the distinction between regulative and constitutive rules drawn by John Searle, and certain criticisms and modifications to this proposed by J.R. Ransdell.

Upon the basis of this discussion, using the notions it has proposed, and upon the ground which I

trust it has to some extent cleared, I proceed in Part II to construct an account of forms of life as systems of systems of systems of rules. In Chapter 4 I discuss the form and function of the rules which I take as the basis of my account; in Chapter 5 I present a notion of formal and interpreted games as systems of rules; in Chapter 6 I present a notion of ways of acting as systems of interpreted games; in Chapter 7 I present a notion of forms of life as systems of ways of acting; and in Section 21, I discuss the sense and the extent to which such a position permits relativist theses.

In Part III, my concern is to explore some aspects of the position which Part II is intended to present. In Chapter 8, I consider the implications of my position for aspects of the concern with rationality and objectivity which is so constant a characteristic of those adopting ant-relativist stances. In Chapter 9, I take up the particular example - the bridging of the is-ought gap - with which Searle had been concerned in the context of a theory of the sort I have offered, and argue that it can be seen not as a special sort of case, but as an instance of a perfectly general problem, in the terms of my theory. In Chapter 10, I indulge a purely speculative urge to stretch the notion of a significant alternative world or language or conceptual scheme as far as - perhaps

even a little further than - my thesis will support it. I suggest that this indulgence allows one to see, not so much that with which my thesis is primarily concerned - the accounts of meaning structures which allow significant theses properly called relativistic to be formed - but the kinds of concern which are conducive to the formation of such theses: the kinds of concern which encourage us to think that the realist and the idealist are "... equally delightful and equally deplorable - after all, the difference between them is purely conventional!"[7], as Goodman put it.

Later, defending himself against the claim that he was confused in his use of 'convention' as opposed to 'content', he said, of the view he had put on the distinction:

> Perhaps the most straightforward way of putting it is that as the distinction between what is due to discourse and what is not flickers out, so does the significance of the issue between realism and idealism; and perhaps I should have put it that simply. But after a time, one becomes somewhat weary and even distrustful of flatfooted philosophy.[8]

Throughout this essay, I am concerned with some version or other of a distinction between convention and content. It does not, I think, flicker out; I hope that I move it to illuminate the weave in the fabric of our discourse and our lives in another way.

# PART I.

A basis for relativistic theses is possible.

CHAPTER 1 A negative definition of relativism leaves the way open to meet certain prima facie objections, and directs attention to the basis of relativistic theses.

SECTION 1 A position of naive absolutism can be defined; and forms of relativism can be defined upon certain objections to this position.

If the uses of 'relativism' are too diverse, then a more useful use must be prescribed. The risk of thus coming to examine an insignificant artefact, while interesting notions are neglected, is less than it may seem. Relativisms may be many; but the aspects of their areas of application which give rise to relativistic theses are fewer.

Consider, for example, how small from one point of view is the shift, in Steven Lukes's discussion, from cognitive to moral relativism. In a part of his negative argument against cognitive relativism, he says:

> ... the influence, however deep, of theories, systems, paradigms, perspectives and so on upon men's perceptions and understanding is one thing: the relativist claim that there are no theory-independent objects of perception and understanding is another. Similarly, the influence of theories upon what men may count as valid and consistent is one thing; the relativist claim that validity and consistency are theory-dependent is another.[1]

When he turns to moral relativism, the argument is paraphrased as saying that the "... influence of moral codes, ethical systems, ways of life, etc. on men's actual moral judgments and actions is one thing; the claim that there are no correct moral judgments or objectively right actions is another".[2] That the cases, arguments and conclusions may differ in the two areas is for my present purpose less important than the fact that the concern is in both cases with the same things: the relation between the world and our perceptions of or judgments about it, and the basis upon which perceptions and judgments are properly accepted.

Despite differences in style, much the same interest can be seen in Goodman. He says

> We cannot test a world-version by comparing it with a world undescribed, undepicted, unperceived .... While we may speak of determining what versions are right as 'learning about the world', 'the world' supposedly being that which all right versions describe, all we learn about the world is contained in right versions of it; and while the underlying world, bereft of these, need not be denied to those who love it, it is perhaps on the whole a world well lost.[3]

Again, with 'versions' substituting for 'perceptions' and 'judgments' - and for an aspect of 'actions' - there is the concern with the relation between versions and the world, and with what establishes a right version.

I shall attempt to identify, in a preliminary way, a notion of relativism which draws its use from

these interests, and is based not on the assertion, but on the denial of a position. This procedure does not identify a clear relativist thesis, but this is not a serious disadvantage, since my purpose is to show how a thesis might be developed, rather than to state and defend one. Given this purpose, I hope to gain from the negative procedure proposed two advantages.

First, I shall gain some degree of immunity from certain general objections to relativist theses which I shall consider in the next section. These arguments offer a *prima facie*, and in some cases clearly powerful case for supposing that the enterprise of developing relativist theses can be seen to be doomed before even it is begun, and so require early acknowledgement. However, the account of relativism which I think evades their force cannot be stated without the basis which I offer in Part II. I therefore employ a negative definition which does not commit me prematurely on the issues upon which these arguments turn, but nevertheless gives some standing in the field of debate.

Second, I focus attention first on lines of argument rather than statements of position, since the position the denial of which is to identify a notion of relativism is not itself important. Crucial lines of argument bearing on the interests mentioned above, and,

I shall suggest, arising from problems of justification and certain notions of forms of life, can be shown, at least in a preliminary way, to be significant through this process of negative definition. Thus this process can provide a basis for the further process of developing a relativist thesis.

I call the view of which relativism is a denial naive absolutism; and I define naive absolutism as the view constituted by the following claims.

1. Something is independently the case.
2. Something that is independently the case is expressible.
3. Something that is expressible is knowable.

These claims are deliberately very general: proponents of a variety of philosophical views could accept them. The notion of naive realism traditionally used in discussions of perception is a specific version of naive absolutism, in which the existence of physical objects is part of 1.; but a naive absolutist would not have to be a realist in this sense: a phenomenalist might be a naive absolutist, as might a scientific realist. I call the position naive only to stress the fact that I have no interest in criticisms of it which might be met by a more sophisticated version, but only in criticisms which are radical in their effect.

I take the traditional account of knowledge as justified true belief, and a correspondence theory of truth, to be part of naive absolutism. Thus, 1. above may be regarded as the assertion that the world has a determinate character, that it has this character regardless of the character it is taken to have, and would have it in most respects in the absence of any sentient beings; 2. as the assertion that we are able to say things that are true in that they correspond to the way the world is; and 3. as the assertion that we can find out which of the things we say are true. On this interpretation, we can know things only if we can say things that are true, and what we say can be true only if there is something to which it corresponds: i.e., low-numbered claims are necessary conditions of high-numbered claims, and thus high-numbered claims are sufficient conditions of low-numbered claims. Thus the naive absolutist would say, quite properly, that his knowledge depended upon what was independently the case in the world; but his belief in the independence of the world rests on his belief in the existence of knowledge, rather than the other way around.

This logical structure affects the effects of selective denial of the constitutive claims. To deny 1. is to withdraw the necessary condition of 2. and 3. in their intended interpretations: it is to deny naive

absolutism absolutely, and to require a reinterpretation of 2. and 3. which would at least include a reconsideration of correspondence theory of truth. To deny 3. has no such immediate and dramatic effects: 1. and 2. can be retained in their intended interpretation, though not on the same ground. However, it is one effect of the abandonment of faith in knowledge as justified true belief that beliefs would have to be collected on some basis other than their being known to be true; and I shall later suggest that the effects of this change are more radical than is sometimes supposed. The denial of 2. would require reinterpretation of 3., and leave 1. ungrounded. This sounds radical, but in fact is not particularly interesting. The unsupported 1. merely manages to assert the existence of a "something I know not what" as the ineffable cause of the knowledge claimed in 3. - the world bereft of versions which might as well be lost as not. The new sense forced upon 3. will at least have in common with that forced by the denial of 1. the rejection of a correspondence theory of truth, and probably more; generally, one might say, the effect of denying that language expresses independent facts is not likely to differ significantly from that of denying that there are such facts to express.

The interesting possibilities, then, are those involving the denial of 1. or 3.; first, however, one

must see that there are certain difficulties in the idea of a denial of 1.: "It is not the case that something is independently the case" is at best unperspicuous. One might qualify 'independently', making it, say, 'independently of perception by a spirit'. One could then say that Berkeley denied 1.; but this would leave the existence of spirits, at least, to satisfy 1.. One could produce a similar effect by qualifying 'something' as 'something material'; but although this does go against naive realism, and therefore against a possible use of 1., it misses the point. 1. does not say, of any particular kind of thing, that it is independent. It is not contradicted by saying that something of a certain sort is not independently the case, but by saying that nothing is; and this is not an entirely easy claim to understand.

There is, of course, an implicit qualification on 'independently', which is explicit in the paraphrase: roughly, 'independently of being taken to be the case'. That is, 1. might be reexpressed as: something is the case, whether it is taken to be so or not. But I am not sure that this form of 1. is more readily denied than the original, though it may be more perspicuously expressed. The notion of something's being the case is very near the rock-bottom of intelligibility. The attempt to deny it must be false. It is not that 'Nothing is the case' is self-contradictory; it is

rather that if it is true it is false, and therefore it is false. To say that the statement has been made is to falsify it.

However, on this account, the force of 1. is not that of a statement about the nature of reality, but of a determination to hold open the possibility of error as to what is the case. Thus the difficulty of denying 1. arises not from its stating an incontrovertible fact, but from its being a rule of grammar, in the sense in which Wittgenstein suggested that 'You can't feel my pain' is a rule of grammar. We must maintain a gap between something's being the case and something's being taken to be the case in order to maintain our notions of assertion and of knowledge claims: it is in this sense that 1. states necessary conditions for 2. and 3..

Now, while this line of argument does not lead to a denial of 1., it does change the intended sense. 1. was intended as a statement about the nature of reality, and not about the nature of a language. To deny 1. its intended sense presents the naive absolutist with effectively the same problem as a denial of the truth of 1. in its intended sense: he still has to reinterpret 2. and 3.. Thus the line of argument provides what is effectively a denial of 1., and does so by disturbing the naive absolutist assumption of a

simple correspondence relation between judgments and the world. By relocating what was intended as an extra-linguistic reference point within language, it questions the assumed possibility of recourse from language, or of transcending language in the way required to check claims against reality. It thus opens just those questions about the relation of versions and the world which Goodman thought were answered by a radical relativism.

I shall call the virtual denial, or, in other terms, the radical reinterpretation of 1., *linguistic relativism*. I do so not to preserve a connection with any thesis about the differences between natural languages, but as a reminder that the basic thrust of the argument was to turn what were taken as extra-linguistic matters into linguistic ones.

I shall digress briefly at this point to explain why I put aside concern with natural languages. While it may turn out that differences between natural languages reflect differences significant for linguistic relativism, it is not necessary that this should be so: distinct natural languages are quite acceptable to a naive absolutist. However, their differences must be subject to a translatability requirement, which arises in the following way. All languages must include provision for stating what is the case in the same world. After all due allowance has been made for differences

between the users of different languages which might affect translation, such as differences in the nature and sophistication of theories, or in ideology, or in aesthetic attitudes, there remains a body of statements in each language which is related to a body of statements in any other because each corresponds, or fails to correspond, to the same thing. No doubt there are languages into which 'The cat is on the mat' could not be translated because the language has no word referring to cats; but to suppose that there was a language into which no statement of that sort could be translated, or in which no term referring to cats could be introduced even when a speaker of the language had been shown a cat, would be either to suppose, contrary to hypothesis, that the languages were used in different worlds, or that the status of one as a language was in doubt. Crudely, one might say that anything that can't be translated into any part of my language at all isn't a language at all. This suggests that naive absolutism may involve some notion of observation language. This would not be surprising. Naive absolutism involves faith in the possibility of knowledge; and traditionally observation languages have expressed knowledge claims: "This is a human hand", for instance.

Linguistic relativism is thus concerned with the relation between language and the world; it will

have a primary concern with theory of meaning, which it will invest with ontological significance in a way which I will take to have been most interestingly exhibited by Peter Winch.

If the effects of denying 3. are, as I suggest, more important than they seem, the process of denial at least is simple, and readily illustrated. A sceptical appeal to the regress of reasons, together with common arguments to the conclusion that the regress is not satisfactorily arrested by appeal to experience, intuition and so on, offers a perfectly general denial of 3. A Popperian falsification theory, to the extent that it rejected the notion of a distinguishable observation language, would also involve the denial of 3., substituting a notion of the way in which we might improve the stock of statements about the world by a systematic process of weeding out.

Such processes of denial, whether they lead to stories about commitment, or to notions of critical rationalism, yield positions which might be called relativist in that they deny, or in the case of such criticalist theories as that of W.W. Bartley,[4] offer no satisfactory account of, the resolvability in principle of differences about the nature of the world which naive absolutism requires. I shall call the denial of 3. epistemic relativism. Epistemic relativism has its

basis in the second of the general interests I mentioned above, which might be generally represented as an interest in problems of justification.

I have now two loose notions of relativism, each of which offers an area of enquiry, and a direction for the enquiry to take. I can consider what reasons there might be for denying our ability to transcend language, and thus for some version of what I have called linguistic relativism; I can consider what reasons there might be for supposing there to be a reasonable notion of justification which we cannot satisfy, and thus for some version of what I have called epistemic relativism. It is not at the moment to be supposed that the enquiry will support any particular positive theses under these names, or even that it will support this kind of distinction; the account of language which shows our inability to transcend it may also show the limits of justification. It is important to have a start and direction for the enquiry; but I think it is necessary to travel hopefully in order to see what might count as an arrival.

SECTION 2 Further indications of direction for this enquiry can be derived from a consideration of certain highly general objections to relativist theses.

Before proceeding to develop the enquiry, there are two patterns of objection to this movement towards relativism which might be considered, even at this very early stage. One, which might be called the incomprehensibility argument, is directed against linguistic relativism, and suggests that no position of this kind can be intelligibly stated. The other, an antisceptical argument, attempts to arrest justificationary regress in a way that does not leave beliefs unfounded, or founded only relative to some arbitrary commitment which opens the way for relativism, and can be seen, at least in the first place, as opposed to epistemic relativism. Either, if completely successful, would, at least apparently, stop further movement in these directions, and negate even this preliminary effort to define a possible notion of relativism.

As a first example of an incomprehensibility argument, showing a common way in which the problem is stated, and also showing something of the difficulty that can be met in deciding just who is contradicting what statements by whom, I shall look at some aspects of

Donald Davidson's comments on conceptual schemes.[1] He sets out to argue that we cannot make good sense of conceptual relativism, at least as an exciting doctrine.[2] Since I have declared the intention of using Winch as a prototypical exponent of linguistic relativism, since 'conceptual relativism' has been applied to Winch's position, and since that position in some respects resembles the sort of view Davidson discusses, this looks like a clear opposition. Davidson also accepts "... the doctrine which associates having a language with having a conceptual scheme",[3] which appears to forge a further link, through my use of 'linguistic relativism' and my comments on natural languages in the previous section. Yet there are confusing features.

He says that he follows Quine on the basic evidence for a theory of radical interpretation;[4] but Quine on this sort of basis generated views which are quoted by Davidson as examples of unsatisfactory doctrines.[5] Not impossible, of course; but in the context of Davidson's paper it is appropriate to wonder whether the adjustment he makes to Quine's statements will involve a "... reinterpretation of words in order to preserve a reasonable theory of belief",[6] and what the consequences of choice might be.

Next, Colin McGinn, in an argument directed

against an aspect of Davidson's discussion,[7] quotes Davidson as expressing the principle that taking an object in the world to be the subject matter of a man's belief involves taking it that he has many other true beliefs about that object, which he takes to be analogous to a condition on semantic reference. The notion, following from these principles, that the ancients, in thinking the earth to be flat, did not think it about this earth, has an odd sound when taken in the context of Davidson on conceptual schemes; certainly, McGinn's account of how, with the assistance of causal theories, we can "... impute preponderantly false beliefs to the ancients in relation to the earth and the stars without the implication that they believed these things of those entities as they are conceptualized by us",[8] is more consistent with naive absolutism than is Davidson's position.

Of course, Davidson's position in the comments referred to is entirely consistent with a major part of the thrust of his discussion of conceptual schemes: his attack on ".. the dogma of a dualism of scheme and reality". With such a dualism, he says,

> we get conceptual relativity, and truth relative to a scheme. Without the dogma, this kind of relativity goes by the board. Of course truth of sentences remains relative to language, but that is as objective as can be. In giving up the dualism of scheme and world, we do not give up the world, but reestablish unmediated touch with the familiar objects whos antics make our sentences

and opinions true or false.[9]

But for the naive absolutist, the dualism of language and the world was the defence against, not the occasion for, conceptual relativism. His pleasure in not giving up the world will be less when he finds that his unmediated access to it is not to be distinguished from his unmediated access to language; and his suspicions will not be allayed when he is told that "... if we cannot intelligibly say that schemes are different, neither can we intelligibly say that they are one".[10] He will be even less consoled when he realises that the question whether Davidson's concepts of or beliefs about the world and language were radically different from his depends upon the outcome of a debate on the principles of radical translation.

Of course, not everyone who would deny naive absolutism must be a relativist. I wish only at the moment to say that whether a denial of naive absolutism is relativist or not is not always a clear one: it is just not clear whether it is Davidson and the naive absolutist, or Davidson and some sort of relativist, who are, as Davidson says Kuhn's scientists may be, "like those who need Webster's dictionary, .. only words apart".[11]

Words matter in another way: a way that is of great importance because it affects the way in which the

issues are identified. In the quotation above, we found Davidson saying that truth remained relative to a language, though not to a scheme; which might remind us of his willingness to identify these notions quoted earlier. On this point he says:

> We may accept the doctrine that associates having a language with having a conceptual scheme. The relation may be supposed to be this: if conceptual schemes differ, so do languages. But speakers of different languages may share a conceptual scheme provided there is a way of translating one language into the other. Studying the criteria of translation is therefore a way of focussing on criteria of identity for conceptual schemes.[12]

The point is repeated a little later.

> We may identify conceptual schemes with languages, then, or better, allowing for the possibility that more than one language may express the same scheme, sets of intertranslatable languages....Can we then say that two people have different conceptual schemes if they speak languages that fail of intertranslatability?[13]

One reason for not attempting to argue that Davidson makes truth both relative and not relative to the same thing is that 'language' is equivocal. The main sense of 'language' seems to be that of 'natural language'. Davidson mentions English and Hopi as cases of languages for which failure of intertranslatability has been claimed; and it is a natural and congenial notion that different languages may express the same scheme, resembling the view to be mentioned below that sentences, possibly in different languages,

may express the same proposition. Then, to find that someone has a different conceptual scheme is to find that they have a language that I can't translate. Perhaps this is why so much discussion in this area has kept close to anthropology - it allows stories about translation difficulties, if not about total failure.

But while foreign languages may be handy examples, foreigness of language is not the point. Quine, who made great use of this approach through translation, reminded us that this was so. He said "... the resort to a remote language was not really essential. On deeper reflection, radical translation begins at home. Must we equate our neighbour's English words with the same strings of phonemes in our own mouths?"[14]; and his answer is that we need not. Now, if we try to make this point in the ordinary sense of 'language', we have to say that the same language may express different schemes; and then what do we make of the notion of intertranslatability between English and English, and how do we conceive of its failing?

If we do not begin by assuming that there cannot be different conceptual schemes, and we agree that languages differ when conceptual schemes do, then we cannot take it that 'English' names a language, for it may name a set of non-intertranslatable languages.

And if 'English' does not name a language, how do we understand the use of 'language' when Davidson speaks of a set of intertranslatable languages; and if we lose our grip on 'language', how do we understand 'translation'?

I suggest that we must either retain the notion of language as natural language, in which case the invoked notions of language and translation beg an important question, or we think of 'language' as having a sense closer to that of 'conceptual scheme' than to that of 'natural language', in which case criteria of translation will need to be illuminated by criteria of identity for conceptual schemes, rather than the other way around. I wish to stress that, in any case, there is reason for doubt about the apparently innocuous business of expressing the issues of relativism in terms of translation.

A similar pattern occurs in comments on relativism by W.H. Newton-Smith.[15] He takes relativism to be the view that something can be true in one context and false in another, particular social groups or theories being examples of such contexts. He argues that if the something is a sentence, relativism is trivial, for of course sentences may have different meanings in different contexts, and thus different truth values; while if it

is the proposition expressed by a sentence, then it is necessary for sentences to express the same proposition that they have the same truth values, so that relativism is incoherent.[16] He then says that his point

> can be put in terms of translation as follows. If I am able to find a sentence '$S_1$' in my language which translates a sentence '$S_2$' of another language, I cannot accept it as logically possible that these sentences should differ as to truth value. If I find myself attributing different truth values to the sentences I have to revise my view that one is a translation of the other. Thus this attempt to have a thesis of relativity ... fails because it is incoherent.[17]

He adds that we cannot recover by supposing ".. that there might be sentences that cannot be rendered in a meaning equivalent fashion within our own language", since in this case nothing would be true in one context and false in another.[18]

Let us first notice a difficulty for his first piece of argument. Winch mentions, as different modes of life, science and religion,[19] a comment which provides the basis of an example which I discuss at some length in the next chapter, and in Part II. Consider a sentence identifying God as the creator of the universe, which is pronounced true by someone in a religious context,and by someone in a scientific context pronounced false on the ground that the use of 'God' is evidence of the multiplication of entities beyond necessity. Do we say that the sentence has different meanings in these

contexts? The question is difficult: following something like Davidson's account of propositional attitudes, we might want to say that the two speakers differed in belief enough to be thought not to be talking about the same thing, so that the sentences differed at least in extension; on the other hand, we might want to maintain that the scientific view was intended to deny exactly what the religious view asserted. No doubt there are trivial cases; but the question whether a sentence has the same meaning on two occasions of its use is not always trivial.

To shift the question to the context of concern with translation is, for Newton-Smith, just to ask about truth conditions. If the sentence has the same truth conditions in both contexts, then it can only be that one speaker is mistaken. If the truth conditions are different, then we don't have relativism after all. This merely shifts the difficulty. Are the truth-conditions the same in both cases? Suppose we write out the two sets of truth conditions: do we ask if each contains the same sentences? Well, no; better to ask whether each set expresses the same propositions, or is a translation or a meaning-equivalent rendering of the other. But this is no advance. Problems are not removed by facile tautology-juggling.

The pattern of argument depends greatly upon the importance attributed to meaning equivalence. The claim runs: ".. the assumption that we can identify .. diversity in beliefs presupposes that we can identify sameness of meaning in sentences across .. differing perspectives ....If we were to make the radical assumption that we cannot recognize diversity .... we cannot even formulate a non-trivial thesis of relativism".[20] There is, I shall suggest below, something correct in this; but there is also something wrong. To some extent this claim anticipates my later argument; but some difficulty which I think arises from the use of 'translation' can be pointed out now.

Translation, especially the radical sort which interests a philosopher like Quine, is a way of coming to understand a new symbol system by mapping it on to one already known. Success requires that at least some sentences of each system must be meaning equivalent; and one might argue that all could be, or could be made so: it is perhaps in this mood that Davidson points out how good Whorf is at rendering the content of Hopi sentences in English, and Kuhn at describing a pre-revolutionary era in post-revolutionary idiom.[21] But this is entirely the wrong model for questions about religion and science.

The protagonists of these positions may be supposed to share a natural language, and to understand

each other's beliefs perfectly well. There is much to be said for the view that we could not develop any interesting relativist thesis if this were not so. But this does not imply that there must be some recognizably scientific statement which is meaning-equivalent to 'God made heaven and earth', or a religious version of '$e=mc^2$'. The protagonists differ, and we could not make this point if they could not, even in principle, understand each other; but their understanding each other is not a matter of their constructing meaning-equivalent pairs of sentences, as it might be if they differed as speakers of English differ from speakers of French. Nor can we argue that difference implies understanding, and understanding is a product of translation, and translation requires meaning equivalence, and meaning equivalence requires identity of truth conditions, hence the only ways of differing that are relevant are those involving difference over truth value.

Part of the trouble here is the same equivocation over 'language' that I claimed to find in Davidson. Newton-Smith considers the relativization of truth to contexts, and he refers to these contexts at one point using 'theory', then offers as analogous remarks in which 'language' does this job. But his notions of translation are more suited to the contexts of natural

languages.

I do not regard these attempts to show the incomprehensibility of relativism as successful; but they are related to an argument about the incomprehensibility of alternatives which is more interesting; it is close to the principle, mentioned by Davidson, that ".. nothing .. could count as evidence that some form of activity could not be interpreted in our language that was not at the same time evidence that that form of activity was not speech behaviour".[22] One version of the argument is presented, though not well developed, by Roger Trigg, in discussing the view that reality notions are relative to languages. He argues that on this account, different languages require different worlds; but that we must suppose that people of different communities see the world similarly in order to suppose their languages to be intertranslatable.[23]

There is an important point here, which Trigg goes on to misrepresent as the point that unresolved disagreement leaves the relativist/objectivist controversy wide open. But this is feeble. By an earlier argument of Trigg's, disagreement, whether resolved or not, shows only that linguistic relativism cannot be relevant. One cannot disagree with the non-translatable statements of a being from a different world. Indeed, it is not clear what reason we could have for supposing

that a statement had been made, even if, from the point of view of this hypothetical ultimate foreigner, it had.

The sharp edge of the problem may be lost when thought of translation helps to lend a quasi-anthropological tendency to the discussion. Thus, Trigg quotes D. Z. Phillips as saying that he cannot condemn child sacrifice in some remote tribe as he could condemn the murder of a child at home, for he would not know what it was that he condemned. I think it is interesting that this doubt over the object of an attitude is more or less that expressed by the anti-relativist Davidson; but Trigg is concerned rather to argue that

> If the whole argument is removed from the individual level to the level of conceptual systems, it may be said that a condemnation of other systems is merely a reiteration of our collective commitment to our system. The argument then becomes another version of the accusation of begging the question, which ... is itself merely an assertion of conceptual relativism.[24]

But again he misses the point. Consider an example based on Shaw's discussion in his preface to "Saint Joan". An atheist would disagree with the Inquisition about the existence of souls or the rules of the spiritual universe; but he could see that, given the beliefs of the Inquisition on these matters, any amount of bodily suffering would be infinitely less than

the suffering of a soul condemned for eternity, and that this was a consideration relevant to the rights and duties of the Inquisition. In this case someone might want to say, of the atheist or the Inquisitor, that he merely reiterated his commitment to his own system, provided there was deemed to be no non-question-begging way of establishing the correctness of one or the other. But of course the atheist *would* know what the practices of the Inquisition meant to its members. *Ex hypothesi*, he does not know what they mean in the sense of accepting them, but they must be comprehensible to him. He does not suppose that any concepts are peculiar to either side, but only that they disagree, concerning concepts which both have available, as to which of them are applicable.

This, the situation supposed by Trigg, can at most be epistemic relativism: the atheist and Inquisitor may be held to face a question which is in principle not resolvable. For linguistic relativism, understood as the theory which Trigg attributes to Winch, we must further hold that practices, and the concepts applied to them, and the world in which they are adopted and discussed, are in some sense an indissoluble whole, so that the atheist and the Inquisitor have no significant concepts in common. The main complaint about this is that it renders obscure everything said so far.

If the atheist does not have the relevant concepts in common with the Inquisitors, he never found out what they did. When he thought he was finding out what they believed, he was actually attributing a set of beliefs to people to whom they were incomprehensible. He did not discover a different conceptual scheme, for the only concepts he understood were his own. So the attempt to get rid of a common objective reality would not provide a new account of difference and dispute, for the discovery that a situation involves difference and dispute is itself evidence that the situation is not one to which linguistic relativism applies.

Though this argument seems to me powerful, I do not think it is so final as I have tried to make it appear. It serves to force a demand for clarification on linguistic relativism, but it will be weakened insofar as the demand is met; and there are ways in which its edge as an anti-relativist argument can be blunted, at least at this stage of the discussion.

One of its assumptions is that linguistic relativism involves commitment to a view of language as an indissoluble whole, so that any difference is a total difference. One can note that even when Davidson sets out to consider partial failure of translatability, he discusses principles of radical interpretation which

are quite general in their application. While it is true that linguistic relativism owes an account of how this commitment might be avoided, it is also true that the commitment is not clearly established. It does not follow from the claim that "Something is independently the case" is a rule of grammar, or the more or less similar claim that Trigg attributes to Winch, that different languages must determine reality differently in every respect. An account of language which showed possibilities for limited difference in determination of reality would be significant; and will be more readily achieved without notions of translation based upon the relations between natural languages.

Moreover, even if linguistic relativism does entail the incommensurability and mutual incomprehensibility of different languages, it does not follow that it has no value. We are accustomed to hear it said that psychologically satisfying models of modern physics to some extent misrepresent, like a classroom model that shows an atom as a congeries of little lumps and connecting wires. But we do not therefore say that such models are useless. Given that their limitations are recognised, they can be put to good use. Similarly, even if linguistic relativism turned out to permit no more than that models of alternative languages could be constructed, and these in some respects misleading, it

would not follow that they were of no use at all.

Neither of these lines of defence is in any way final. Rather, they represent the acceptance of an obligation to provide an account of a language which shows how they can be used. If such an account is not forthcoming, then I think that linguistic relativism must fall to an argument of the general kind that I have suggested.

Epistemic relativism, as I have presented it, denies the possibility of establishing which of our beliefs are true. This essentially sceptical thesis becomes relativist by arguing the necessity of commitment, as follows.

We do, as a matter of fact, take statements to be true or false, and further take it that, at least in some cases, we get it right. Even if we do not do so in any absolute sense, it would be odd to suppose that we did not succeed in drawing any distinction at all. We establish statements as true or false not absolutely, but relative to some body of belief taken for granted, or self-evident. Since no body of belief sufficient to establish contingent truth or falsity can be self-evident, it must be taken for granted. In turn, to establish the truth of such a body of belief can only be to show that it is true relative to some further

body of belief, taken for granted in <u>that</u> argument. Hence our knowledge claims, though proper enough in the sense that they have all the grounding possible, must rest on some body of belief which is unjustifiable, but to which we are committed. Since more than one internally consistent body of belief may be taken for granted, ultimately irreconcilable differences are possible.

This line of argument assumes that any claim is liable to challenge for justification, and employs a kind of methodological scepticism to show that the demand for justification outruns the supply. Against it, one could claim to find irrefutable answers which stopped the sceptical regress; one could argue that scepticism used to generate a regress of reasons was improper; one could deny that justification was always due. I shall ignore the first, taking it that there are no claims adequate to support contingent beliefs that are beyond sceptical challenge. The second I think is interesting only when it is a consequence of the third; but sometimes, operating alone, it may reveal a situation which is to be expected if something like epistemic relativism is correct. Thus Patrick Corbett, while apparently keeping some place for justification, treats the sceptic as a mischievous child, whose gadfly persistence may be stopped by

hoisting him with his own petard.[25] Corbett does not make clear just when and how we are entitled to use this ploy; but I think that there is an answer, and suggest something of it in the next chapter. For the present, however, it is the third counter-argument which matters, for if it is correct, the others are redundant.

One exponent of such a view is W.W. Bartley, in The Retreat to Commitment. Impressed by Popperian disconfirmation, and noting in passing that political advance becomes easier when we stop trying to get good rulers and turn to deciding how to get rid of bad ones, he suggests that we should not regard claims as established by the degree to which they are justified, but by the degree to which they are open to criticism. That is, we should not seek to establish claims at all, since this requires authoritarian notions of justification and opens the way to a sceptical regress. Rather, we should hold all claims tentatively, not in the sense that we feel doubt or hesitate to act, but in the sense that we recognise their openness to challenge. Of course, "Why do you believe that?" would not be a proper challenge; something on the order of "That is false, because ..." would be required.[26] An exponent of Bartley's comprehensively critical rationalism (CCR), is ".. one who holds all his beliefs, including his most fundamental standards and his basic philosophical position itself, open to criticism."[27] He might be supposed to say'"Some of the

beliefs I hold may in fact be true; but since there are no guarantees or criteria of truth, no way of definitely deciding, I can never know for sure whether what I believe to be true is in fact so"'.[28]

The first thing to note is that this is an effective criticism of justificationary approaches only if CCR works. The general discussion shows only that acceptance of an obligation to justify, together with some other premisses, commits one to limitations on rationality which permit relativism based on arbitrariness. Since this is very much the view towards which I am proceeding, I would not accept it as a reason for adopting a non-justificationary approach. But if CCR works it shows that rationality is unlimited; and this would be a reason for abandoning justificationism. It is part of the problem that the rationality which is unlimited is based on a notion of criticism which is said to be separated from the notion of justification, and this might cause difficulty; but if CCR does not work, this problem is not pressing.

That it does not work is argued by J.W.N. Watkins.[29] He argues that it is the kind of "Heads-I-win-tails-you-lose" doctrine that Bartley himself condemned, and that ".. in one sentence: in support of the claim that CCR is criticisable we are challenged to _criticise_ it in a certain way - namely, by trying to

show that it is uncriticisable!".[30] I accept this view, not only because of Watkins's argument, but because it suggests evidence that Bartley has not escaped the problems of regress. Watkins has argued that a cogent argument for the uncriticisability of CCR would only show that CCR is criticisable, and thus that CCR is only trivially criticisable. He then suggests that someone might argue that by showing that no "level-1" criticisms could succeed, he had offered a strong "level-2" criticism of CCR, so that CCR is really criticisable, if only at a metalevel. He regards this as establishing the criticisability of CCR only in a Pickwickian sense.[31] He does not notice, or does not see fit to mention, that the pattern recurs on level 2. His comment on Pickwickian senses suggests an argument against level-2 criticism. If this argument succeeds, it shows that CCR is genuinely criticisable on level 2, and he must move up another level to make this point. It seems to me that the possibility of a real challenge keeps one step ahead here, in rather the way the possibility of a real justification keeps one step ahead: the problems of regress have been shifted, not eliminated.

Even if this line averts the immediate danger to justificationism, it is not illuminating. Some further comments may be. Bartley's disavowal of certainty is very like that which I attributed to the epistemic relativist; the difference is that the

relativist, being a justificationist, takes the choice between theories to be arbitrary when the theories are unjustifiable; Bartley takes the decision to be arbitrary only between ".. two theories against which there exist no criticisms one is unable to defeat".[32]

First, the notion of justification from which Bartley claims to have separated the notion of criticism is an absolute one: a claim is absolutely justified only when it is shown to follow from a basis of unchallengable authority. One could define a weaker, relative notion of justification: a claim is justified relative to a basis from which it can be inferred. This is an important sense because the regress of justification breaks under sceptical pressure at the point where no relative justification is available; the question of absolute justification may arise, but need not. And this point might as well be described as that at which no significant criticisms can be raised. In terms of Bartley's own examples: an empiricist such as Hume did not see our claims to know what our sense experience was as lacking justification, but as subject to no conceivable criticism; Descartes did not accept the _cogito_ because he could not justify it, but because he thought that no significant criticism of it was possible.

The point is that which Bartley himself makes in stressing the importance of logic. That p entails q is indifferent to matters of contingent truth or

philosophers' interest. Depending upon whether we affirm or deny p and q, the implication can be used either to justify (in the relative sense) or to criticise either p or q. Thus criticism and relative justification are not separable notions: in recognising something as a criticism of a position, we recognise what would have gone to its relative justification. Providing that Bartley is not applying 'unjustifiable' to theories as it applies to behaviour - as a term of condemnation - he must suppose the justificationist to lapse into commitment not just because of some general despair over absolute justification, but because, and at the point at which, no relative justification can be found. He then owes an account of what it can come to, at such a point, to be open to the criticism of which no conception can be formed. After all, though no doubt Hume and Descartes were convinced, had anyone managed to invent a "decisive criticism", we have no reason to suppose that they would have been too stubborn to listen, or too stupid to understand. And what more could a comprehensively critical rationalist do?

This line of argument suggests that not only is the justificationist basis preserved, but that the best of Bartley's argument may serve to improve rather than refute it. However, it might seem to involve too cavalier a treatment of Bartley's notion of openness to

criticism, about which he did have something to say. The thought is worth following, not because it helps Bartley, but because it raises a point of importance for my enquiry.

For some proposition/belief/hypothesis/theory/ principle/way of life/ etc. P, (Bartley does not discriminate systematically), to criticise P is to show its inconsistency with some Q. Obviously it would be too easy if we could refute P by taking 'Q' as not-P; there must be some conditions on values for 'Q'. Bartley suggests two. The first is that we accept Q. The second is that Q may be either an account of logical consistency, or the deliverances of sense observation, or a scientific theory, or the statement that P solves the problem it was intended to solve, or something else. The second is a permissive condition, and could not be otherwise, since any putative value for 'Q', or the statement that a list of values was correct, would in turn have to be open to criticism.

This opens a new possibility of regress. In defending any P, I invoke some not-Q. I argue rationally from not-Q only if I am prepared, on some other occasion, to have it criticised; and just because I have used not-Q as a premiss, I should perhaps make special efforts to criticise it. In this regress, new issues emerge.

Suppose I claim, as P, that my car is in good running order. I would justify this, generally, by appeal to a body of factual belief, mechanical theory, principles of goodness in motor cars, and a principle of sufficient evidence which removed the necessity of, say, driving the car continuously in order to prove it was still in order. An ordinary criticism of P would deny some part of this. Suppose that I consider this criticism and reject it. P' now becomes this body of belief, theory and principle, to which is opposed some Q. I can take this Q to be a more general body of belief, theory and principle; but first, there are new problems arising from the fact that defending explanatory theories and defending principles of behaviour or action are not the same sort of process, and the notion of criticism and defence is now not only complex, but problematic. Still, suppose that I deal with this somehow, produce a highly general not-Q, and on this basis maintain P. To save time, suppose that this not-Q, re-named P, consists of full data, received scientific theory, and the most general principles of behaviour. *Ex hypothesi*, not-Q could not consist of yet more general items; it would have to offer reasons for receiving that as data, that as theory, and that notion of satisfactory behaviour. Again, the problem is not just complexity. It is that the nature of the justificatory task, and the critical task, has changed again: we have to ask what kind of Q

will do. The point of interest in all this is its role as corrective to the impression that a justificatory regress is a simple succession of steps of more or less the same kind, complicated only by branching as premisses multiply. It is this impression, I think, that allows the notion that the sceptic can be stopped whenever we tire of him, or that no justification short of absolute justification is of interest. If the regress is complex, the task of diagnosis is not adequately recognised by Bartley.

It might be suggested that the problem is gratuitous; that even though Bartley allowed for the criticism of ways of life, he did not mean it all to be done at once. But sorting out the strands only makes the problem sharper. I might have modified the above account to cover just a regress of explanation, ending in a general scientific theory, and all the process conducted on the principle that scientific theories were proper values for Q. But this principle, too, must be criticisable; and neither P nor Q can include scientific principles on pain of circularity.

At first, this looks easy. We are invited, in effect, to criticise the adoption of science. It might be criticised on the ground that it tends on the whole to decrease human happiness, which would be better served

by the consolations of religion, or on the ground that it mistakes the world for a mechanical system, when in fact it is an animistic system better manipulated through magic. These criticisms seem to invoke the test of the problem, the first saying science does not solve the problem of how to make people happy, the second that it does not solve the problem of what the world is really like. This is not apparently circular.

But then: how do we decide whether science makes people less happy? To attempt to determine what would have been the present state of satisfaction on the hypothesis that humanity had abjured science would be to attempt to predict in a scientific mode, and one cannot employ the mode which is under criticism. On the other hand, to suggest that God would reward a proper display of faith, in the manner of C.S. Lewis in Perelandra, is no better. And it is not clear what other way of deciding there might be. And for the second criticism, in which the description of the problem explicitly rules science out, the problem is even clearer. If the only criticisms we can find are, like these, invitations to circularity, it is not clear what our claim to remain open to criticism amounts to. Certainly, it is not clear that there is any major point that epistemic relativism misses.

So, although the lines of argument I have been considering raise significant issues, they do not constitute a case for rejecting either of the largely negative accounts of relativism I have suggested. They create the need for answers to particular questions. Perhaps for linguistic relativism, the most pressing is the question how an alternative language can be discussed, or a situation in which an alternative language is said to operate can be recognised; for epistemic relativism, the most pressing question might turn out to be how it can be prevented from turning into linguistic relativism, for the discussion of the last few pages has suggested that it is much less the simple sceptical response to difficulties than it might seem.

However, the aim of this section was merely to deal with positions which might seem to stop my journey before it began. The beginning requires a consideration of the kind of reason which might serve to support a relativist thesis.

CHAPTER 2 There is a possible basis for relativism in certain theories of knowledge and meaning.

SECTION 3 There is no adequate empirical or moral basis for relativism.

Discussion in the previous chapter has involved, though it has not considered in any detail, what might be called logical and linguistic grounds for relativism. I also noted a tendency, arising from the use of remote communities and new natural languages as a basis of discussion, to argue as though it was upon some aspect of these communities and languages that our notions of relativism depended. So, before logical and linguistic bases are considered in some detail, it is well to dismiss the claims of what I shall call empirical and moral grounds. Although these grounds overlap in various ways, it is at least possible to distinguish between logical and linguistic grounds on the one hand, and empirical and moral grounds on the other, on the basis that while the former, to the extent that they are sound, make some form of relativism necessary, the latter, to the extent that they are sound, establish a form of relativism either as an hypothesis or as a duty.

The attempt to found a version of relativism

on more or less empirical grounds involves the claim that observation of distinguishable, and perhaps preferably previously isolated communities, leads to the discovery that there are differences between communities which might be called differences in "world-view" or "form of life". A world-view or form of life is certainly supposed to include religious and normative beliefs and principles; and, to the extent that this ought to be regarded as something different, beliefs held about the regulation of the natural world, such as beliefs in science or magic. It may be said that members of different communities "see the world differently", or "live in different worlds".

This is not just the commonsense view that we can have an observational basis for thinking that a community holds different beliefs from ours, but the view that those beliefs are true in that community, so that our observations offer us either a view into another world, or at least allow us to see that another world exists.

Such a view presupposes a sharp separation between observational and theoretical language, since the observations that, in the first place, must be made by an observer operating in his own world will subsequently provide the basis either for his knowledge of the world

the observed community inhabits, or for his knowledge of the existence of such a world.

Although the account I offer in Part II supports the separation of relatively observational and theoretical languages, it does not support so radical a separation as some aspects of this story suggest, nor does it support the reductionist epistemology which seems to be implicit in other aspects. Hence it is important in the development of my account to see the kinds of difficulty which arise on this contrary assumption.

Now, it is *prima facie* odd to base the claim that an observer cannot observe the same world as members of a community on his observations of that community: it seems to lead to just the sort of paradox of which relativism is so often accused. It is certainly possible to observe differences in behaviour, and to find reasonable grounds for saying that members of a community judge by standards and in the light of beliefs different from those of the observer. But the fact of disagreement is not in itself an argument for relativism, since it might as reasonably provide occasion for attempts at the rational resolution of disagreement, for imputations of ignorance and moral deficiency, or, where highly general systems of belief were in question, for something like the

foundation of missionary societies. Indeed, such observations could lead directly to relativism only if they counted as observations of a "different world" - if the observer saw the community's behaviour as taking place in the community's world rather than his own - and it is not easy to understand what such an observation would be like.

Perhaps he could "get on the inside" - become, in effect, a member of the community - but although this may be possible, it does not seem consistent with his retaining the status of outside observer, and this brings problems. If he can observe the community's behaviour as taking place in the community's world only when he becomes a member, and thus ceases to be an outside observer, he cannot have an observational base for relativism. If he lacked such a base _qua_ member of his original society, he must lack it _qua_ member of the community he set out to observe. Only if we suppose him to keep a foot in both worlds has he grounds for relativism, as opposed to the experience of conversion; but this requires that he be both community member and outside observer, which does not appear to be possible.

Hence such observations as he makes, and offers as grounds for relativism, must be observations that he

can make as a member of his original community. The differences which he wishes to accomodate by an account of different worlds must be differences detectable in *his* world, such that a "different worlds" hypothesis is at least a plausible account of these differences; as against, say, suggestions that the community observed is primitive, or ignorant, or pre-scientific, or pre-logical, or immoral, or heathen, or whatever. It is therefore reasonable to ask what kinds of observable difference are possible.

The answer is disappointing. Differences that the observer can see as arising merely from difference in environment would not do; and differences interpretable as arising from differences in opinion will not do for the reasons mentioned above. Differences interpretable as differences in some basic attitude, say, to human life or personal survival may look more hopeful; but these are explicable as a result of training, and we need not explain them relativistically unless we bring some prior, possibly moral, concern into the situation.

Indeed, this is another point, in addition to those mentioned in Chapter 1, at which we may wonder whether social-anthropological travellers' tales may not serve to beg questions, since they may lead us,

either from a feeling that such oddity must reflect a fundamental difference or from a kind of Western-imperialist guilt complex, to snatch at relativistic accounts in these cases, which would have been rejected as accounting for differences to be found in our own back yard. It is perhaps some support for this view if it can be found that those telling travellers' tales with a relativistic bias show signs of absolutism at home; and this can be found, at least in the opinion of I.C. Jarvie and Joseph Agassi.[1]

The observational differences so far suggested are readily explicable. To some extent, their explicability is guaranteed by the required common understanding in other areas. Not only do they not require even moral relativism, but it has been suggested that full understanding of beliefs held in the community on such questions of the nature of man, and human life and after-life, may serve to explain observable differences without the need of either moral relativism or moral error.[2] Could the case be otherwise? Could there be kinds of observation which suggested differences which were inexplicable in any terms available to the observer, and thus seemed to support a more strongly relativist approach? Could there be observational grounds for supposing there to be a community inhabiting the sort of self-contained world, incommensurable with our own, that linguistic relativism might be held to propose?

Perhaps there would be such a case were there no common ground at all between the observer and members of the community, so that everything that happened in the community was incomprehensible; but this suggestion is of no real help. This is not because of the initial implausibility of the suggestion that nothing was common - surely, after all, we could expect to find Winch's triumvirate of birth, copulation and death - though the points are connected. It is rather that some common ground is presupposed in the claim that a community is being observed. Since in general we anticipate finding communities only of humans, and nothing could be a human community if birth, copulation and death had no place in it, Winch's triumvirate is relevant; but this is contingent. Nothing rules out the possibility of non-human communities, nor do I think we must reject the notion of a non-human community the members of which were not involved with birth, copulation or death. A community of robots, the members of which were, in the normal run of things, immortal, does not seem out of the question.

We should, however, at least have to be able to recognise some transactions between members of the community as "human-like" in some sense. What this sense is I am not quite sure; but there are at least two strong candidates for criteria of "human-likeness":

the use of language, and moral concern. It seems clear that if nothing in the observed situation looked like, or was interpretable as, the use of language, we would not be willing to regard any components of the situation as members of a community. About moral concern I am less sure. What I intend by 'moral concern' here is that there should be reason to suppose that in at least some cases the individual or collective interests of other members of the community counted as reasons for a member in deciding on a course of action. These are quite liberal criteria - they allow that we might consider the question whether there are non-human communities within our present experience - but there is no obvious reason to expect that further observations would not yield to explanations of the kinds suggested above.

We might postulate observations of a kind that would be inexplicable even when the observer and community had much in common. The discovery that members of the community could discriminate between qualitatively identical objects, for instance, or that they could discriminate on the basis of common differences in impossible situations - say, in terms of colour when unable to see - would not be explicable in terms of any accepted theory, attributable to the observer, of which I am aware. Bundling such observations together under a title like "psi phenomena" is not helpful, unless

the title indicates the existence of a reasonably well-developed theory. In a similar class would be cases of successful magic. We already have a name for this, not to mention a variety of theories; but these theories we might assume would be unacceptable to a scientifically-oriented observer.

This sort of thing ought, one would suppose, to fit the "different worlds" story. Magic works in their world, though not in ours; and we can see that this is so because we can "see into" their world a little, even though we cannot get far enough in to work magic ourselves. But such observations, though radical enough in their way, do not support any sort of incommensurability thesis. Paradoxically, this view can be maintained only if these recalcitrant phenomena can be explained without too much disruption of the observer's world view. In this case it can happily be said that the phenomena which are thus explained are distinct from those produced by magic, and any genuine comparison between world views denied. But should acceptance of the phenomena lead to changes in the observer's world-view - should they, however described, become equivalent to the acceptance of magic - then incommensurability cannot be claimed, since it is only too clear that there has been conflict between two world-views, and one of them has won. It would then become incumbent on the relativist to invent the adjustment which

the observer might have made in order to preserve both the observer's world-view, and the relativist thesis.

One recourse is to deny the phenomena. As a matter of commonplace, there are plenty of accounts, if only travellers' tales, which purport to be observations of magic, or indeed, of almost anything else one might wish. No doubt many of these can be dismissed on investigation, and it is at least likely that others would be similar, although investigation is not possible. Still, where cases are common, or where the process of investigation is such as to be itself liable to error, or where the assumption of error, deception or illusion is too freely employed, one might feel that there remains a case to answer. It might seem that this will do for the relativist: all he need do in order to preserve his thesis is suppose that reports of difficult phenomena need not be accepted. But this doesn't help. Even in the case where a report is dismissed on no better ground than that there must have been deception or illusion involved, there is the implicit admission that _if_ the report had been true there would have been a problem, and this is all that is needed to overthrow the suggestion that world-views are self-contained to such a degree that they cannot impinge on each other at all.

It may be more to the point to deny the

possibility of recalcitrant phenomena than the truth of claims to have observed them. To say that no handsome prince has ever turned into a frog, if this is just to say that no such metamorphosis has ever been observed, is implicitly to accept that such observations would have to be accounted for under the given description, and thus to rule out the case as a possible ground for linguistic relativism. But to say that princes cannot be turned into frogs, if this records a determination to treat any putative observations of such metamorphoses as something else, say, hallucinations, and putative reports of such observations as misdescriptions, is a very different matter.

This does appear to be the sort of case in which the phenomena do not force any revision of the observer's world-view, yet are taken quite differently in the world-view of the community he observes; and to the extent that this alternative world-view can be said to be understood by the observer, it may seem that he has a possible basis for linguistic relativism. This may be part of the point of Horton's remarks, when he says:

> Why does the scientist reject the magician's view of words? One easy answer is that he has come to know better: magical behaviour has been found not to produce the results it claims to. Perhaps. But what scientist has ever bothered to put magic to the test? The answer is, none; because there are deeper grounds for rejection - grounds which

> make the idea of testing beside the point. To see what these grounds are, let us return to the scientist's basic predicament - to his awareness of alternative idea-systems whose ways of classifying and interpreting the world are very different from his own. Now this changed awareness gives him two intellectual possibilities. Both are eminently thinkable; but one is intolerable, the other hopeful.[3]

To reject the magical world-view is to reject certain ways of classifying and interpreting the world; in effect, to rule that certain putative identifications of phenomena are inadmissable. One can then observe that these identifications are issued and accepted in the community under examination, without being thereby committed to the occurrence of phenomena thus identified, and hence claim observational evidence for alternative world-views. Whether the existence of alternative world-views, thus established, is support for relativism is another matter.

According to Horton, the first intellectual possibility, the continuance of the magical world-view, is intolerable because "... it means that the world is in the last analysis dependent on human whim, that the search for order is a folly, and that human beings can expect to find no sort of anchor in reality".[4] One may escape from this through faith in some constant reality to "... the modern view of words and reality as independent variables"[5] - presumably, the sort of view that leads Horton to speak of his protagonist as

"the scientist". Of these alternatives, Horton says: "Intellectually, this second possibility is neither more nor less respectable than the first. But it has the great advantage of being tolerable whilst the first is horrific"[6].

Clearly this claim, to the extent that it expresses a relativist view, does not do so on the basis of observation: neither the observation, in the sense permitted by the first alternative, of magical events, nor the observation, in the sense permitted by the second alternative, that events are identified in the community in magical terms, could support the conclusion that the alternatives were not distinguishable in terms of intellectual respectability. Moreover, there seems to be a suggestion that if the scientist went to the trouble of putting magic to the test, he might find that he did know better after all, and his predicament would be dissolved.

But how would one put magic to the test? To adopt Horton's first alternative, with its horrific implications, is to embrace magic; to adopt the more hopeful second alternative is to repudiate magic. Neither alternative could properly be called a test, and no other alternative offers. Could we now say: a kind of relativism is forced upon us by the existence of

incompatible world-views which we cannot put to the test, therefore observational evidence for the existence of incompatible world-views is observational evidence for the existence of a kind of relativism? To this question there are several replies.

First, the claim that members of the community identify phenomena in magical terms requires translation of their utterances, and is thus the product of a complex net of assumption, comparison and inference rather than of observation.

Second, the claim that the world-view their utterances reveal is incompatible with that of the observer, if this means that they identify phenomena in inadmissable terms, would support claims of faulty translation, or of an unusually high incidence of delusion or mass hallucination at least as readily as any relativist thesis.

Third, it is not clear that the scientist could not put magic to the test in some sense: in the sense, perhaps, that modern scientific medicine can put traditional folk arts of healing to the test, finding in some of them merit independent of their explanatory embroidery. Generally, this story does not identify an observational base, nor does it show that the base it

does identify supports a relativistic conclusion in preference to any other.

In sum, it seems that neither the origin nor the justification of relativist theories is to be explained as resting on empirical observation. Even if it were accepted, contrary to some of the arguments above, that the existence of incompatible world-views could be established on such a basis, this would not lend support to a relativist thesis; indeed, quite the contrary, since one would be constrained, in the view one took of the newly discovered alternative, by the canons of the process of discovery under which it was found and identified. This, of course, does not show that relativist theories must be whimsical or unjustified; it shows merely that their origin and justification must be sought in fields other than that of empirical enquiry, and in views other than those concerning the diversity of human societies.

One might put this point more strongly. Any argument that can be offered for relativism must be one that holds even in the event that no more than one world-view has ever been held; for the task of testing and justifying that world-view in isolation, or against alternatives that are merely possible, must, if relativism is correct, be as impossible as the task of

showing its superiority to another world-view which is actually held. At most, one could say that there would be less obvious occasion to develop relativist theories if there were no systematic disagreement; but this is not to the point.

That relativism cannot be read off the face of the world as a simple fact is not surprising, since if it were so, the kind of relativism I have called linguistic would be true only if there were multiple worlds in a most direct and simple sense: we should have to be able to move at a step from the commonplace world into fairy-land, and ignore the change only at the cost of the most perverse distortion of evidence. And what the world would have to be like in order to offer an observational base for epistemic relativism is even less clear. If we explain differences relativistically, it is because we have a theory, of a very general sort, about man, or the world, or cultures, or knowledge, which obliges us to accomodate the facts of cultural difference in a particular way. One must then ask: what kind of theory could this be?

One sort of candidate, which has been common in some areas of recent writing on education, appears to be moral theory. Unlike the views mentioned above, which present moral claims as conclusions from relativist premisses, these writings appear to draw relativist

conclusions from moral premisses. Thus Esland, in a paper offering educational recommendations, begins by describing a position, or class of positions, as "objectivist epistemology".[7] His sharpest criticism of this position - though perhaps not the sharpest that might be made - is that it is hard not to see it as dehumanizing. As a basis for his further remarks, he describes a presumably non-objectivist epistemology, using notions like the intersubjective negotiation of meanings, and freedom from external facticities, which is intended to escape this criticism.

It is perhaps positions of this sort that Gellner refers to as anthropomorphism - as the ".. plea for a humanist psychology or sociology", or in this case educational theory and epistemology. It does appear to fit in the category of ".. positive, moral-order-preserving anthropomorphic doctrines".[8]

The detail of Esland's comment is of no particular interest, and has been dissected by Flew.[9] The general pattern of argument, in which an epistemological conclusion appears to follow from a moral premiss, is. On the face of it, the case is implausible; that it would be in a more or less moral sense more satisfactory if one epistemology were sound and another were not does not seem compelling. One looks for an appropriate

variation of Gellner's comment that

> the autonomy of ethics only followed on the autonomy of science, the exclusion of the argument from morals to fact, from 'ought' to 'is', of the form 'This must be true, otherwise our life would not make sense', or 'This cannot be true, otherwise our life would make no sense'. 10

Of course, it might be a development of an argument from epistemology to morals. If an epistemological position O entailed the falsity of a moral position M, then the truth of M would entail the falsity of O, and we might even be able to offer some account of an alternative epistemology which would be consistent with M; and perhaps this is what Esland thinks he is showing. But there are problems in this pattern. I shall later argue that epistemological positions do not have moral consequences. I shall then be concerned with some alleged consequences of relativist positions, but the point holds generally.

But even if the entailment were thought to hold, one would still have to show why M was better grounded than O, and this, given the nature of O as objectivist, raises difficulties. M cannot be defended on grounds which presuppose objectivism; but it is not entirely clear what the force of non-objectivist arguments for M could be. Presumably, if I were an adherent of O, and it were Esland's purpose to persuade

me that M, he would have to proceed by an "intersubjective negotiation of meanings". Even if I knew what this meant, I suspect that I would be uncertain of how negotiations were to proceed; and why, in the course of these negotiations, I should give up the meanings in which I accepted O in order to have it shown that M.

I do not think that, generally, arguments from moral positions, classifiable by Gellner as positive anthropomorphism, are significant. In order to take them seriously in a philosophical context, one must convert them to arguments of a different kind: Gellner's negative or defensive, epistemologically based anthropomorphisms, which argue "... that the very nature of knowledge, in the sphere in question, is such that no non-anthropomorphic theory can possibly be true".[11] It was such a process of conversion which led even the casual comments on Esland above to focus on his notion of meanings. So we need, not a moral theory, but a theory of knowledge and meaning. Gellner's example of a proponent of a theory of the right sort is Peter Winch, and I shall follow him in considering the theory developed in Winch's writings. It may seem at least insensitive to take, as an example leading to relativism, one who is on record as denying imputations of relativism. However, Winch's work provides such a useful example, and his denial of relativism is so equivocal, that I feel justified in so taking him.

SECTION 4 Winch offers an epistemological basis for relativism in an account of forms of life in which the notion of identification is central.

Since in my subsequent discussion, the account Winch gives of forms of life is substantially modified, a relatively brief acknowledgment of a debt to Winch's more or less Wittgensteinian position might suffice for my central purpose. However, there is a reason analogous to that offered for the discussion in Section 3 for giving Winch's views more space.

On his account, the relation between observational and theoretical language is, at least in the case of human action and social relations, so very tight as to make outside observation of the activities of a community impossible; and some of his uses of 'form of life' seem to suggest that the possibility of observing the activities of a community, resting as it does on the possibility of correctly identifying them, presupposes something like a participatory understanding of the total life of the community.

Although the account I offer in Part II includes an account of the relation between relatively

observational and theoretical language, it does not support so close a relation as this, nor does it support the view that the total life of a community, or its language, must be intelligible as a whole or not at all. Hence it is important in the development of my account to see the kinds of difficulty which arise on the contrary view.

Winch's position is complex, and one has a choice of points of entry, and lines to follow. I shall first trace a short path which is not obviously favourable to relativism, then consider whether this path adequately represents his position. I hope to show in this way the significance in his story of notions of identification, and of the difference between the constitution and the operation of a form of life; and also to indicate the directions in which attempts to clarify the notion of a form of life might proceed.

Winch proposes a difference between physical and social sciences. This difference is related to a difference between their respective relations to philosophy. He says:

> Whereas the scientist investigates the nature, causes and effects of *particular* real things and processes, the philosopher is concerned with the nature of reality as such and in general .... the sense in which the philosopher asks 'What is real?' involves the problem of man's relation to reality, which takes us beyond pure science.[1]

But this is the physical scientist: for the social scientist the case is different. In the light of Wittgenstein's remark that it is forms of life that have to be accepted as the given, he suggests that

> .. whereas the philosophies of science, of art, of history, etc., will have the task of elucidating the peculiar natures of those forms of life called 'science', 'art', etc., epistemology will try to elucidate what is involved in the notion of a form of life as such.[2]

This has consequences for the social sciences, since ".. the central problem of sociology, that of giving an account of the nature of social phenomena in general, itself belongs to philosophy".[3] This is because ".. the social relations between men and the ideas men's actions embody are really the same thing considered from different points of view",[4] while in the case of physical relations between objects,

> ... although human beings can think of the occurrences in question only in terms of the concepts they do in fact have of them, yet the events themselves have an existence independent of these concepts. There existed electrical storms and thunder long before there were human beings to form concepts of them or establish that there was any connection between them. But it does not make sense to suppose that human beings might have been issuing commands and obeying them before they came to form the concept of command and obedience.[5]

So we might say, of some visiting extra-terrestrial anthropologists, that we need have no reason to suppose that they must get their account of

electrical storms and thunder on earth wrong, even if the concepts they in fact had were in some way different from ours; but we could expect error in their account of human society if they did not bring to it the right concepts of, say, command and obedience. Suppose that they had no such concepts: that each of them acted always only for his own reasons, and that expressions which sounded to us like commands were understood by them only as invitations. When they observed a situation in which a sergeant called "Eyes right!", and his men all turned their eyes to the right, they would describe this as a situation in which an invitation was unanimously accepted, and perhaps speculate on the common interest felt by all the men in some object to their right, and the reasons they might have for thinking the sergeant's judgment of the interest-value of this object to be reliable. In doing so, they would go wrong in two related ways. They would attribute to the sergeant and the men beliefs and interests which they probably did not hold, and would leave out of their description the social relation which was in that case of central importance.

Now, there is no obvious impetus to relativism here; but this is perhaps because, if the point of this line of argument is to make it clear that the extra-terrestrials face a different task in their description of human activity from that which they face in their

description of the physical features of Earth, the difference consisting in a requirement in the first case that they should operate with human concepts rather than with their own, the point is not conclusively made. They are in error, but it is not clear that they could not correct the error by further observations in their own terms. They might note that we engage in practices like locking people up, or withholding goods from them, and that in some cases these practices are related to declined invitations. They might observe that there are signs by which this category of invitations can be identified, such as the uniforms worn by the sergeant and his men. They might then speculate that the known consequences of declining invitations in this category constitute, for the majority of those receiving them, a sufficiently strong reason for acceptance to override any other interests they might have at the time. It would then be much less clear that they were wrong in either of the ways previously suggested, even if it were only much later that they noticed the correlation between the acceptance of invitations in this category, and our use of the previously puzzling expressions 'command' and 'obedience'. Of course, they are still using concepts peculiar to social relations, and concepts which we also have; but it is not clear that this matters, provided they are not using our concepts for this case.

It seems that the force of Winch's argument must have been missed, and that this has occurred because the suggested identity between social relations and ideas of them has been neglected. The argument which he offers immediately following this claim is that social relations are internal, in the sense that an event's character, or perhaps its falling under a certain description, is intrinsic to it if it is a social event, but not if it is a physical event. He says:

> An act of obedience itself contains, as an essential element, a recognition of what went before as an order. But it would of course be senseless to suppose that a clap of thunder contained any recognition of what went before as an electrical storm; it is our recognition of the sound, rather than the sound itself, which contains that recognition of what went before.[6]

Now, if the identity between social relations and ideas of them is accepted, we can certainly argue that the extraterrestrials must be wrong, no matter how detailed their account: the idea of an invitation, even when unpleasant effects of refusal are recognised, is not identical with the idea of a command, therefore an act of acceptance of an invitation is not identical with an act of obedience, therefore the extraterrestrials reported acts which never took place, and missed acts which did. And this must be clearly distinguished from the similar-sounding, but false, claim that they reported acts which took place, but misdescribed them, for this would be to suggest that an act could be

identified as a physical movement, and subsequently described in social terms, whereas Winch clearly wants to say that an act can be identified only in social terms. But it is not easy to come to terms with the argument which supports the necessary identity claim.

It does not appear that his argument rests on the view that there is a logical relation between social concepts which is lacking in the case of physical concepts, since he points out that our use of 'electrical discharge', 'electrical storm' and 'thunder' does commit us in ways analogous to those in which we are committed by our use of 'command' and 'obedience', and regards this theory-impregnatedness as a general, perhaps even a necessary, feature of language. His point rather seems to be that the referents of physical terms have an existence and character independent of these logical relations, whereas the referents of social terms do not.

It is possible that his point is otherwise. Concepts like 'electrical storm' are quite sophisticated, and that one does happen to embrace 'thunder'; but this might not have been the case. Simpler concepts - of lightning as a flash of light in the sky, and thunder as a loud noise coming from the sky - might be in some sense theory-impregnated, but they need not be impregnated by any particular theory, or by a theory which relates

them. I might have accomodated these phenomena in quite different theories, yet there still be a sense in which I could be said to pick out the same things, as lightning and thunder, as does Winch. So it might be possible to express his point as one about the presence or absence of logical relations: one might say that a statement in which an expression is used to refer to some aspect of a social situation *immediately* entails a statement involving a particular social concept ('X is an act of obedience' entails 'Something was recognised as an order'); a statement in which an expression is used to refer to some aspect of a physical situation entails a statement involving a particular physical concept only through the mediation of some theory ('X is a clap of thunder' entails 'There was a lightning flash' only in conjunction with certain theoretical premisses).

This proposal introduces a concern with the relation between observational and theoretical language; and while I shall be concerned with aspects of this issue in the next chapter, particularly in discussing some arguments by Scheffler, it does not fit Winch's approach well. Also, the proposal, even in this crude form, raises problems. Even if the claim is accepted for the examples given, it is not clear that it is generalizable: that all social concepts are different

in this way from all physical concepts. An attempt to investigate reveals ambiguity in the status of the proposal itself. Is it to be understood as a comment on two categories of concept already distinguished in some other way, or as a criterion by which a distinction between social and physical concepts might be made? If the latter, its generalizability is guaranteed, but it must be appropriate to argue against its adoption on the ground that it introduces unnecessary complications into the social sciences. If the former, this kind of argument may not be appropriate; but its generalizability must come under question, as must the basis upon which the distinction is drawn.

I shall put this suggestion aside for the moment. As I have said, it does not appear to be Winch's intention to argue from a claim about different relations between concepts, but rather from a claim about different ways in which concepts relate to their reference. One reason for keeping the conceptual relations story in play is, as noted above, that it shows a connection with later discussion. A more immediate reason is that the alternative seems such an unlikely story for Winch to offer, given the general features of his position. He seems to say that the important difference is between things which are in some sense identical with the concepts particular people have of them, and things which are independent of the concepts

anyone has of them; and it does not seem consistent with his general position that he should allow there to be anything of the second sort.

In saying this, I am in some danger of being held to have misunderstood Winch. He has offered it as a truism that it is one thing for a man to think that something is so and quite another for what he thinks to be so, and has disavowed crude Protagorean relativism.[7] Of a statement about the height of Ben Nevis, he has said:

> What we need, in order to determine the truth or falsity of this statement, is geological, not anthropological or linguistic, information. Using Jarvie's terminology, I might express this by saying 'the reality of Ben Nevis' height is extra-linguistic.[8]

So he certainly wants to say that what makes certain kinds of statement true or false is extra-linguistic in some sense; and its being extra-linguistic in this sense might include its being independent of the concepts we have of it. In Jarvie's sense, the world's being extra-linguistic involves its not being altered by the language in which it is being discussed; and since its having or not having particular concepts would presumably be a relevant feature of the language, it would follow for Jarvie that at least in the case of physical things and events, they are independent of the concepts we have of them. That it follows for Winch is less clear.

After allowing that he might say that the reality of Ben Nevis' height was extra-linguistic, he goes on as follows.

> Does this mean, though, that the reality of *height* is extra-linguistic? Well, we might speak in this way if we wanted to express the point that the kind of remark I have made about the sort of information which is relevant to questions about Ben Nevis' height would apply equally to *any* questions about a mountain's height (or, with suitable modifications, about the height of anything else).[9]

But he thinks this misses the point of philosophical questions about reality; and goes on to speak of an observer from a group of whom we might say: "For them height has no reality". He imagines this observer saying: "There is no height", and meaning by this something like: "The whole idea of height as something to be measured is an illusion". He compares this case with philosophers who have denied the reality of time without wishing to deny the truth of propositions such as 'Before I had breakfast I washed myself'. He quotes Wittgenstein saying: "It is what human beings *say* that is true and false; and they agree in the *language* they use. That is not agreement in opinions but in form of life".[10]

I think we might, reasonably in this context, invent an extension of Wittgenstein's comment, to the effect that in their opinions, which are matters upon which humans may disagree, and which are not identical with their language or form of life, the determinants

of truth and falsity may involve " a certain constancy" which is independent not only of their wishes and beliefs, but also of their language; but in their language or form of life, agreement in which is presupposed by their disagreement in opinion, the notion of checking against extra-linguistic reality is inapplicable. This is consistent with Winch's use of Wittgenstein, and with what he goes on to say about the ways in which Zande beliefs might be corrected or changed. Now, the question is whether, in his remarks about thunder and lightning, Winch is saying things analogous to remarks about the reality of Ben Nevis' height, or to remarks about "the reality of <u>height</u>".

When he says that there existed electrical storms and thunder long before there were human beings, he expresses an opinion with which I can agree. Moreover, should it turn out that we were wrong in this opinion, the evidence of our error would not be linguistic information. But when he says that it would be senseless to suppose that a clap of thunder contained any recognition of what went before as an electrical storm, the situation is less clear.

It is my opinion, and I suppose his, that the supposition that a clap of thunder contained recognition of anything, in the sense in which he says an act of

obedience contains recognition of what went before as an order, would be false, since this would imply that thunder was the act of a conscious being, and I do not believe that thunder is the act of a conscious being. But I do not find the supposition senseless, since I can readily conceive of circumstances in which I might have believed that thunder was the act of a conscious being. The situation is complicated by Winch's use of 'electrical storm'. This is a sophisticated concept, set in a causal pattern of explanation which does not allow sense to the notion of an effect's containing recognition of its cause; and it would be, if not senseless, at least very odd of me to use these concepts in an attempt to say just the sort of thing they are not intended for. But this is just the point that human beings can think of occurrences only in terms of the concepts they do in fact have of them, and Winch is quite explicitly not making this point.

He seems to be trying to say that the reality of thunder - not just what we need in order to determine the truth or falsity of some statement in which 'thunder' appears, but the reality of <u>thunder</u> - is extra-linguistic. He was less sanguine about height. The most he appeared to allow that a claim for the extra-linguistic reality of height might mean was that the rules applicable in answering a particular question would be applicable to a range of other questions. No

doubt this is true of thunder too; but it falls short of making the desired point, since if this is all that having extra-linguistic reality comes to, acts of obedience would surely have extra-linguistic reality also.

The general point is this. If Winch, in developing his argument, is using the notion of reality which shows itself in a particular language or form of life, then perhaps he can say that reality is independent of concepts in some cases and not in others, providing he is using the notion correctly; but he cannot take his conclusions to be statements about languages or forms of life. If, on the other hand, he is talking, in some second-order way, about the way the notion of reality shows itself in a particular language or form of life, then his conclusions will of course be about forms of life; but the most he can do is to show what it comes to to say that reality is extra-linguistic in that language - he cannot make it a part of his argument that reality really _is_ extra-linguistic there. In his response to Jarvie, he seems to be taking the second line rather than the first; but in _Idea of a Social Science_ he seems to be taking the first line, yet treating his conclusions as though he had taken the second.

It is perhaps worth making this point in a slightly different way. Winch has defended himself against imputations of relativism by invoking a distinction which might be called a distinction between the use and the standing of concepts. The use might be roughly identified with the set of rules by which the truth or falsity of statements in which the concept appears is decided. Whether people agreed in the resultant judgments of truth and falsity would depend upon the nature of the rules, as well as on the sort of contingent data which Winch characterised, in the case of Ben Nevis, as geological, not anthropological or linguistic; but it is a tautology to say that those sharing the form of life in the language of which the concept had a place would agree in the rules. To agree in such rules is what it is to share a form of life. By the standing of a concept, I mean to refer to its being an object of agreement, in the sense which makes it partly constitutive of a form of life.

On Winch's account, it is possible to discuss, debate and to some extent identify the use of a concept. This process, the common notion of conceptual analysis, must be understood as an exploration, "from the inside", of the form of life in which the concept has a place. Consideration of its standing is more tricky. Of course, in showing its use, one shows that it has standing in the form of life; but this is not a great

achievement. That any concept has standing in the form of life in which it is used is presumably part of what we take as given in taking a form of life as given. But this cannot show that it has standing in any other form of life: this is the point that Winch makes when he speaks of the reasonable lack of Azande interest in our scientific or logical concepts, or the people for whom height was not real.

He uses this distinction most explicitly when dealing with charges of crude relativism. He takes it that those making the charges have assumed that he is relativistic about the use of concepts - one thinks of Newton-Smith defining relativism in terms of disagreement over truth and falsity - and points out that he is not. He then goes on to express relativist views about the standing of concepts. This is clearly the pattern of his remarks on height, quoted above. However, the distinction is also implicit in his discussion of the Azande. It is not just because we are interested in them as a society, and social science is different from physical science because social concepts are different from physical concepts in their relation to their reference, that we must take care to get their social concepts right. It is rather that <u>all</u> their concepts must be assumed to have standing in their form of life, as our concepts have standing in ours, and it

is their form of life that we are studying. Now, we may have very good reason to expect that the use of some of their concepts - observational physical object concepts, perhaps - will be very like our own; but we cannot jump from that expectation to the conclusion that their form of life will be fundamentally like our own, or even that the concepts we find most familiar will be quite identical. A concept's standing is standing in a particular form of life; we must explore its use in that form, not in another.

But then it seems that even if his comments on 'thunder' and 'obedience' are correct, i.e., if there is a form of life in which these concepts have standing and the use he suggests, his recommendations can have standing only within that form of life. If the social scientists he addresses do not share that form of life, then the concepts he describes have no standing for them, and his advice is worthless. If we take it - reasonably enough, given the significance of agreement - that they can be said to share his form of life only if they accept his analyses of critical concepts, then there is some reason for saying that they do not; and what is he to do then? But perhaps their apparent understanding of his claims shows that they do share his form of life. It would seem that the task of epistemology, to elucidate what is involved in the notion of a form of life in

general, has not been well done, since it is not in the least clear how the question whether these social scientists share with Winch a form of life is to be decided.

Again, this line of argument may be said to do less than justice to Winch's position. It makes it appear that his main concern is with analysis intended to establish the use of certain concepts; but at most points, and particularly where he is concerned with Wittgenstein's account of following a rule, his main interest is clearly in the question how concepts have standing. As an introduction to his development of this discussion, he quotes Wittgenstein's remark about forms of life as the given as a summary expression of the view that "... the philosophical elucidation of human intelligence, and the notions associated with this, requires that these notions be placed in the context of the relations between men and society".[11] We are to see these relations, expressed, *inter alia*, through agreement in rules, which is agreement in concepts, as constitutive of forms of life. This position has immediate pluralistic consequences. A plurality of forms of life is possible if it is possible for groups to disagree in concepts - to go differently about the business of going on in the same way. A plurality of forms of life is actual if people actually do disagree in concepts. It clearly is possible for groups to disagree in concepts, and there is reason for

saying that some actually do, so we have pluralism more or less at once.

This is, of course, a little too quick: there is nothing in this primitive story to show how much disagreement, or what kind of disagreement, is needed if we are to count more than one form of life. However, the exploration of ways of improving the account of a form of life is still to come; what should be noted now is that two possible objections which have been mentioned above do not hold.

This is not a case of inferring relativism from empirical observation, of the kind I have already rejected. Observation of the differing behaviour of groups cannot tell us whether they disagree in concepts, or agree in concepts but differ in opinion. It is only in the light of a more or less Wittgensteinian theory of meaning of the sort that Winch proposes that we can sort out such questions. Second, this is a case in which disagreement is a ground for relativism. Disagreement in the use of concepts is at least in principle open to resolution - the notion of correctness has some force. Disagreement in the standing of concepts is another matter. If we encounter a group which denies standing to some concept or set of concepts in which we agree, we can perhaps explain to them the

use we give the concept(s), so that they could recognise correct or incorrect use, yet they might still deny standing; and there does not seem to be any way of saying that this denial is correct or incorrect, or any further point we might go on to make. We might explain to them, as Winch does to the social scientists, what we think it is for a concept to have standing; but only if they rejected the explanation might there be any reason for their granting standing to the concept(s) in question. For them to accept the explanation would be for them to see that there could be no reason, at least no reason that we could offer them, for them to allow the concept(s) standing.

So this line does nothing to lessen the force of my last criticism of Winch's argument. Indeed, it may strengthen it. When Winch is confronted by a social scientist who approaches the task of describing society with a battery of more or less scientific concepts - one who says, as Winch quotes Durkheim as saying, that social life should be explained, not by the notions of those who participate in it, but by more profound causes which are unperceived by consciousness - what is he to do? By way of reply, he says this.

> The crucial question here, of course, is how far any sense can be given to Durkheim's idea of 'the manner according to which associated individuals are grouped' apart from the 'notions' of such individuals. [12]

I am not sure why this is the crucial question. Presumably, it would be possible to identify some individuals and groups in terms acceptable to Durkheim's program. Perhaps Winch wishes to suggest that they would not be the *right* groups; but if this is a question about use, it is to be answered on the basis of Durkheim's concept set, not Winch's; and if it is a question about standing it is not clear what role 'right' could be playing. It is interesting that Winch speaks in terms of "giving sense", which certainly makes it seem that the conflict he discusses is over standing rather than use. But how can standing be a matter of conflict? Differences over standing should indicate different forms of life; and though no doubt forms of life can conflict in some ways, the notion that Winch seems to suggest here, that the conflict is of a rational sort, is not compatible with the difference between use and standing as I have understood it. Winch appears to be doing just what he says cannot be done: attempting to reshape one form of life to fit another.

This criticism is very similar to one of those offered by John Kekes,[13] whose arguments I will discuss in the next section. It is part of the complex of problems which arise because the question how forms of life might differ, and what they might have in common, is left so

open. One is rather left with the impression that a form of life, like one of Wittgenstein's language games, is what it is, with nothing further, of a general nature, to be said. However, Winch's discussion has suggested that the notion of forms of life provides a potentially useful perspective on questions about the relation of meaning and truth, and the conditions affecting identification and individuation; and might yield a notion of relativism more substantial than the negative forms I have employed, and more plausible that such primitive versions as that set up by Newton-Smith. In continuing the discussion by examining the counter-arguments offered by Kekes, I shall hope to show that something like Winch's position can be maintained, and also to find indications of the lines along which attempts to improve the notion of a form of life might proceed.

SECTION 5 Consideration of a criticism of Winch suggests further development of the notion of a form of life.

Kekes addresses Winch's position as a relativist theory of rationality. He notes that it has been opposed on the ground that there are absolute standards of rationality, and on the ground of unsatisfactory analysis, citing as an example the vagueness of 'form of life'. His criticisms, however, he says are "... internal. They show that if what Winch says were true, the certain obviously possible things would be impossible".[1]

His first criticism is based on the need for a rule by which the difference or sameness of forms of life could be determined, and is developed to yield the dilemma: "If the required rule is part of a form of life, then infinite regress results. If the rule is independent of all forms of life, then Winch's thesis is false, for forms of life, then, may be judged externally".[2] Given Winch's commitment to Wittgenstein's view of the relation between the notion of a rule and the notion of sameness, the basis of the argument is sound; and there is no doubt that the notion of a rule outside all forms of life must be unacceptable to Winch. The argument for infinite regress turns on the suggestion that, if the rule is part of a form of life, then that

form must already have been distinguished from others by another rule. concerning which the same questions arise. In the absence of a clear notion of a form of life, and of the possible relations between forms, it is hard to evaluate this argument. It is Kekes's view that there can be no relations between forms. He says:

> There must be discrete forms of life, for if there were not, notions of rationality would not be really different.... But the compulsion to recognise discrete forms of life makes it impossible to account for communication between participants in them. For communication requires a shared medium and if forms of life are really discrete, then there could not be any. On the other hand, if there was a shared medium, then forms of life must be related to it, and through it to each other, consequently they would not be really discrete.[3]

This view, that forms of life must be unrelated if they are to be discrete, was not a part of his initial representation of Winch's account of a form of life; it is, however, a crucial part of his argument. It is on this that his claim that Winch makes impossible what is clearly possible depends. The pattern is clearest in his discussion of a religious/scientific debate between two Winchian characters called Rench and Sinch. I shall return to this example below; for the moment I merely want to note that the point that Kekes makes here is so glaringly obvious that it is almost inconceivable that Winch could have overlooked it, and so totally destructive of the notion of discrete forms

that Kekes attributes to Winch that it is almost inconceivable that the attribution could be correct. It must be said, however, that I do not find anything in Winch which would serve as a conclusive refutation of the attribution. Much of his discussion seems to assume the possibility of communication, but he does not provide any clear reason for rejecting Kekes's claim that through this relation the discreteness of forms is dissolved, nor do I see how he could do so, except on the basis of the sort of clear and detailed account of the notion of a form of life that he has not offered. Thus Kekes's argument exploits the vagueness of the notion of a form of life; and while it is fair criticism, it has the weakness that a more precise notion might pass between the horns of the dilemma.

This weakness may have occurred to Kekes, since he considers a possible objection to his argument which involves some slight consideration of the kinds of rule which might go to make up a form of life. The objection is that:

> ... the rule used for distinguishing different forms of life occurs within a form of life, but it is self-referential. The rule may be said to contain a description of some important features of the form of life within which it occurs. Other forms of life can then be distinguished by noting whether or not they lack some of the specified features. [4]

He offers two reasons for rejecting this suggestion. The first, that the point of having such a self-

referential rule is to distinguish one form of life from another, and that this involves assuming that there are different forms of life when the rule is supposed to establish that there are, seems to be just a mistake. That, whatever may be the difficulties of getting outside a system, we can at least work up to its limits from the inside, is a common Kantian view, argued by Strawson;[5] and it does not depend upon assuming that there are alternatives.

The second reason is that a self-referential rule could distinguish between those activities which were in conformity with it and those which were not; but not, among non-conforming activities, between those which were poor attempts at conformity and those which fell under another rule. By way of example, he suggests that a self-referential rule for science, applied to religion, could not determine whether religion was a different form of life, or an inferior form of science. He does not cite a possible rule. One may imagine he has in mind something like : an observation counts as evidence only if it is intersubjective or repeatable in the required sense. Since religious contexts commonly allow status under such rubrics as religious experience or revelation, religious activities would not conform to the rule. But whether we have the choice Kekes suggests, between taking religion to be

non-science or bad science, depends on the status of the rule.

John Searle has discussed a distinction between constitutive and regulative rules.[6] I shall be concerned with this distinction in some detail in the next chapter. For the present, it is sufficient though somewhat over-simplified to say that the difference is that breach of constitutive rules does, while breach of regulative rules does not, cancel the claim to be engaging in the activity to which the rules apply. The required sort of self-referential rule would surely be constitutive; and if so, the conclusion could only be that religion - or those activities which we have chosen to test against the rule, in which religious experience and revelation are concepts with standing - is not science.

It is important not to suppose, as perhaps Kekes does, that such a use of a constitutive rule would be a simple, one-step process. If I wished to decide whether two people were playing chess, I would not conclude that they were not the first time I observed an illegal move, for people can be, occasionally or systematically, in error. So I might interrupt them to cite the relevant rule, and only on the basis of their subsequent behaviour reach a conclusion. I would have to behave similarly in order to decide that a chunk of discourse was non-scientific; but I see no reason to

think that the choice will remain open indefinitely.

This seems to have missed two of Kekes's points: that the rule has not shown that the discourse identified as non-scientific is part of a form of life; and that I should not presume the possibility of communication.

It would be absurd to suppose that the constitutive rules of chess could include those of other games as well; the same applies to the constitutive rules of science. What is puzzling is Kekes's apparent assumption to the contrary. If we can find constitutive rules for science, perhaps we can find them for religion too; and it is these rules, not those of science, that will identify the religious form of life.

Kekes's answer, I think, would be as follows. Suppose that we are participants in the scientific form of life. Why, then, should we come to think that there are others? Why should we accept these "religious rules" as constitutive of a form of life, rather than an aberration? Why should we say that certain queer concepts have religious standing, rather than saying they have no standing at all? And if we were willing, for no reason, to suppose an alternative form of life, how could we cross the communication gap to find out about it?

The answer to the last question is crucial and difficult. I do not think that Winch provides it, though he gives a hint, which I shall mention below. I take the attempt to answer this question as my main task in this thesis, and I take the proper articulation of the notion of a form of life to be necessary for success. Some preliminary suggestion is desirable; but I defer this to the discussion of Kekes's second criticism of Winch, which provides a better occasion. To the earlier questions an answer was suggested earlier. The source of our willingness to recognise alternative forms of life must lie, not in rules constitutive of particular forms of life, like the evidence rule I suggested, or Kekes's reference to hypothetico-deductive method, which establish the use of concepts; but in such rules as might be found to show how concepts have standing, or, in Winch's terms, show what is involved in the notion of a form of life as such. Such rules would be discoverable in any form, and would be the same in every form. Kekes, of course, quite properly asks by what rule <u>this</u> sameness is judged, and how it is related to various forms. I think that this question can be answered; but the answer must await further developments.

I should like to note at this point that although my invocation of Searle's notion of constitutive rules is subject to further discussion, it has a

*prima facie* appeal arising from the way it meshes with the distinction between use and standing which I have attributed to Winch. One might say: a constitutive rule establishes the use of a concept; the concept's standing consists in its occurrence in a constitutive rule. Of course, nothing so crude as this will ultimately do; it can be seen only as an adumbration. However, it is a further part of its appeal that at least one point which at first sight counts against such meshing, on closer examination offers some degree of promise. Searle's examples of rule-constituted activities - playing chess, making promises - are limited and specific, and seem remote from the sweeping conception of a form of life. Moreover, they clearly admit the common medium of communication which Kekes sees as troublesome for Winch. But then we turn to the hint which I mentioned above.

Winch says:

> In a discussion of Wittgenstein's philosophical use of language games, Mr. Rush Rhees points out that to try to account for the meaningfulness of language solely in terms of isolated language games is to omit the important fact that ways of speaking are not insulated from each other in mutually exclusive systems of rules. What can be said in one context by the use of a certain expression depends for its sense on the uses of that expression in other contexts (different language games). Language games are played by men who have lives to live - lives involving a wide variety of different interests, which have all kinds of different bearings on each other. Because of this,

> what a man says or does may make a difference not merely to the performance of the activity on which he is at present engaged, but to his life and to the lives of other people. Whether a man sees point in what he is doing will then depend on whether he is able to see any unity in his multifarious interests, activities and relations with other men; what sort of sense he sees in his life will depend on the nature of this unity. The ability to see this sort of sense in life depends not merely on the individual concerned, though this is not to say it does not depend on him at all; it depends also on the possibilities for making such sense which the culture in which he lives does, or does not, provide. 7

For 'form of life' in Winch's writings, we sometimes seem able to read 'unity' or 'culture' in roughly the sense those terms have in the passage quoted; and at other times to read something rather like 'language game'. The lists of examples and synonyms which Kekes quotes from Winch illustrate this variation in scope. Science, art, history and religion, no matter how obsessively they may be pursued by individuals, will generally only be a part of the life of the individual, and a smaller part of the life of the community in which they are pursued; the life of a monk, on the other hand, may be thought of as a case in which, at least in principle, the whole life of the individual is an expression of faith, and is in this representative of the community in which it is pursued. 'Mode of life' or 'way of life' may be thought to apply to a unity or culture; 'form of activity', 'category

of behaviour' and 'mode of discourse' seem more readily applicable to language games. Although very often Winch seems indifferent to the scope of 'form of life' and its possible cognates, in the passage above he is drawing a reasonably clear distinction, involving a part-to-whole relation. His warning is against exclusive concentration on the part; he might, by way of corollary, have warned against exclusive concentration on the whole. So, if Searle's notion of constitutive rules does not seem readily applicable to the most sweeping notion of a form of life, it does not follow that it is not applicable at all. And to the extent that it applies to something on the scale of a language game, it might provide a basis on which the necessary account of the articulation of a form of life could be developed.

While the above is, strictly speaking, sufficient for my main purpose in this section, Winch's general position is of sufficient importance in my story for it to be desirable to consider what force certain arguments, turning on notions of communication, criticism and rationality, have against him. I shall therefore go on to consider the second major criticism of Winch which Kekes offers.

This criticism is based on the claim that

positions, beliefs and practices in religion, politics, history and so on, are commonly criticised from the standpoint of another position, and by extracting rules from this common practice, and distinguishing reasonable and unreasonable ways of going about it, one could present it as a form of life. On this basis Kekes offers a dilemma:

> ... either forms of life can be criticised from the point of view of other forms of life, or they cannot be. In the former case, magic and religion can be argued to be irrational without absurdity. In the latter case, Winch's criticism of the form of life involving external criticism of other forms of life is illegitimate. In either case,[8] Winch's position collapses.

This criticism, or at least the second horn of the dilemma, is very like that which I advanced in the last section, so I must clearly have some degree of sympathy with it. However, I do not think that Kekes's version will do.

The dilemma says in effect: concerning the external criticism of forms of life, either Winch is wrong or he is right. If he is wrong, then he is wrong; if he is right, and says that he is right, then he is wrong to say that he is right, and so he is wrong. If one takes it seriously, the second horn is a disaster. If forms of life cannot be criticised from the outside, then no one does so, though they may make mistakes which they mistakenly call external criticism of a form of

life; thus there is no form of life involving external criticism of other forms of life; thus in criticising this mistake Winch does not break his own rule. The real argument behind this confused presentation seems to be as follows. It is clearly possible, because it happens, that people criticise one position from the standpoint of another: in Winch's terms, they criticise forms of life externally. Hence Winch must be wrong, since he says this is impossible. If he admits this, fine; if he denies it, we can show that in denying it he refutes himself by offering external criticism of a form of life. It may be that he thinks that Winch himself acknowledges the activities in question by criticising them, that he should accept them as a form of life, and that the alleged confusion of Kekes's argument was in fact just a reflection of the internal confusion of Winch's position.

Now, it seems obvious that in order to adjudicate the point, one would at least have to be clear upon the difference between one form of life and another, and in this neither Kekes nor Winch is very helpful. Winch uses science as an example of a form of life; but in the light of the quotation referring to language games which I gave above, it seems clear that only in the most exceptional circumstances could science be the totality of the "unity" or "culture" to which he there

refers. To say, as he there does, that the rules and conventions of activities may relate to "a sense of the significance of human life" does not seem to advance us very far. Kekes, however, does make an attempt to move the discussion on.

He offers a religious and scientific discussion between two people called Rench and Sinch, who set out, in a Winchian way, to understand each other's positions, which turn out to be based, respectively, on the Thirty-Nine Articles and the hypothetico-deductive method. But when this is achieved, Sinch says in his heart that there is no rationality in accepting the Thirty-Nine Articles, a view which he might then debate with Rench. In order to clarify the issue, Kekes introduces a distinction between weak rationality, which consists in behaving in accordance with some norm, and strong rationality, which consists in behaving in accordance with a rational norm.

The question he raises concerns the medium in which such arguments are conducted. He thinks it cannot be either religious or scientific discourse, and he takes it that these are the only possibilities Winch could allow. He proposes that the medium is a natural language, in this case English, that the technical sense 'rational' acquires in religious or scientific discourse "...derive from the original, non-technical uses of

'rational'", and that the debate will usually involve some comparison of derivations.[9]

Now, the suggestion that, because 'rational' has a sense or senses in English it can be used to decide a religious/scientific dispute sounds rather like the suggestion that English embodies opinions on what is and what is not rational, and this is a suggestion that Winch explicitly rejects. An alternative interpretation, that the use of 'rational' by English speakers shows the norms which they take to be rational, would bring the notion of strong rationality close to that of conformity with popular opinion, and this would not serve Kekes's interest well.

A great deal of the trouble with Kekes's argument is the obscurity of the target; the way in which he has set it up affects his talk of 'rationality'. He begins by making Rench and Sinch as Winchlike as he can; but since we do not know what exactly a form of life is, and what are its scope and limits, the force of saying that they participate in religious and scientific modes of life is unclear, and the way is open for Kekes to set the scene very much in his own terms: we do not really know what they can and cannot do, if they are to remain faithful to Winch.

Each understands the other perfectly well, in the sense that each could understand, present and appropriately criticise arguments in the idiom of the other. Sinch thinks that Rench is only weakly rational: that he acts in accordance with an unacceptable norm. But then Sinch participates in a scientific mode of existence, and the Thirty-Nine Articles are not an acceptable substitute for hypothetico-deductive method. What Rench thinks of Sinch's rationality we are not told. We are told that an answer to Sinch's challenge in religious terms would be very unsatisfactory indeed; but apart from the unsatisfactory suggestion that a natural language may be the path to satisfaction, we are not told what kind of answer might be satisfactory, or why it should be supposed that satisfaction was possible.

Kekes is now edging away from Winch: he no longer takes the notion of religious discourse very seriously. If he did, he might have to say that Rench could reply only in religious terms, and thus could not satisfy Sinch, who could accept an answer only in scientific terms - an answer which might be acceptable to Winch, and incidentally quite plausible as a description of much "debate" of this sort. But Kekes wants to show us real debate, and to do this he has to drop Winch altogether. Religious and scientific discourse, he

says, are not languages, but technical vocabularies superimposed on natural language. So when he says that Rench should not answer in religious terms, and that Sinch does not conduct his argument in scientific discourse, he means that they abandon their technical vocabularies, and use the resources of ordinary English. And now he is so far from Winch, and from any serious notion of a form of life, that the relevance of his argument is tenuous. He could save it only by showing clearly, albeit in his own terms, the possibility of real debate between Sinch and Rench. This possibility, could it be shown, would be something that Winch would have to accomodate. But even with the freedom he has allowed himself to build implicit contradictions of Winch into his presentation of the case, he fails to show it.

The argument is not to be conducted in scientific discourse, for Sinch is not a fool, and does not expect to be given an experimental justification for accepting the Thirty-Nine Articles. But if he does not expect this - and it is not clear that he would be a fool if he did, though no doubt he would be disappointed - then what does he expect? Well, he expects to be shown the derivation of the religious from the ordinary senses of 'rational', and may complain if it is too thin-drawn. But what "ordinary senses" of 'rational'? Rench can say that religious rationality

involves conformity to a norm; but this will not do for Sinch, who must be shown not only that 'rationality' in religious contexts has this ordinary sense, but that it is derived from an ordinary use.

Kekes employs both 'use' and 'sense' in speaking of 'rational', without drawing a distinction between them. I think that this is unfortunate, for this may well be the real distinction between weak and strong rationality. One is weakly rational when one observes the sense of 'rational' - when one acts in conformity with a norm. But any use of 'rational' must involve a particular norm; and given any use of 'rational', further cases of weak rationality can be sorted into those which do and those which do not conform to the same norm as the given case. 'Rational' is used in discourse in non-technical language, so it is possible to ask of any technical use whether the norm it invokes is consistent with the norm of an ordinary use. To find that it is, is to find that the technical use is strongly rational - at least, this is what Kekes's argument seems to say.

But ordinary discourse is indecisive. Regard for empirical evidence is a significant norm, but so is regard for intuition or insight, and one can override the other in complex ways. Of course, empirical

evidence is beyond rational challenge in the right sort of case, but such tautologies do not decide the issue. Now, if Sinch expects to be shown that the religious norm is derived from the empirical norm he will be disappointed. But if he is shown that the religious norm derives from a norm of intuition, why should we suppose that he is satisfied by this, rather than that he extends his doubts to those who employ 'rational' in this way in their non-technical concerns?

All that Kekes does is to extend the unresolved conflict into domains which might have been thought to be excluded from the original notions of scientific and religious modes of existence. In so doing, he does not show genuine debate to be possible; that Rench and Sinch both understand these ordinary senses is no more to his point than the initial assumption that they could understand each other's technical discourse, unless we have licence to assume that there are no disagreements that they express in non-technical English, with effect on their uses of 'rational'. If English does not embody opinion, the only sense it can give to 'rational' is the weak one; if it does, then it need be no more common to Rench and Sinch at its non-technical levels than it was when they spoke technically.

I do not think that Kekes makes the point he

intends; but it does not follow that he makes no point at all. The activities of Sinch and Rench, and the problems they raise, reinforce the doubts about Winch's use of 'form of life' which I expressed above, in relation to the comments on unity and culture which I there quoted. Rench and Sinch have much in common, and Winch must somehow take this into account; but it is not easy to see how, if science and religion are really distinct forms of life, he is to do so. The gulf between forms of life lies between them, and where are we to place the things they seem to have in common?

The problem would be solved, to Kekes's satisfaction, if he could show that it was possible to criticise one form of life from the standpoint of another, for this would reduce the gulf to an insignificant crack. But the question whether this is possible is confused rather than clarified by the discussion. In one sense, of course it is. Sinch can extract from Rench's discourse those expressions to which he can give some satisfactory, i.e., scientific, sense, reconstruct the argument, and show it to be bad science: he can accuse Rench of lack of evidence, as Rench can accuse him of lack of faith. This sort of thing probably makes up the greatest part of the "immense literature" of religious and political argument to which Kekes directs Winch's attention. Neither side need be ignorant of

the position of the other: they may know it very well, and refuse to concede any part of it. To suggest that because they understand both views, they are somehow prohibited from speaking or acting in accordance with either is just nonsense. Yet this is what Kekes does. He reminds us of such quarrels, then refuses to allow his sample protagonists either to attack or defend from the point of view by which they were identified.

On the face of it, he does so because it is otherwise impossible to see how they could argue with each other; but of course they can argue with each other withoutabdicating standpoints. Each can reiterate his principles, and deny those of the other; each can attempt to persuade the other by invoking feelings of pity or fear or pride; each can abuse the other. Of course, this is not, in a certain restricted sense, rational argument; but it is not irrational either; and nowhere has Kekes shown that the criticism which he wants to count as a form of life must be rational in that restricted sense. Without this requirement, the practice of criticising one form from the standpoint of another would not need yet a third form to do it in - it would be preeminently an activity we could perform at home. If we were not Winchians, then, as Kekes himself has insisted, we would need a good reason for seeing the odd behaviour of others as an alternative

form, and would not need to put our standpoint in storage during the argument; if we were Winchians, we would not expect the argument to be rational in the restricted sense. Only the fact of rational debate between forms would force us, in either case, to play by the rules imposed on Sinch and Rench; and that there is such a fact Kekes has failed to show. So why did Kekes choose this path?

He wishes to refute Winch's claim that one form of life cannot be criticised from the standpoint of another. Now, in the sense I suggest above, the claim is false, and Winch must know this as well as Kekes or I, else how could he suppose that there were any such practices as those of the social scientists whom he criticises? But this will not do for Kekes. He wants to press home the dilemma that Winch must account for communication across forms, yet is committed by the very notion of discrete forms to denying it; and in this case Winch might argue that there is no communication, even where there is a common natural language. So Rench and Sinch must be supposed to understand each other. But this is not enough: Winch could allow them two common languages, without there being a language shared by the forms themselves. Nothing will do but that Rench and Sinch compare the validity of their forms in rational debate. This is what Winch cannot

allow, so this is what they must be supposed to do.

But the argument stands on its head. That distinct forms preclude communication, and that communication is a fact to be accomodated, are not shown by the discussion of Sinch and Rench, but presupposed in it. They are said to participate in different forms. But there is no attempt to show what account of their activities could be given under a theory of forms of life, or how much of our ordinary descriptions of what goes on could be accomodated or explained under such a theory. We are told that there is a rational issue between them: "If Sinch is right, then Rench may turn out to be irrational after all"[10]; we are told that they can debate it: ".. Sinch may make this view known and Rench may contest it"[11]; we are told that they abandon their forms in the debate. The only possible question is in what forbidden medium the debate is conducted, so the notion of a form of life moves from vagueness to incoherence without the need for any troublesome attempts at clarification.

Yet there is so much here that Winch might have accomodated. Clearly, Sinch could criticise Rench as a bad scientist, neither knowing nor caring what significance that behaviour had for Rench himself. Less clearly, he might be right to do so, if this were part of living

a form of life, as opposed to seeking to understand other forms, or the notion of a form of life as such. Clearly, Sinch could come to understand Rench's form of life, and the significance Rench found in it, and yet reject it. Clearly, he might attempt to persuade Rench, in a non-ratiocinative way, to prefer the scientific form of life; and he might succeed in this. Much less clearly, but I think intelligibly, Sinch might come to think that he and Rench differed in a language game but shared a form of life. This last suggestion could cause problems for Winch. It might allow that the activities of scientific social scientists of which he disapproves, and those of philosophical social scientists which he approves, could be alternative language games within the same form of life, which would suggest that he had sadly misunderstood his own theory; but it would provide richer resources for dealing with Kekes.

Now, whether what I have said is clear is really so, and whether such suggestions as the last are useful at all, are parts of the general question of what sort of account can be given of the notion of a form of life. I have tried to show that, even in its present form, the notion can blunt the edge of such criticism as Kekes's, even if it cannot wholly turn it aside. I take the most promising directions of enquiry to have

emerged in this discussion to be the idea that a language game account allows the articulation of the notion of a form of life; the relationship which I have suggested exists between this aspect of Winch's theory and Searle's account of constitutive rules; and the need to give an acceptable account of the basis of communication between distinguishable forms.

## CHAPTER 3 A pattern, discoverable in justificatory regress, can be identified and discussed by the use of a notion of constitutive rules.

### SECTION 6 A comment on procedure.

The attempt to present forms of life as organizations of language games requires an account of language games which shows both how they are distinguished and how they are related. That is, one needs to show, with respect to some universe of discourse, both that it is in a certain sense partitioned or discontinuous, and that it is in a certain related sense integrated or continuous, and that both these senses are explained by the notion of a language game. There are two more or less complementary ways in which the attempt might be made, both of which could be explained by reference to the Wittgensteinian background to this way of stating the task.

That such an explication of the notion of a form of life is reasonably consistent with Wittgenstein's use of the expression is shown not only by such judgments as that of Rush Rhees quoted above, but also by such comments of Wittgenstein's as "We see that what we call "sentence" and "language" has not the formal unity that

I imagined, but is the family of structures more or less related to one another"[1].

One method of approach is that implied by the aphorism "Don't ask for the meaning, ask for the use"; and explicit in his comments when considering the notion of games. We can attempt to show what it is that we do and say under certain circumstances; and perhaps, as an extension of this method, what it is we might do or say, or the extent to which we could decide what to do or say, in unusual circumstances, or in circumstances in which the point of our activities were somehow different. It is in the context of such an endeavour that one can most easily understand his suggestion that philosophy simply puts everything before us. In such a process, we rely upon acceptance of our identification of activities, a point which is significant for that notion of a constitutive rule which I mentioned above, and will discuss in more detail in this chapter. There is a relation between the notion of such rules as prescriptive and behaviour directing, and as descriptive, or as providing the basis upon which identifications are possible.

At one point, Wittgenstein says:

> Let us now examine the following kind of language game: when A gives B an order B has to write down a series of signs according to a certain formation rule. The first of these series is meant to be that of the natural numbers in decimal notation.[2]

Now, B's following this rule is his doing a certain thing - "'obeying a rule' is a practice"[3]. But our saying what he does - our identifying his practice as writing down the natural numbers in decimal notation - is equally a practice related to that rule, and exhibiting our agreement in it. This relationship will recur from time to time, first in the form of the suggestion, by Scheffler, of an indeterminacy between empirical statement and definition; second in the form of the question whether Searle's constitutive rules license prescriptions or descriptions; and in other ways. At present, its point is that it explains the sense in which an identification might be accepted in such contexts as this: it will be accepted to the extent that it identifies what we do or say, or might under some circumstances do or say, in ways that are accepted as expressions of the rule that the activities in question express. Thus in this approach one tells stories of real or imaginary activities, hoping first for assent, and second to show, through that assent, significant features of the case.

The second approach is that which in Wittgenstein takes the form of describing simple language games which we might imagine being played, although we do not actually play them. These games have, in his view, a limited function. He says this of them.

> Our clear and simple language-games are not preparatory studies for a future regularization of language - as it were first approximations, ignoring friction and air-resistance. The language-games are rather set up as <u>objects of comparison</u> which are meant to throw light on the facts of our language by way not only of similarities, but also of dissimilarities.
>
> For we can avoid ineptness or emptiness in our assertions only by presenting the model as what it is, an object of comparison - as, so to speak, a measuring-rod; not as a preconceived idea to which reality <u>must</u> correspond. 4

However, I want to suggest that, although such a "model" is not something to which reality <u>must</u> correspond, it is something to which reality might <u>come</u> to correspond. We might set out a language game which people chose to play, so that what we presented as an object of comparison became one of the family. And, if our model were suitable, it might allow the playing of a philosophical game. That is, while it is extremely improbable that the model would provide a substitute for all the games that people play, it might substitute for some of the games that philosophers play, if they came to see it as providing a rule by which they acted and identified; though it might not regularise language, it might come to count as a philosophical thesis.

In this chapter, I shall employ both of these approaches. In Section 7, I shall offer an account of what it is that goes on in the context of justificationary regress, and suggest that we might see the regress

as concerned with just that sort of federation of games that might constitute a form of life. In Section 9, I shall consider Searle's notion of constitutive rules as the basis of a formal model of language games united in a form of life, and thus of a playable philosophical game. Following my practice in Chapter 2, I shall in Sections 8 and 10 consider fairly specific criticisms of the views expressed in the preceding sections, with the intention both of defending the views, and finding in the criticisms some basis of clarification or improvement.

## SECTION 7 Consideration of the justificationary regress leads to the recognition of "ways of acting" in which both belief and convention are involved.

In that part of the discussion of Section 2 which concerned the views of W.W. Bartley, I noted his view that relativistic commitment arose from inability to terminate the regress generated by the acceptance of justification as the basis of rationality. I also noted that the problem of justificationary regress was taken by Bartley to require for its solution an absolute rather than a relative notion of justification. This is a common attitude. Quinton, for example, gives an account of philosophical theory of knowledge as ".. bound up with the idea that knowledge as a whole is a hierarchical system".[1] He pictures Aristotelian contexts of relative knowledge modelled on geometry being collected as by ".. Plato's conception of dialectic as a sovereign study in which the relative axioms ... of particular bodies of knowledge give up their status as axioms on being derived from an absolute and all-inclusive set of first principles".[2] That this is his own view becomes clear when, in his discussion of the numerous distinct systems which were later formed, he says:

> The only objectively logical ground for choice between them was their comparative economy in.. axioms and primitive terms. The idea that one set might be epistemologically preferable to

> the others ... was lost sight of. This formal relativism about systems of formal truths passed over into the analysis of factual truth ... in the face of disputes about the nature of the basic factual statements ... Carnap ... put forward a principle of tolerance, allowing that any formally satisfactory systematization of knowledge was as good as any other ... since the dispute about foundations was resolved in this radical way, by denying the significance of the question at issue, philosophers interested in logical systematization have in effect abandoned an interest in knowledge.[3]

The instrument of the regress is scepticism: the notion of justification turns on the notion of challenge. Thus Quinton develops the comments above be reference to "sceptical challenge"; and even Rescher, in the course of a detailed defence of coherence theory of truth, takes account of the "spectre of scepticism", which he represents as leading to questions like ".. how can there ever be a secure standard of rational acceptance? How can we secure such a standard save by appealing to another standard - and then again onwards until ultimately some unjustified standard is irrationally accepted?".[4]

One should first see that this absolutism is itself a guiding principle. It is not an integral part of a sceptic-induced regress of justification. The sceptic may base his approach on the assumption of a liability for justification attaching to claims generally, and proceed by requiring that justifications be

actually given; but this is the limit of his substantive commitment. He may suggest evidence contrary to a claim, where this evidence is such as ought to be acceptable to the proponent of the claim; but it is not his business to deny claims. Hence scepticism is not a process suited to falsifying views, or even to casting doubt on them. Even if it is assumed that the demand for justification must outrun supply, the outcome can only be a body of unjustified claims. This is not a demonstration of their falsity; it is not even a reason for abandoning them. Scepticism is an instrument by use of which to induce a justificationary regress we can show something of the structure in which our initial claims were made. The question whether we require some total structure allowing absolute justification if we are to accept anything at all as a justification is another matter.

In my discussion of Bartley, I have already suggested that the structures revealed in a justificationary regress are not simply linear, or something like tree diagrams in which each step is of much the same sort as the others; but that justifications come in clusters. Something similar is implicit in the comments quoted from Quinton, though his concern is only with the kind of clustering represented by explanatory theories. I think it is possible to see that the clustering of justifications is more complex than this, and that its effects

include not just the ways in which we attempt to defend the claims we make about the world, but also the ways in which we find it possible to talk about the world. It might be helpful to consider an imaginary example of conflicting claims, in which these aspects of contexts of justification can be exhibited.

Suppose that my motor car displays a mechanical defect - it misses fire to such an extent that I can no longer coax it up even a moderate gradient - and I take it to a garage, where I consult a motor mechanic. Suppose further that after inspection, he delivers the following report: the trouble is that fairies are finding their way into the engine and stealing the sparks, a thing they often do in order to re-ignite fireflies that have gone out. The best solution would be to carry some spark-emitting device on the car so that they did not need to get into the engine.

The interestingly problematic feature of this situation is not that what I am told is false. We may suppose that my views are in conflict with those of the mechanic, so that I take his report to be false, do not adopt the remedy proposed, and, should there be any apparent sign that the proposed remedy is effective, I seek an alternative explanation. But the report could

have been false in less bizarre ways. I might have been told that the plugs were defective when in fact the distributor was at fault, without seeing the case as in any interesting way problematic. In fact, it would be much more appropriate to call this sort of mistake a false report; the original supposition raises questions not so much about the truth of the report, as about the basis on which truth is attributed to reports.

Nor is the problem adequately identified by saying that demands for justification expose a body of background belief divergent from that commonly held by motor mechanics, so that we should go on to see which of the divergent beliefs is susceptible of further justification. Mechanics might differ, in ways affecting the practice of their trade and their successful performance in it, and we might represent their differences as such precisely because we understood the criteria under which they would compete for justification; but this does not seem to be that sort of case.

We can, of course, compare this report with a more conventional one by seeing which proposed remedy works; but this process will not be like that of checking to see whether it is the plugs or the distributor which is at fault. Even if the proposed remedy appeared to work - that is, if the car ran

satisfactorily after it had been put into effect - and such conventional remedies as were tried did not work, I should not take this as establishing the claims about fairies against the competing normal account of the way an engine works. I should be puzzled, no doubt; but hardly convinced about the existence of fairies. Nor would the mechanic's further success compel me to accept his beliefs, though there might come a point at which his record of success constituted a reason for me to change my beliefs in some way.

The attribution to myself of this conservative attitude is not entirely a psychological hypothesis. I am to some extent committed to conservatism by my first statement of the case. In this statement I made use of such terms as 'motor car', 'mechanic', 'misses fire', and so on. These expressions, together with a number of others, draw part of their meaning from their use in the context of activities concerned with the restoration of function, and to the extent that to be a motor mechanic is to be one who restores the function of a certain kind of thing, there would be some pressure to change the use of 'motor mechanic', and certainly a substantial change in the connotations of the term, if it were to be the case that established procedures for restoring function ceased to work, and other quite different procedures succeeded. These notions of success relate the terms to various norms and conventions

of behaviour, and notions of function, so that our view of their applicability is not only disturbed by a break-down in expected outcomes, but by improper attitudes: a conservationist who said cars ran best when they were wrecked, and proceeded to wreck them in the interests of environmental conservation, could not be called a motor mechanic.

The terms also draw meaning from their use in contexts that might be loosely described as scientific - that is, in which certain ontological assumptions and heuristic principles are parametric. If the procedures found to be effective in restoring function were to change in the radical way supposed in the example, and if this were accepted as good reason for accepting the explanation my imaginary mechanic proposed, this would involve to some degree the abandonment of these parameters, and hence a change in the sense of the terms employed in describing the situation.

On this account, in accepting the mechanic's report fully, for whatever reason, I would effectively change the meaning of the terms in which the situation was initially described, and hence change my account of what the situation in fact was. Although this change may appear to be in some degree required in order to retain the place of the terms in function-restoring

activities, even this is not clear, since the function which was to be restored - the engine's smooth running - must be identified in terms that are themselves to some extent affected by the changes in sense affecting the cluster of relevant terms generally. Thus I will not be in a position to assert that the lost function has been restored, since this assertion would require the use of a seriously ambiguous term to refer to the function: a term which occurred once in its abandoned, and once in its new sense.

Even to say that one function had been replaced by another might be difficult, since the new network of terms might make it hard to use any term to refer to running in the way the old term did; as, for instance, a scientific world-view makes it hard to refer by the use of 'magic', except perhaps in a Pickwickian sense.

Thus my conservatism involves not just a desire to retain a body of accustomed belief, but a need to maintain continuity of reference. The situation is much more complex than one in which a mistake has been made, or a demand for justification has been rejected arbitrarily. The problem is not that I cannot know that the mechanic's claims are false unless I know that all the claims upon the basis of which I reject them are true. The problem is that, appearances to the contrary

notwithstanding, I cannot make sense of his claims in the context in which they have arisen. Or, in other terms: the mechanic and I do not look at the same things in order to see which of us knows what he is talking about; it is rather that until we know what we are talking about, we do not know whether we look at the same things.

Hence one might suggest that the function of a common use of 'knowledge' is connected to relative rather than absolute notions of justification; that it serves to preserve the sense and reference of critical terms in a domain of discourse; that this is effected through the preservation of the bodies of belief and convention which direct the use of the terms in question; and that these bodies of belief and convention constitute the contextual features of this ordinary use which systematic scepticism has traditionally been thought to place unjustifiably at risk. On the account suggested here, it can be seen that, since conceivably one might have held to other beliefs and conventions, the sceptic is not merely mischievous, but justified in asking why these, in particular, are held; but since what is at risk is not one or more specific beliefs so much as an accustomed way of acting in the world, resistance to the sceptic is not mere dogmatism, and the task of providing further justification is more

complex than it was when the sceptical challenge could be answered within that way of acting.

A formally similar answer occurs to Rescher, who suggests that we cope with the sceptic by

> ... recognizing that the things one rationally accepts are not of a piece. Specifically, it is necessary to give careful heed to the distinction between <u>theses</u> on the one hand and <u>methods</u> on the other..... we justify the acceptance of specific theses because (ultimately) they are validated by the employment of a certain method - on our view the scientific method as codified in the coherence approach.[5]

His account of the significant distinction and context is not, of course, quite like mine; but it shares the concern with the significance of notions of relative justification, and the concern that we see clearly that to challenge a context of relative justification is to enter into a debate of a very different kind from that conducted within that context.

I shall henceforth use the expression 'way of acting', used informally above, as a technical term for a context of relative justification, taking it, in a way suggested by my imaginary example above, that the identification and explanation of objects and events in a way of acting is in terms of an interactive body of belief and convention, which might be said to be constitutive of a way of acting in that its defence introduces a significant discontinuity into the justificationary

regress.

While my imaginary example provides the occasion for the introduction of the term 'way of acting', it is not offered as a satisfactory sample. One can readily see that involvement with motor cars, were this accepted as a way of acting, would be neither distinct from other ways of acting at the same theoretical level, nor discontinuous with ways of acting at other theoretical levels. The notion of a way of acting can be in some degree improved by seeing why this is so, and that there can be potential ways of acting to which such criticism does not apply.

The first criticism is that activities involving motor cars are not distinct in terms of the beliefs and conventions they involve. Many of the propositions relevant to activities involving motor cars would be equally relevant to activities involving trains and bicycles; and though to a lesser degree would overlap with the propositions which were in the same sense constitutive of the contexts in which most of my other forms of activity were conducted. The second criticism is that, since the mechanical principles in question in such a case are specific examples of more general law-like statements, and the conventions in use are specific applications of more general principles of conduct, the

context of activities involving motor cars is not in any important sense discontinuous with more theoretical levels of discourse. Hence the notion of a way of acting, so far as it depends for its substance on this example, may be vacuous, and in any case cannot support claims about the loss of sense and reference, of the kind made above.

The seriousness of the first criticism depends upon the length to which it can be pressed. Nothing important happens if the context is required to embrace motor cycles as well as motor cars, and considerable harmless extension beyond this seems possible. However, if it could be shown that such a way of acting was not distinguishable, in the sense of being constituted by a distinct body of belief and convention, from any other way of acting, then 'way of acting' would be perfectly general, as I take 'form of life' to be perfectly general when used to refer to some totality or unity; and the entire weight of the notion would fall upon the notion of a distinction between theoretical levels.

Pending further discussion, a case, of a preliminary sort, for saying that the notion of a way of acting cannot be extended to total vacuity can be based on those ways of acting which provided the easiest basis for the discussion of constitutive rules in the past:

i.e., games such as chess. If playing chess can be defended as a distinct way of acting in the sense so far suggested, as I think it clearly can, then there are at least two ways of acting, and the expression is open to the kind of use I have given it. However, the question how ways of acting are to be distinguished that are more significant and less stylized than chess remains an important one.

*Prima facie*, the second criticism is directly effective against my present line of argument. If the path of justification leads from appeal to a mechanical principle in support of a judgment that a particular situation exemplifies that principle to appeal to a more general law in support of the particular mechanical principle which is held to be an application of that law, there seems to be no reason to say that the path is discontinuous; and the same can be said of a path leading through conventions of increasing generality.

Briefly put, a possible answer is as follows. From the fact that empirical beliefs and conventions are interactive at a certain theoretical level, and both can be presented as derivable from more general beliefs and conventions, it does not follow that these more general beliefs and conventions are similarly interactive. We may take empirical beliefs about motor cars into account

in formulating conventions for their use: we might, for example, determine speed limits in part by reference to tables of minimum stopping distances at various speeds. The tables we use might be explainable as instances of more general laws, dealing perhaps with friction or the conservation of energy, and speed regulations might be explainable as as instances of more general conventions of regard for the welfare of others; but it does not seem that these groups of relatively general and fundamental claims are in any significant way interactive. Moreover, if, as this sequence seems to suggest, our empirical defence path might lead us eventually to propose theories regarding the behaviour of sub-atomic particles, it is not even clear what an interactive relationship between these theories and the most general conventions of behaviour could be like. The notion of a way of acting involved the context of a body of interactive belief and convention, and it cannot be preserved when the interaction is lost. Hence the distinctness of ways of acting at different theoretical levels, as evidenced by discontinuity in the regress of justification, can be maintained.

A way of acting, as so far discussed, offers a unit distinct within, yet related to, the general body of our activity and discourse. Consistent with the Winchian model I have proposed to use - and indeed with the Davidsonian view of the identity of objects of

propositional attitudes mentioned in Section 2 - a way of acting includes ways of identifying objects in which are established both sense and reference in a context-relative way. A way of acting provides a context of relative justification, and turns the problem of absolute justification into the problem of justifying ways of acting. It is no doubt because of this last point that Quinton, in the discussion already quoted, sees claims about distinguishable kinds of knowledge as yet another sceptical challenge. Taking the logical gap between physical object talk and sense impression talk as an example of a justificatory discontinuity, he proposes solutions. Most interestingly, he mentions, as one concerned with the gap, Waismann, of whom he says:

> He sees the gaps as indicating the lines of fracture between various strata in language within which, but not between which, strict logical relations obtain. Rejecting intuitionism and reductionism, he allows that evidence from one side of a gap can support, in an unanalysed way, hypotheses about what lies on the other side of it.[6]

Ways of acting are not solely strata of language, though given the significance which I have attributed to the systems of descriptions through which reference is secured, it is clear that I shall employ claims about such strata in distinguishing ways of acting. Thus an account of the logical and conceptual relations which obtain within and between ways of acting will be the main part of the development of my account of ways of acting and forms of life, commenced later in this chapter, and

developed in Part II.

Before attempting this development, I shall consider an objection which might have force against even the preliminary outline of the notion of a way of acting so far offered, by attacking the assumed relationship between belief, sense and reference on which it rests.

## SECTION 8 An objection to the notion of ways of acting based on extensionalism involves implicit abstraction, and an inadequate notion of individuation.

Essentially, the objection I wish to consider, while allowing some change in the sense of expressions as a consequence of change in the context of belief, denies the alleged effect on reference. It might suggest that although in some sense of 'means', a sentence like "I took my car to a mechanic, and now it's running well" means one thing to me if my background beliefs are scientific and another if they are magical, this does not show that the things or events to which terms like 'car', 'mechanic' and 'runs' refer vary accordingly. Indeed, it is a condition of our understanding statements like that made in the first part of the preceding sentence that we suppose reference to be preserved. If we could not take it to be the statement that different beliefs were held with regard to the same things, we would not know how to take it at all.

Very much this point is made by Israel Scheffler, in terms of distinctions between category and hypothesis, and between individuating and categorizing. He says:

> ... suppose two theorists with non-overlapping category systems.... we are not .. driven to describe them as necessarily observing different things ... A category ... both

> delimits items to be recognized, <u>and</u> sorts them ... the same form of individuation into separate things may be shared, over a given range, by two non-identical categories.... Nor does anything prevent us from trying to explain the <u>genesis</u> of categorizations differing wholly in <u>individuation</u>: we may characterize their divergent items as having been differently composed out of elements admitted by us.[1]

So, both the scientific and magical view of things might, though they involve different hypotheses, employ the same categories; even if they differ in categories, they may agree in individuation; and even if they don't do that, we may still be able to cope: one party can learn to recognise the individuals the other recognises, even if this seems to him an odd way to divide up the world.

This line leans heavily upon the distinction between category and hypothesis. Scheffler says:

> Conceptualization .... links up with the notion of <u>category</u> and, also, the very different notion of <u>hypothesis</u>. The very same category system is, surely, compatible with alternative, and indeed conflicting hypotheses .... Conversely, the same set of hypotheses may be formed compatibly with different category systems .... Simply to set up an alphabetical filing system for correspondence is not yet to determine how tomorrow's correspondence will need to be filed. Conversely, to guess that the next letter will need to be filed under "E" or "L" is a prediction that may be made whether or not we have a place for "X" in our system.[2]

There is much in this that is reminiscent of Winch's Wittgensteinian distinction between agreement in a form of life or language, and agreement in opinion, in

which the former is prerequisite for the latter, but does not guarantee it. Agreement in a category system is consistent with disagreement over distribution; agreement or disagreement over distribution requires agreement in a category system, though, on Scheffler's account, we can see what is going on even in the case of non-overlapping category systems providing there is agreement in a form of individuation. The dispute between Scheffler and Winch may be presented as a dispute over the notion of meaning. For Winch, meaning is primarily sense, established in a language structure; for Scheffler, it is primarily reference. He summarises the argument from meaning which he opposes thus:

> A category term .. derives its meaning from its role within a language system; its meaning is not an atomic somewhat, mysteriously linked to its physical character. But then to alter the language system in any way is to alter the meaning of the category term.[3]

He argues against this view, saying:

> In sum, though connotative meaning is relative to language, it may remain fixed through alterations of belief; opposing theorists may employ selected synonymous terms, and indeed the same language system, though holding conflicting beliefs.[4]

Since Winch does not think that language structures forbid disagreement in opinion, this conclusion is not one which he would be obliged to deny, though it is no doubt intended to conflict with such views as his; but it does not express Scheffler's main point.

He goes on to suggest that the conclusion is strengthened by thinking of meaning as reference rather than as sense, identity of reference surviving, he suggests, both changes of belief and changes of synonymy relations.

One might ask at this point what it can come to to say that a category system is "consonant with any distribution which the data may form in actuality"[5], if category terms are defined extensionally, for it is not easy to see how the distinction between the definition of categories and the actual distribution of individuals is to be drawn, and this distinction carries much of the weight of Scheffler's position. He does say that

> ... the classification of any scientific proposition as definition or empirical truth is .. largely a matter of choice in the interests of convenient systematic presentation for the purposes at hand; nothing beyond referential relations need, in any event, be taken into account.[6]

But the problem is not whether extensionalism can cope with both definitions and empirical truths; it is rather that by meeting the question above by calling on a notion of purposes at hand, this line makes the notion of purposes at hand so powerful that Winch, for whom such a notion is centrally significant, or I, who build it into the notion of a way of acting, begin to appear as champions of objectivity rather than as its foes. Some further consideration of Scheffler's argument may help to make this point.

The only hypotheses he considers as possibly expressed through a category system are of the form 'Category C has n members'. These, he says, affect our judgments of the usefulness of a system; and we may revise the system if they prove false, say, by dropping the "X" in our letter filing system. He says:

> Acceptance of the scheme may be said, then, to be based on the hope that no category ... will remain empty; yet the scheme itself does not prejudge the satisfaction of this hope but rather allows perfectly well for the expression of its frustration as a certain sort of distribution. 7

Now, it is not immediately clear how this state of affairs might have come about: whether because we have received no letters from Xanthippe or Xerxes, or no letters about xylophones or xebecs. This is not a quibble: the expression "an alphabetical filing system for correspondence" does not adequately identify a system of categories, but only a set of labels that might be applied to categories. It is thus fairly easy to think that the only respect in which category systems might express hypotheses is the one that Scheffler suggests. The example supports a treatment of a category system as a set of sets, in which we can find out anything we need to know about the set - indeed, all that we can know in the context provided - by counting members. We find, of course, that we cannot learn, just from the statement that a domain of particulars is organized into sets on an

unstated basis, what the cardinal number of any set will be, whether any set will be null, or of which set a randomly selected particular will be a member. We are therefore to infer that category systems do not embody or prejudge hypotheses. It is also easy to see the creation of category systems as arbitrary, and to suppose that "... the same sort of arbitrariness, if such it be, characterizes all intellectual creation".[8]

Scheffler makes his comments on the notion of a category system abstracted from all considerations of its use: from questions concerning its acceptance, and from questions concerning the purposes at hand. But an abstracted notion may not be an independent notion; and Scheffler's conclusions run counter to the background knowledge which we need in order to understand him. We know what an "alphabetical system for filing correspondence" is, and we can see the sense in which it is arbitrary, and the distribution of letters is contingent. But this is because we know what correspondence is.

We know that the alphabet can be of use in filing correspondence, and we might suspect that it would be odd to keep empty files, while other files bulged awkwardly; and taking this for granted, we might be drawn into Scheffler's little world, where what really matters about correspondence is the number of items

that wind up in each file. But if we begin to reflect upon what counts as an item of correspondence, or its content, or the point of filing it, or why a folder with a letter of the alphabet inscribed on it should count as a file, or what it was for a file to count as a category, we might begin to see the filing system as a partial expression of the body of belief and convention within which the activity of exchanging correspondence is carried on. These beliefs and conventions are not changed in the light of contingent features of our actual correspondence, because nothing that counted as correspondence could conflict with them; and it is only because we understand them that 'alphabetical filing system for correspondence' makes any sense to us. It may be reasonable to abstract the notion of a filing system from the context in which it is a meaningful one in order to make certain points about it; but the point that the system is independent of the intensional features of the context is not one that can be made in this way.

Scheffler's use of abstraction would be less worrying if the extensionalism which supports it were itself independently supported; but it is not. The argument which supports the claims on constancy of meaning, particularly meaning as reference, quoted above, is persuasive and perfunctory. He says, in effect, that we should not think of category systems as sets of

boxes so arranged that we cannot change one without changing all the others. He says that boxes that are similar in some respect before a change may remain similar, in that respect, after the change; and suggests that we will be more convinced of this if we stop looking at boxes, and look instead at the things that go in them. But it is not at all clear that this is so. Even if the similarity in question is similarity of content, as one suspects it is, one would need to be assured that the similarity was not being preserved by putting things in the wrong boxes; and how one is to be assured of this without attending to the boxes as well as the things is unclear.

I take this to be the view of the commentator who remarks of Scheffler that:

> .. he does not produce any substantial arguments for this confidence in the referential constancy of scientific terms.... References do, after all, depend on senses ... It seems quite open for someone of relativist inclinations to maintain that the referential values of scientific terms are as theory-dependent as their senses, varying whenever the theory in which they are used is modified.... there will always be new and as yet unexamined instances ... for them to apply to. And this makes it difficult to see how their referential values can be fixed by 'agreement on particular cases' - we obviously cannot fix the referential value of such a scientific term by simply laying down that it applies to each and every one of a given number of identified individuals. 9

Another aspect of Scheffler's use of

abstraction arises directly from his distinction between a category system itself, and its acceptance. Having made this distinction one can say that any hypothesis bearing on the usefulness of a system of categories is is to do with its acceptance, and the system itself can be anything at all. Presumably, a system of filing all letters under "A" would be all right <u>as a system</u>, though it would be acceptable only to one who hoped (or hypothesised) that all his letters would be fileable in such a system, or that he would get so few letters that it did not matter. But all that this makes clear is that the notion of a system of categories, abstracted from all considerations which bear upon its acceptance, is not a significant notion.

As an alternative way of making this point, one might say that Scheffler's use of the distinction between category and hypothesis works only for unaccepted systems of categories; to consider a system as accepted is to consider it not in itself, whatever that may mean for an in-principle-arbitrary artefact, but as a partial expression of a way of acting, and thus as a partial expression of a body of belief and convention. Of course there is a distinction between category and hypothesis: this is just to say that we can draw upon category systems for the terms of statements which can be true or false, a proposition which no relativist has

denied. And these statements may be just those concerning the distribution of particulars over categories that Scheffler suggests, although not if we were to suppose that category-terms were extensionally defined. On the whole, I think that, far from providing a convincing case against the view that beliefs and category structures are related in the way that my account of ways of acting suggests, Scheffler reinforces it by showing how important the role of purposes at hand and the point of activities is, in providing a context from which intelligible abstractions can be made.

The other main line of his argument concerned the distinction between categorization and individuation, and presented the latter as significantly independent of the former. We may, he suggests, allot the same individuals to a variety of category systems. Again, a commonplace truth covers a major mistake. Of course, we might vary our filing system for correspondence in a number of ways, but keep in it the same letters. But all that is happening here is that we are considering variations in a sorting system applied to something which is already individuated in terms of categories. We can vary our use of the alphabet; but a letter of the alphabet is not one of the categories which allows individuation to work here. I understand the ways of acting in which corresponding and filing correspondence

have a place; I know, therefore, how to carry out the kind of individuation Scheffler's example requires; but this is not clearly distinct from my understanding of relevant categories.

I could not sort items if I could not individuate them; but it is not clear that I could individuate them without some idea of how they might be sorted. I individuate items in the first place as things of a certain sort - roughly, as correspondence relevant to certain ways of acting. I must know about the ways of acting in order to perform the individuation. I must know about the general business of correspondence, so that I do not clog the cabinets with lunch-wrappers , or treat each page of a letter as a separate individual, or include the boss's wife's shopping list, or my own private love-letters. But if I know this much, I also know a good deal about the way individuals are to be classified: I know why categories like "vertebrate" will not do; I know why classifying items by the colour of their paper or ink is unhelpful; I know which of their features and relations give point to the activity of filing them; so I know at least something about the ways in which they can be categorized.

More generally, it will not do to suppose that we might pick things out just as things, with all

questions of sort yet to come. First one must try to understand what 'thing' might be doing in such a claim. Does it indicate a reliance on the most general sort, or an attempt to use a notion of an individual which is "pure": something recognised as an individual without being sorted at all, which must mean without being characterized in any way, since to predicate anything of it, say, being red, would entail that it was at least of one sort, say, extended or visible?

Of course, I can individuate without being sure of, or even considering, all the categories to which the individual might belong. I can pick out something in the fog, and think it is a truck; but perhaps it is a building or a clump of trees, or perhaps just a trick of the light. In any case, I may be sure that it is a public object - that is, I pick it out *as* a public object. I may be wrong even about this, but I do not think that matters. The question is not whether I must be certain, or right, but whether I can pick something out without picking it out *as* something. I cannot pick it out unless I can predicate something of it, however tentatively, and in that case I must pick it out as an individual of some sort - of the sort of which such predications are possible.

This discussion perhaps seems to suggest that

the most generally successful account of individuation would require the most general categories, so that talk of picking out things as such may in the end be possible. This is not so. There is the general point that while more general categories are easier to apply in cases where specific detail is unclear, they compensate by being more heavily theory loaded. It is easier to pick out the thing seen in the fog as a public object than as a truck; but one cannot learn to use 'public object' as simply as one can learn to use 'truck', and it is no advantage to Scheffler to clutter up individuation in this way. There is, however, a more important point.

Scheffler's general position requires that we be able to say things like: "This is the same individual that last week was categorized in one way, and is now categorized differently": it expresses his opposition to the notion that by shifting categories we might lose our ability to refer to the same individual. He therefore requires a notion of individuation which is strong enough to imply reidentifiability. This requires the use of relatively narrow and specific categories, and specific information about the sort of thing in question. I need to know that a gap of a week between sightings does not rule out their being sightings of the same giraffe, but may rule out their being sightings of the same may-fly.

Generally, the wider the category, the less of this sort of information it provides. Animals live for varying periods, and some of them change in ways that others do not. If I know of two sightings only that they were sightings of animals, I do not know how to decide whether they were sightings of the same animal. For a very wide category, like public objects, the case is worse. Some public objects, like light flashes, may be of very brief duration; others, like mountains, persist for a long time. For some, like billiard balls, the observer can change position to some extent without affecting the question of reidentification; for others, like mirror-images, he cannot. It is easy, in the sense previously mentioned, to pick something out *just* as a public object. It is not so easy to pick out the same public object again - for this one needs finer categories.

So not only does one need categories in order to individuate; in order to use 'same individual' as Scheffler must one needs fairly narrow and specific categories. Even the attempt to deal with this by supposing that referential force could be established by agreement on particular cases, criticised by Papineau in the passage quoted above, will not do, for it also relies ultimately on the notion that different judgments apply to the same individual.

Part of the trouble is that Scheffler does not take the notion of a system of categories seriously. In themselves, category systems, as he explains them, are purely formal and essentially arbitrary artefacts. It is not clear that one could design any set of rules which must be satisfied by anything that was to count as a system of categories. This, on my account, is because all the systematic features of category systems have been left behind in the context from which he abstracted the notion. The point of having filing systems is part of the context in which such systems are accepted, and from which the "system itself" is abstracted. That what is so abstracted is not in any serious sense systematic is convenient for his argument, at least as long as we continue to draw upon background understandings for persuasive evidence that there is something significant to be said about the system in itself and unaccepted.

The general view I wish to express is that to see what is systematic in a system of categories requires some understanding of the way of acting in which that category system has a point. It is a view which Scheffler himself seems to endorse when he says that "... meaning is not .... a function simply of the physical constitution of terms; it depends also on the human context within which they are used"[10], though he has

little to say about this context and the nature of its role.

The importance - indeed, the over-riding importance - of the background context of human activity can be indicated by a further comment on Scheffler's thesis of the indeterminacy of propositions as empirical truths or definitions being resolvable as a matter of convenience for the purposes at hand. I have already suggested that the indeterminacy thesis clashes with his extensionalism, the latter requiring at least some category-membership statements to be definitions; but there is another way to make the point that problems created inside his account require a notion like that of ways of acting for their treatment.

The indeterminacy thesis is a judgment on a second-order categorization issue. If we re-apply Scheffler's general view of categories at this level, it seems to say that 'empirical truth' and 'definition' are labels for essentially arbitrary divisions, correctable in the light of distribution. But this requires a doctrine of natural kinds applying to propositions - that they should be truths or definitions in some sense that allows us to check our categories against the facts, and this is what the indeterminacy thesis does not allow. It blurs the notion of a matter of fact, as

distinct from a matter of definition, by making it relative to purpose. Perhaps not the only, but in this context an obvious way to reduce the resultant tension, is to accept that systems of categories are also expressions of purpose, and in that sense are not non-propositional. They can be presented as non-propositional only by abstracting them from the context in which they are used; the benefits of such a process could be significant, but they could not include a refutation of relativism based on the non-propositional nature of category systems.

Thus it seems that my suggestion, in Section 7, that sceptical pursuit of the justificationary regress revealed ways of acting, based on interactive bodies of belief and convention, and constituting a significant context for relative justification by supporting particular possibilities of reference, is not refuted by such arguments as Scheffler advances; and is to some extent supported when one sees to what extent such a background "human context" is required to explain the point of his discussion, and the extent to which he presupposes the availability of category systems which provide the descriptions necessary for the identification and reidentification to support his use of 'individual'. To the extent that this is so, my first purpose, to show that some sort of federation of language-game-like

systems, perhaps countable as a form of life, can be revealed as part of our ordinary practices by a consideration of justificationary regress, is sufficiently served.

What is next required is a more serious account of the sub-structures and relations which on this view constitute a form of life. I turn, in the next section, to some considerations more directly preparatory to this more formal enterprise, based on a consideration of the account of constitutive rules given by Searle. I have already suggested, in the course of my discussion of Winch, some reason for regarding Searle's account as useful.It is a further point that his concern with human contexts covers the gap left by Scheffler, so that his discussion might be expected to cast light upon those questions about the point of activities, the working out of purposes, and the role of conventions, that I have regarded as significant. It could also be noted, following the discussion immediately above, that the indeterminacy thesis which I have argued is a point for concern in Scheffler's story, is reflected in Searle's by the suggestion that constitutive rules have an analogous appearance of indeterminacy between rule, tautology and empirical truth. One might reasonably hope that this coincidence is more than coincidental.

SECTION 9 Searle's account of constitutive rules provides a context in which the issues so far raised can be related and developed.

I have twice found occasion to refer to J.R. Searle's account of a category of rules which he calls constitutive: once in discussion of a criticism of Winch in Section 5, and in the previous section as involving a notion of indeterminacy analogous to the indeterminacy between empirical truth and definition mentioned by Scheffler. I shall attempt, in Part II, to show a form of life as a system of ways of acting, a way of acting as involving a system of what I shall call interpreted games, and an interpreted game as a system of rules. Searle's constitutive rules I shall use as the basis of an account of these foundational rules. I shall criticise Searle's account, not on the ground that the general notion of constitutive rules is unsatisfactory, but on the ground that it requires the support of a much more detailed account of the theoretical structure in which it occurs.

In this section I consider the account which Searle offers of this category of rules, hoping to clarify as far as possible their nature, relations and function. He distinguishes constitutive rules from regulative rules on the one hand, and on the other from

conventions which may or may not be "realizations" of "underlying constitutive rules".[1] I do not find the role of 'convention' in his discussion at all clear, and shall later suggest a way in which the term might be employed, and an alternative way of talking about the "realization" of a constitutive rule set; hence at this point I shall follow Searle in supposing that the nature of constitutive rules is best shown through a contrast with regulative rules.

Searle initially offers two ways of distinguishing between these rule-types. First, he suggests that ".. regulative rules regulate antecedently or independently existing forms of behaviour .... constitutive rules do not merely regulate, they create or define new forms of behaviour".[2] This he subsequently expresses by the formula "The creation of constitutive rules, as it were, creates the possibility of new forms of behaviour".[3] Second, he says that:

> Regulative rules characteristically have the form or can be comfortably paraphrased in the form "Do X" or "If Y do X". Within systems of constitutive rules, some will have this form, but some will have the form "X counts as Y", or "X counts as Y in context C".[4]

The formula he offers for this characteristic of constitutive rules is "Constitutive rules often have the form: X counts as Y in context C".[5]

Having offered these formulæ, he proceeds to

clarificatory discussion, and attempts to express the meaning of the first formula in the formal mode. This he first gives as ".. where the rule (or system of rules) is constitutive, behaviour which is in accordance with the rule can receive specifications or descriptions which it could not receive if the rule or rules did not exist".[6] This, after some discussion of examples, is reformulated as ".. constitutive rules, such as those for games, provide the basis for specifications of behaviour which could not be given in the absence of the rule".[7]

Constitutive rules so characterized are contrasted with regulative rules in two ways. He says that "Where the rule is purely regulative, behaviour which is in accordance with the rule could be given the same description or specification (the same answer to the question "What did he do?") whether or not the rule existed".[8] He also says:

> .. regulative rules often provide the basis for appraisals of behaviour, e.g., "He was rude", "He was immoral", "He was polite", and perhaps these appraisals could not be given unless backed up by some such rules. But appraisals are not specifications or descriptions as I am now using these phrases.[9]

Thus, putting aside for the moment any questions about material or formal mode, one might suggest two more distinctions between constitutive and regulative rules to follow those initially given: third, that

constitutive rules establish new expressions for the description or specification of behaviour while regulative rules do not; and fourth, that constitutive rules provide the basis for description or specification, while regulative rules provide the basis for appraisal, or perhaps evaluation.

I suppose it is the third distinction which Searle takes to be the first expressed in the formal mode; and I further suppose that his reason for so taking it is a view similar to that which I earlier quoted from Winch: the view that social relations and the ideas men have of them are identical. On such a view, and given that the rules Searle considers to be constitutive are constitutive of social relations, the move from speaking of forms of behaviour to speaking of specifications of forms of behaviour might present itself as a move from material to formal mode. But this is a debatable view, and one cannot generate the third distinction from the first merely by substituting mention for use; hence I shall regard the four distinctions as distinct, rather than as reformulations of each other.

Searle illustrates the first distinction by offering chess and football as examples of new, rule-constituted activities; and rules of etiquette - his first example has to do with table manners - as

examples of rules regulating a pre-existing activity - for the example I mention, presumably, eating. To the extent that this establishes a difference, it is not clear that it is a difference of the right sort. We might be supposed to say: nobody played chess before the game of chess was invented; nobody dined before that game was invented. But people did eat, and did not push 32 things around a 64 square board. So dining is a matter of eating in conformity to regulative rules, while playing chess is a matter of behaving in ways made possible by constitutive rules. But this is a contingent matter. People might have been nourished in some other way, and taken up dining as a game; they might have spent time pushing things around a board, then introduced rules to regulate this common practice.

How much does the contingency matter? In the chess case, the contingency is important, since it does not seem to matter at all what the contingent state of affairs was before the game was invented. If people had pushed things around a board, as an art form or a kind of doodling, we would not say that what they then did was the same as what I now do when I play chess. Why, then, should we want to say that a certain similarity - putting more or less nourishing material in the mouth and swallowing it - suffices for us to say that the caveman gnawing a bone is doing the same thing as

someone attending a ceremonial dinner, though doing it, in a certain sense that a rule of etiquette defines, less well? It seems we should be able to say, of someone who attended the dinner and behaved like the caveman, not that he was doing what the others did, i.e. eating, but doing it badly, but rather that he was not doing what the others did, i.e. dining, at all; and this is very like what we would say of one who, being invited to play chess, merely pushed the pieces around the board without regard to the rules.

This leaves the crucial notions of antecedence and independence used in the first distinction obscure. What seems to me the most potentially troublesome aspect of the obscurity concerns the question whether activities have antecedent and independent status absolutely or relatively. Searle seems to suggest the former: chess is an independently constituted activity no matter how we look at it, while rules of etiquette are dependent upon some antecedent activity no matter how we look at them. But the discussion above shows that this is not clearly so. Hence it seems possible that we might find a hierarchical structure of activities, in which 'constitutive' and 'regulative', applied to rules or systems of rules, identified not some intrinsic feature of the rules, but rather their place in the hierarchy relative to other rules which were their "neighbours".

To decide the point one requires a clearer idea of the structure in which rules or systems of rules are components, and one requires this not in addition to, but as part of the account of constitutive rules.

The point of the second way of drawing the constitutive/regulative distinction, in terms of the form taken by rules, is unclear; indeed, it seems at times a complete red herring. The forms suggested are only loosely related to the status of the rules. Searle says that form is not a formal criterion of status, and concedes that the forms can be interchanged. Searle's interest in the forms characteristic of rules is no doubt related to his concern with the forms of ordinary language; but this is not obviously significant when one is concerned, as I am, to generalise and systematise the notion of a constitutive rule as far as possible.

However, although the notion of a natural or characteristic form of utterance is of no great importance to me, the notion of a standard form may be used as a way of recording aspects of the function of a rule-type. As will emerge in the next section, where I note the view that the constitutive rule's function in licensing prescription can be manifest in its form, and in my discussion in Part II, I think that there are significant points to be made in this way. I do not, however, think that

Searle makes them, so this second kind of distinction fails to clarify the notion of a constitutive rule.

The third distinction makes the difference between constitutive and regulative rules turn on the way in which it is possible for behaviour to be described or specified. There does seem to be such a distinction to be drawn. We do say "He threatened a knight fork" rather than "He moved the thing carved in the shape of a horse's head in such a way that ...", thereby employing descriptions or specifications that depend for their sense on the rules of chess; we do say "He slurped his soup" rather than "He misdined", thereby employing terms antecedent to or independent of the rules of dining, or of manners at table. This is an interesting approach, since it connects the account of a constitutive rule to the matter, of great importance in the last section, of the descriptions under which individuals can be identified, and since it reintroduces that concern with the relation between technical and ordinary vocabularies which was important in Section 5. However, the distinction between specialized, game-based terms and antecedent or independent terms has its own problems. What Searle says about constitutive rules does not solve them; rather, the problems show how incomplete the account is.

As an example relevant to this point, we can anticipate a criticism of Searle to be discussed in the next section. The criticism involves the contention that specialized descriptions or specialized specifications established by the constitutive rules of games are in principle eliminable, and replaceable by independent terms. While I shall dispute this claim on at least one of its possible interpretations, it is clear that it would, if successful, completely destroy the attempt to characterise constitutive rules by reference to the necessity for technical vocabulary in some cases, so the non-eliminability of constitutive-rule-based specifications must be argued.

An argument might be based on the Winchian claim that activities would be specified or described correctly _only_ in those terms, and that what could be described or specified in other terms would not be _those_ activities. But again, we lack the information and understanding of the systems of rules within which we are supposed to be arguing, which we would require properly to understand or evaluate such a claim. The mistake, if it is a mistake, of describing a game of chess without using any of the specialized vocabulary of chess, is not like the mistake of describing a cat as a dog. In the latter case, 'cat' and 'dog' share a context of rules in which the mistake can be identified. In the former case, the choice is not between terms, but

between contexts of rules; and we need to know in what context of rules this choice is to be made.

Although the fourth basis of distinction, between specification or description and appraisal or evaluation, is not offered by Searle as central, being rather slipped into discussion, it seems to me that this is where the distinction starts to bite, as one between the rules under which we identify a performance in a sense relevant to the questions earlier raised in discussion of individuation, and those under which we evaluate it as a good, bad or indifferent performance of that kind.

I say that this is the way it seems to me; I am not sure that this is the way it seems to Searle. He says: "Constitutive rules constitute (and also regulate) an activity the existence of which is logically dependent on the rules"[10]; and in a footnote on the same page he comments on the scope of constitutive rules in a way that suggests that one who makes a legal chess move which is part of a plan to throw the game is not playing chess because he is in breach of a constitutive rule. A rule which both constitutes and regulates cannot be distinguished as providing only specificatory and not evaluative descriptions; and if someone can make a legal chess move, not inadvertently, but by intent in the course of

a game, and yet not be playing chess, it seems that the chess terms which we might use to refer to this move both do and do not identify or specify what it is that he is doing.

These odd consequences are, I think, to be explained as due to the extreme sketchiness of Searle's concern with constitutive rule *systems*. He says that rules come in systems, in which any particular rule might be central or peripheral,[11] and even suggests that the form '... counts as ...' might characterise systems rather than individual rules: "... acting in accordance with all or a sufficiently large sub-set of the rules does count as playing baseball".[12] But, in the footnote under discussion, he allows constitutive force to rules which are extra-systemic, or perhaps inter-systemic, and thus clouds whatever clarity the notion of a system might have gained from the comments quoted immediately above, and from the suggestion, also in the footnote, that he includes in the notion of acting in accordance with the rules, those rules that make clear the aim of the game. Again, it is only by providing a more detailed account of Searle's thesis than he has offered that one might clear up the problem.

Generally, I have argued in this section that the notion of constitutive rules, as Searle presents it,

is unclear, and requires the support of a much more detailed account of the theoretical system in which it occurs. I shall attempt such an account in the next section and part. Before doing so, however, it seems desirable to offer some explanation of why, in the face of the difficulties I have suggested, I take this to be a promising course to follow.

In exposing my notion of ways of acting to the criticism implicit in Scheffler's comments, I argued that his acceptance of indeterminacy between definition and empirical truth was a weakness in his position. I argued that although category systems were not forced upon us by natural distributions, they were not arbitrary in the sense of not expressing or depending upon propositions or beliefs; rather, they express or depend upon propositions or beliefs forming the point of the activity in which they have a place. One might say: category systems can be called arbitrary if all that is meant by this is that they cannot be justified or explained by reference to the domain to which they are applied, and perhaps also in the sense that there may be no clear answer to the question "Why should there be an activity with that point?"; but only in those senses. All these claims have their counterparts in Searle's account of constitutive rules.

For Searle, at least some category systems would be the expression of constitutive rule sets; he requires the rule sets to be arbitrary in the first of the acceptable senses; and, given that there is no clear answer to the question "Why should there be such a game as chess?", would take them to be arbitrary in the second. He provides a context in which concern with the function of certain kinds of specificatory or identifying description can be related to concern with the relation of technical and ordinary language. He allows that constitutive rules can have the appearance of rules of procedure, or analytic truths, or definitions. He does not say that they can appear as empirical truths, but clearly they can: the statement that checkmate in chess is achieved in such and such a way can be construed as a statement about what chess players do.

Thus Searle's account of the notion of a constitutive rule, though not yet sufficiently clear, does relate in a promising way to the issues with which I have been concerned, and the position which I propose to develop.

SECTION 10 The account of constitutive rules is made more detailed, and issues clearer, through consideration of a critical comment on Searle.

In "Constitutive Rules and Speech - Act Analysis", J.R. Ransdell's general concern is to argue that constitutive rules do not provide a basis for the analysis of speech acts generally.[1] This is not to my immediate purpose, though I shall later suggest that it involves a mistake which a better account of the structure of a form of life allows us to identify. However, as a preliminary, he attempts an improved version of Searle's account of constitutive rules, and it is with this part of his paper that I am now concerned. His account allows a much more detailed and suggestive consideration of questions about the form of constitutive rules, stresses the importance of the relations between the language of a game-constituted context and that of antecedent or independent activities, and can be made to show the relation between these two matters.

He begins his account of the notion of a constitutive rule by looking at the categories the rules establish rather than the rules themselves; that is, he first directs his attention to "game-terms", this being his expression for referring to those expressions which, according to Searle, are available for the

specification or description of behaviour only because of the existence of constitutive rules. Taking it that ".. games like baseball and chess are paradigm cases of systems of CR's", (that is, constitutive rules), he goes on:

> A fundamental logical point about a game such as baseball is that, although the playing of the game is a physical activity, consisting of interactions between physical objects, these things usually appear _in_ the game only under a special game-interpretation. Thus, for example, the persons who participate do so as Players, considered individually, or as a Team, considered collectively; the ball used is a Baseball; the wooden stick is a Bat; the plot of ground is a Field; the bags located at certain places on the ground are Bases; and so on. All of these terms, capitalized here to indicate that they are special game-terms, have special meanings within the game and are in fact specially defined for the game in the rule book.[2]

He distinguishes between the connotation and the import of these terms, the connotation being "... the characteristics a thing, person or action has that justify the application of a given game-term", and the import being ".. the _logical effect_ of that application".[3] The import, or logical effect, is "... constituted by _rules_ of permission and obligation which apply to whatever object the term is applied".[4] He allows three different kinds of connotation, by distinguishing three different bases on which game-terms are predicated of particular things:

> ... some are predicated on the basis of the physical characteristics of the objects they qualify (e.g., 'Bat' is predicated of a given object on the basis of the object's being of a certain size, shape, type of material, etc.) Some are predicated arbitrarily (e.g., 'Player'

> is assigned to persons selected by some criteria logically external to the game itself ... ). And some - the major part - are predicated on the basis of prior game-interpretations (e.g., being a Runner on First Base is conditioned upon being a Player who has just previously been At Bat, has been Walked or else has got a Hit, and so on).[5]

On this basis, he distinguishes two varieties of the '... counts as ...' pattern suggested by Searle:

(1) 'X counts as Y', in which 'X' is replaced by the connotation of a game-term and 'Y' by the game-term itself; and

(2) 'X counts as Z', in which 'X' is replaced by the connotation and 'Z' by the import ofa game-term which does not itself appear, but which is defined by (2) as a whole.

Searle, he believes, concentrates on (1), but should have concentrated on (2), because

> ... the notion that CR's are quasi-definitional in character is uninformative, even misleading, when the game-term is regarded as occupying the second variable position, since this suggests that the conditions of application constitute the definientia and the game-term in question the definiendum. But in fact the game-term is defined not by what fills the X-slot, but rather by the elements expressed in the X-slot <u>and</u> those expressed in the Z-slot <u>and</u> the conditional relation obtaining between them.[6]

Thus game-terms are eliminable: apart from its mnemonic and practical function, a game-term can be replaced by an expression in the form of (2). Such an

expression "... really expresses a <u>second-order rule</u> relating the satisfaction of a certain set of conditions to the applicability of a set of <u>first-order</u> rules"[7]; and allows us to see that, rather than thinking of constitutive rules "... as merely making certain <u>descriptions</u> possible ... it would be more accurate and more suggestive to say that CR's make certain <u>prescriptions</u> possible"[8]. In illustration, he quotes the following passage from Strawson.

> If one is playing a game of cards, the distinctive markings of a certain card constitute a logically adequate criterion for calling it, say, the Queen of Hearts; but, in calling it this, in the context of the game, one is ascribing to it properties over and above the possession of these markings. The predicate gets its meaning from the whole structure of the game.[9]

He takes this as meaning that it would be inadequate and misleading to throw the weight of identification of the Queen of Hearts solely upon its markings, or solely upon its role in the game, i.e., the import of 'Queen of Hearts'; and says:

> Both are essentially involved in what it is to be the Queen of Hearts, as is the conditional relation between them. To be the Queen of Hearts is to be an object which has such-and-such markings and which is <u>in consequence</u> governed by such-and-such rules.[10]

Hence, he suggests:

> The "X counts as Z" form can be regarded as functionally equivalent to the conditional form "If X then Z", since the satisfaction of certain conditions is represented to be a sufficient condition for the application of certain (first-order) rules.[11]

Ransdell has done a great deal in a short space. He has greatly enriched the vocabulary available for the discussion of constitutive rules; and he has offered a way of tightening the loose and uninformative relation between form and function which Searle presented, without undue reliance on the '... counts as ...' formulation which was, for Searle, such an unreliable instrument. He has proposed a relation between the specialized vocabulary of a rule-constituted context and a prior domain of discourse, in a way that both relates this issue to the proposed second-order structure of a constitutive rule, and shows the possibility of the kind of hierarchical system of meanings previously mentioned. Despite certain criticisms which I think can be made, this provides the basis on which I shall proceed, in Part II, to develop a more detailed account. Here, as a preliminary to that endeavour, I shall note two related issues on which the juxtaposition of Searle's and Ransdell's comments is informative.

The first issue can be approached through Ransdell's distinction between game-terms, which are specially defined for the game through the constitutive rules, and the natural terms through which the game is tied to our background understandings and activities. We may draw a consequent distinction between connotations, in Ransdell's sense, on the basis of their

containing game-terms, and thereby already presupposing certain constitutive rules; or being composed entirely of natural terms. For convenience, I shall call these g-connotations and n-connotations respectively.

Ransdell makes a further distinction, within n-connotations, between those that are related to the game-term arbitrarily, and those that are not. While some distinction of this sort can no doubt be drawn, it is at best a relative one. The physical characteristics of equipment affect the game, of course: one could not play baseball using a stick of spaghetti and a nine pound shot as bat and ball. But, even apart from a more radical point about the interpretation of game-terms to be raised below, it is clear that the range of variation in respect of any physical characteristic which would allow the game to be played is very great, and the point in such a range specified by the regulations governing equipment must be arbitrarily chosen - that is, must turn upon criteria "logically external to the game itself" as much as might the selection of players. Similarly, although players may be chosen for a variety of reasons, it is clear that their physical characteristics are related to the playability of the game in the same way as those of the physical equipment.
Even in the favourable case of chess pieces, where the specific characteristics of pieces are not determined

by the game at all, the choice is not entirely arbitrary, since there are still limits on what would allow the game to be played. Thus the relation between a game-term and some natural terms relating it to an antecedent domain will always be in some degree arbitrary, since the selection of any n-connotation must always be in some degree arbitrary.

This raises a problem about the identity of games. On the account Ransdell gives, the identity of chess is a matter of its being constituted by particular rules, and the identity of a particular rule is a matter of its being a conditional relation between a particular connotation and a particular import. It then seems that by changing a connotation, which we can do within the range of arbitrariness without, one would suppose, effect upon the identity of the game, we must change the identity of the rule; and thus, contrary to our expectation, change the identity of the game. If this is so, then either chess played with different kinds of piece is a different game, or rules with arbitrary n-connotations - and I have argued that all n-connotations are in some degree arbitrary - must have a different status.

Searle appears to take it that it is so, and that the rules in question do have a different status.

He says:

> ... imagine that chess is played in different countries according to different conventions .. [about boards and pieces] . Of these different countries, we could say that they play the same game of chess according to different conventional forms. Notice, also, that the rules must be realized in some form in order that the game be playable.[12]

On this account, the identity of chess is no doubt a matter of the rule-set that constitutes it; but the conventions determining the forms in which it may be realized are conditions not of its identity, but of its playability. Given the importance of identifications in Searle's account, we should surely take rules of conventional realization as distinct from constitutive rules.

It must appear, from Searle's point of view, that Ransdell's account of constitutive rules is in a sense upside down. Ransdell makes a constitutive rule a rule about the applicability of rules; yet it is rules of just this sort that Searle wishes to distinguish as conventional realizations of constitutive rules. There are two possible objections to this claim.

First, Ransdell clearly says that most constitutive rules will have g-connotations, and such cases would count as constitutive rules for Searle too, so the problem about n-connotations cannot be a defect in

Ransdell's general account. But in the same passage, Ransdell says that constitutive rules come in systems, through which we can trace relations of logical priority leading ultimately to n-connotations; so the rules which have logical priority in Ransdell's account are the very ones which Searle distinguished, as conventions, from underlying constitutive rules.

Second, it might be said that what could be shown in the case of chess could not be shown for the non-arbitrary n-connotations of, say, baseball. I have already questioned the non-arbitrariness; but there is a further point. Ransdell appears to take it as clear and unproblematic that the game-term 'Hit' refers to a proper sub-set of the referents of the natural term 'hit', a sub-set picked out by the rules of baseball. But we might take it otherwise. Suppose it were a conventional realization of rules about Swings and Hits that a Player rolled a die, certain results counting as Hit, Strike and so on; and other rules were interpreted accordingly. If this supposition makes sense, there is not the proposed sort of difference between chess and baseball; and it makes sense if we could call such a realization of rules Baseball. Since Ransdell, at least by implication, claims that the physical characteristics of Bats are logically internal to the game itself, he might be assumed to say that we

could not; the way is open, at least, for Searle to say that we could, through a more careful delimitation of our notion of constitutive rules.

It is some reason for leaning to Searle's side that Ransdell's incorporation of n-connotations in constitutive rules leaves him with problems of identity, both for rules and for systems of rules, which Searle offers some hope of avoiding. However, since Ransdell's problems arise at the points where he attempts to plug the very substantial gaps which Searle left in his account of constitutive rules, Ransdell's notions cannot be put aside.

The second, closely related issue, concerns Ransdell's account of the form of constitutive rules. I want to suggest that there are serious difficulties in the notion, central in his account, that the essential characteristic of constitutive rules is that they are second-order rules.

Game-terms are defined by constitutive rules, conditional in form, having as their consequent the import of the game-term they define, that is, those rules of permission and obligation which apply to the object to which the term applies. Hence constitutive rules become second-order: "... the fact that such a

formula mentions a first-order rule (or set of rules) in its apodosis is, of course, what makes it a second-order rule".[14] Presumably, a rule is first-order when it does not mention another rule. I do not think that this distinction would prove as readily drawable as Ransdell seems to suppose; but I wish here to suggest what I take to be a more important problem, which closely relates to the first issue, and arises when we accept the distinction in Ransdell's terms.

He takes his account of constitutive rules to suggest both that such rules are second-order, and that "... the game-term is in principle eliminable from the description of the game altogether".[15] The proposed replacement for a game-term is a constitutive rule, or perhaps a constitutive rule preceded by 'the thing/person/action such that ..'. Now, it follows from this that a first-order rule cannot contain a game-term or any expression synonymous with a game-term, since if it did, it would contain an expression replaceable with an expression which mentioned a rule, and would hence be, contrary to hypothesis, second-order. Thus first-order rules cannot be, so to speak, game-oriented: they must be perfectly intelligible, if somewhat pointless, to one with no acquaintance with the game in question. But this leads back to the problem noted above.

Suppose a set S of first-order rules of a game G. Since the members of S, being first-order, cannot show in themselves their game significance, we might ask: what is it that makes S the rules of G? The answer appears to be: the constitutive rules of G, which <u>both</u> establish the sense of the game-terms of G, including 'G' itself, <u>and</u> establish relations, basically definitional, between those game-terms and the natural terms in which members of S are expressed. But it is precisely this dual function of constitutive rules which, I have argued, leads to problems of identity, at least in the case of chess. By somewhat stretching some comments of Searle's, I have suggested that these problems might be eased if constitutive rules were restricted to the function of establishing the sense of game-terms in the game; and the relations between these and natural terms were established by rules, of a different sort, for the conventional realization of the constitutive rules. This distinction will in fact be a central feature of the account I attempt to develop in Part II; and though the line is not directly open to Ransdell, I shall nevertheless be able to base much of my early account on the structures, vocabulary and insight which he has added to Searle's somewhat sketchy proposals.

## PART II

Relativism can be based on a notion of a system of systems of systems of rules, counting as a form of life.

## CHAPTER 4 There is a significant distinction between constitutive and interpretive rules.

### SECTION 11 A comment on rules and conventions

One might begin the attempt to articulate the notions of a way of acting or a form of life by considering a number of common verbs which might be said to lead to the identification of ways of acting: terms like 'eat', 'fight', 'love', 'worship' and so on. Eating, fighting, loving and worshipping might reasonably be regarded as kinds of "natural" behaviour rather than as ways of acting in the intended sense; but for each, one can identify a conventionalized form - banquets, duels, *affaires* and religious services - which appear to be more likely candidates.

There is in this approach some conflict with an aspect of Searle's account of constitutive rules which I have previously mentioned and questioned. He seems to say of such cases that the conventionalized activities are always to be identified by the original verb, as activities prior to the rules which might be introduced to conventionalise eating as banqueting, fighting as duelling, and so on. The rules in question would therefore be regulative, not constitutive. Certainly he says this

about eating, and he might be expected to extend the account to other cases. It is not clear whether the initial verbs are to be taken to identify a level of behaviour that is natural or primitive, in some sense which precludes its being thought of as rule-constituted; or whether the point is just that, whether rule-constituted or not, eating is the basic form of activity involved, and the encrustation of ceremony characterising banquets is only a regulation of that basic activity.

Against this, I have argued that the distinction between constitutive and regulative rules is best regarded as context relative, rather than as the absolute distinction which Searle seems to suggest. One need not suppose that eating is the main point of a banquet. Those attending may neither need nor want the food; this, even if true, seems to leave much of the point of the activity untouched. One might attend a banquet to be seen to attend, to hear or make speeches, to savour food or wine without particularly wishing to consume much of it; and banquets might be planned to meet these purposes, rather than to see that everyone gets a square meal. In other words, if the point of the activity is the consumption of food - if we say that what is happening _is_ eating - then the rules for conducting a banquet don't constitute _that_; but if we _call_ the occasion a banquet, thereby indicating that the point of it is something other than

just eating, then it is not clear that Searle's claim holds.

Of course, it is not to be supposed that all rules relating to an activity would count as constitutive, whether on Searle's account or on any other. The suggestion to a beginner at chess that he should not move the same piece several times in the opening may increase his chance of winning, but his neglect of it does not cancel his claim to be playing chess, as would his neglect of a constitutive rule. Such a suggestion may well be taken as based on a regulative rule. Although it does not contribute to constituting the game, it is not beside the point of the game. It helps to establish notions of quality applicable to chess games, enabling us to say of someone not merely that he is playing chess, but also that he is playing it well or ill. It enables evaluation, which I have already suggested is one of the clearer aspects of Searle's distinction between constitutive and regulative rules

Rules of etiquette have no comparable use in the business of eating qua eating. The notion of eating well is not that of eating politely, or in due form. Rules about the order in which food items should be taken might be a basis of gustatory value, though this is relevant only if the point of eating is taken to be gustation rather than nutrition; but the concerns of Emily Post could fairly be called, from either point of view,

mere convention. Thus we can distinguish between two kinds of non-constitutive rule as follows: a regulative rule does not contribute to establishing the point of an activity, but it does provide a basis on which the activity can be evaluated as a way of achieving that point; a mere convention serves neither of these purposes for the activity to which it is applied.

However, it cannot be assumed that a mere convention is entirely arbitrary, irrelevant and insignificant: it may be so; but there are other possibilities. Regulative rules, because they are directed to the point of an established activity, do not point to anything beyond it, except through the tenuous links provided by the systematic ambiguity of terms like 'well' and 'ill'. Mere conventions may indicate points beyond that of the game to which they are applied. They may do this in two ways: either by indicating a feature of the context in which the game is set, or by showing how an alternative game might be constituted. For example, in educational situations, teachers are not permitted to use extreme forms of torture on their pupils. Depending on the account given of the point of the education game, the rule proscribing torture might be constitutive, establishing part of the point of the game - say, to develop a certain sort of relationship with the pupil; or regulative, reflecting a belief that the point of the game is not well served by such means; or merely

conventional in the sense suggested above, having no direct relation to the point of the education game itself, but arising from an important feature of the social context in which the education game is set. In the last case, the significance of the convention is mainly in establishing a contextual feature, but it is also possible to see that in a different education game, which had that part of the context as part of its point, the rule would be constitutive. Similarly, rules of etiquette are mere convention where the point of eating is taken to be nutrition, but might serve to constitute dining as an activity with a different point.

That a rule-set might be merely conventional in one activity and constitutive of another is an important suggestion, bearing on the way in which ways of acting might be interrelated in a way of life; but I shall not attempt to spell the matter out at the moment. Similarly, I shall defer the question whether any level of activity is to be regarded as absolutely primitive, rather than as primitive only relatively to some other level. For now, it is sufficient to note that the distinction between regulative rules and mere comventions permits us to preserve something like Searle's account of constitutive rules as the basis of an account of games, even in the kind of case which he thought was to be excluded.

The initial suggestion, then, to guide the search for ways of acting, is to look for conventionalized

forms of common activities, or, in a sort of formal mode, to look for terms that are related to common verbs as 'banquet' is related to 'eat', or 'duel' to 'fight'. One need not suppose that each common verb would deliver a way of acting. There may be activities which have not been conventionalized; and it would not be surprising if eating and drinking turned out to have conventionalized forms in common, or if an activity were conventionalized in more than one way, as worship might take many forms. These possibilities also bear on the possible interrelatedness of ways of acting. However, this approach allows at least a first approximation: a way of getting some sort of body into the notion of a way of acting, so that one might be able to move on to questions about the development of notions of identity and completeness for ways of acting. The conventionalized forms are to be regarded as constituted by rules which establish the sense of terms like 'banquet' and 'duel'.

The above discussion of constitutive and regulative rules and mere conventions, particularly the educational example which suggests that what is in some sense the same rule might be any one of these, depending upon features of the context in which it occurs, shows that we cannot rely upon the form of a sentence expressing a rule to show what kind of rule it is: to use sentence form as a criterion for sorting out rules in use would be to risk going very much astray. However, the purpose of

clarifying the notion of a rule-constituted game can be served by attempting to establish what would be a perspicuous standard form for a constitutive rule; and to that purpose I now turn.

SECTION 12 Constitutive rules should be distinguished from rules of interpretation.

In discussing the form of a constitutive rule, I shall make use of some of the terminology introduced by Ransdell in the paper previously discussed. I shall employ his distinction between the connotation and the import of game-terms, i.e., between "... the characteristics a thing, person or action has that justify the application of a given game-term to it, and the logical effect of that application"[1], the logical effect being the rules to which the thing becomes subject by the application of the term. I shall use 'C' and 'I' to stand for expressions of connotation and import respectively. I shall also employ Ransdell's distinction between game-terms, which "... have special meanings within the game and are in fact specially defined for the game in the rule-book"[2], and non-game, or natural terms, the meaning of which is prior to, or independent of, the game in question. I shall use 'G' to stand for game-terms, and subscript 'g' or 'n' where necessary to indicate the terms of which expressions of connotation or import are comprised.

In these terms, and on Ransdell's account, Searle's preferred version of the form of a constitutive rule is

(1) C counts as G.

One of Searle's examples, when put in this form, is "The king's being attacked in such a way that no move will

leave it unattacked counts as checkmate". He says that such rules are almost tautological in character, and the fact that they can be construed as rules or as tautologies is a clue to their constitutive status[3].

Ransdell's own preferred version is

(2) C counts as I.

He offers as example '"Allowing four Balls to pass while one is Batter counts as being henceforth subject to the following rules:__" the list of rules which are then specified being, of course, those which apply to a Runner on First Base'[4]. This form, he says, is functionally equivalent to the conditional form[5], so we might write

(2') If C then I.

Game-terms are defined by the whole of expressions in the form (2'): thus, 'He was walked' is defined by a conditionally expressed version of the example above[6]. His earlier discussion of the way "game-terms are predicated of particular things" suggests that we are to see a system of such rules as one in which game-terms introduced in this way are used in the C and I places of other rules, and as having at its foundation level rules in which natural terms occur in the C place[7]. Thus we might distinguish

(3) If $C_g$ then I

from

(4) If $C_n$ then I

not as different forms of rule, but as displaying internal differences between rules related to their place in a system.

The main trouble with this account is that (3) and (4) exhibit significantly different logical behaviour: the suggestion that connotation is a sufficient condition of import is true in the case of (3), and false in the case of (4). If C is a sufficient condition of I, then I is a necessary condition of C. This is clear enough in (3). If the player is not subject to the rules which apply to a runner on first base, then we can infer (*inter alia*) that he has not just allowed four balls to pass as a batter, i.e., that he has not been walked. The situation in which this inference would be mistaken - in which the player satisfied the connotation but was not subject to the import - is not possible: the point of the rule is not to deny that this situation will occur, but to make such a description meaningless. This, no doubt, is the point that Searle made by saying that constitutive rules had an appearance of analyticity, or were quasi-tautologous. In the case of (4), however, the inference does not work. Something may have the natural characteristics of a baseball bat or achess piece, yet not be subject to any rules of those games, being in fact a club or an ornament.

Consider this example. I observe a man seated at a table with a pack of cards. He takes thirteen cards from the pack one by one, placing them face down in a pile, then turns the pile over. He then places one card face up above and to the right of the pile, and places four

more cards face up in a row to the right of the pile. I decide that he is dealing a Canfield layout. It is part of this decision that I identify the cards I have mentioned as stock, first foundation and tableau respectively, and assume the applicability to them of certain rules. For instance, in identifying one card as first foundation, I note that it is dealt in a certain relation to the stock, and that it may be built upon in certain ways, but not used for building on the tableau. At this point, the man takes the card I have identified as first foundation, and places it on one of the cards I have identified as the tableau. I point out to him that this is wrong. I then discover that, although acquainted with some forms of patience, he is not acquainted with Canfield, and was not dealing a game of any sort, but merely laying cards down at random as an accompaniment to thought.

In these circumstances, I must withdraw the claim that the card is subject to the proposed rules, and on the basis of a rule of form (3) defining 'first foundation', must consequently withdraw the claim that it was dealt in a certain relation to a stock, thus, in effect, withdrawing the claim that it was a first foundation. There seems to be no difficulty about this, and the status of the rule in form (3) is not affected by my discovering that it does not apply in this case. If, however, the rule is to be in form (4), having in its

C-place the sort of description which I gave in the third and fourth sentence of the last paragraph, the case is different. The rules governing first foundation do not apply, but I cannot in this case deal with the matter by denying the correctness of the descriptions in the C-place, which continue correct regardless of what the man intended. Hence I must regard this case as falsifying the rule; and this is not the way constitutive rules are supposed to work, either on Searle's account or on Ransdell's.

We might retain the conditional form by reversing it; i.e., by replacing (4) by

(4') Only if $C_n$ then I.

This would allow us to infer from the correctness of an attribution of import to the correctness of certain natural descriptions in cases where such a rule applied, and its application would be entailed by the correct attribution of import; but not from the correctness of any natural description to the correctness of an attribution of import - that is, to the applicability of a rule. This is intuitively plausible, and it saves the rule in the sort of case imagined above; but it has the problems I mentioned in the earlier discussion of Ransdell. If the natural descriptions are specific and precise, they will have the effect of forcing us to say that informal or improvised games of baseball are not really baseball at all. This would conflict with the way we normally identify such activities. If, to avoid this, we make the natural descriptions more general and indeterminate, they

will have about the force of 'such properties as allow the rules of the game to be satisfied', which seems to make the natural characteristics to which game significance is attributed less important that we take them to be.

Before accepting such consequences, one might consider abandoning the conditional interpretation of the '.. counts as ..' formula where natural terms are involved, and considering some alternative way of understanding it. Two relations can be suggested as more or less analogous to the relation which the formula expresses.

One possible analogy is with the relation between the language of a scientific theory and observation language, as these are distinguished by Scheffler in the work previously discussed. Here we have an analogue of game-terms in the technical terms of a theory, the meanings of which depend primarily on their place in the theory, as in the case of such terms as 'atom', 'nucleus', 'electron', etc.; and a very close analogue of natural terms in the terms of an observation language, regarded as independent in the sense that their applicability is not dependent on the soundness of the scientific theory in question, and the truth of a statement in which they appear is not dependent on the truth of any putative law contained in the theory.

It is perhaps tempting to think that the relation between theoretical and observational language

is best seen as arising from the process of experiment. At the cost of some over-simplification, it might be represented as a conditional relation in the form of (4') above, in which the truth of a statement in theoretical terms is presented as sufficient condition of the truth of a statement in observational terms. The difficulties which I suggested attach to this form disappear in such a case: the observational description should clearly be as specific and precise as possible, and the possibility that we might be forced to distinguish sharply between situation which we had previously treated as similar is not a problem. But this apparent support for the conditional form is a result of over-simplification. The conditional framework of an experiment requires in its antecedent not only an hypothesis in theoretical terms, but also a statement of initial conditions in observational terms. Theoretical and observational terms are conjoined rather than conditionally related in the antecedent; and the sort of relation which (4) and (4') attempt to express is presupposed rather than exhibited in such a context.

An alternative context is provided by the notion of scientific explanation, as when a lightning flash is explained as an electrical discharge. The crucial point of such an explanation is commonly an identity claim: the lightning flash is explained when we understand that it is the same thing as an electrical

discharge - provided, of course, that we understand the theory in which 'electrical discharge' has a place. At first sight this is even less promising. There is a well-worn path from such identity claims, through Leibniz's law, to claims of intersubstitutivity for the expressions concerned. I have already suggested that there are difficulties for weaker requirements than this; and we could certainly not recast (4) or (4') as identity statements, or as statements that the expression in the C place was interchangeable with the expression in the I place, in view of the fact that the C place may contain a list of physical characteristics of an object, and the I place is required to contain a rule.

Yet the example does not seem entirely beside the point. Sentences like "The piece of wood with such-and-such properties is the baseball bat" are in use, are related to the kind of rule I am considering, and can be regarded as identity statements. They even support a limited intersubstitutivity: I can say, to a beginner who I know is slow to acquire new terms, "Hold the piece of wood by the thin end", and achieve thereby just what I would achieve by saying "Hold the bat by the handle". The qualification "limited" is important; every lightning flash is an electrical discharge, but not every piece of wood of the same sort need be a baseball bat. But perhaps this difference is not critical. Broadly speaking, scientific theory takes the whole

range of physical phenomena as its domain, hence no observational description of a phenomenon will be outside its scope; and, to the extent that the theory is sound, there should be no problems, at least with substituting theoretical for observational terms, as long as the observational language is sufficiently rich to cope with the distinctions which are there to be made. Baseball rules, on the other hand, have as their domain only a limited range of human activity - that which, on the account under consideration, they constitute. Thus there is no reason to suppose that 'bat' should be substitutable for a natural description occurring outside the domain of the rules, even if it would be so within their scope. That is to say, the difference may indicate a difference in the scope of the relata, rather than a difference in the form of the relation.

A slightly different account of the relation, which helps to make this point, can be drawn from another example. In a discussion of central state materialism, Deutscher has suggested that a typical expression of this position should be regarded, not as an identity statement, but as an identification statement, in which something incompletely known and inadequately identified as a mind state is, on the basis of more complete knowledge, more adequately identified as a brain state.[8] This line clearly takes the physicalist language of the scientific theories in terms of which central state materialism is

stated to be superior to the ordinary or technical language of the dualist theories which allow the identification of mind states. It is for this reason that, although the scientific basis of central state materialism has the sort of all-inclusive scope mentioned above, and there is clearly an identity statement buried in Deutscher's version of the thesis, one does not expect the two identifying descriptions to be intersubstitutable. The physical state descriptions which might be substituted for mental state descriptions are generally not available, and if they were, would not serve purposes such as moral assessment, which are an important part of the activities in which the dualist language is used. Mental state descriptions which might be substituted for physical state descriptions are generally available, but are not expressed in terms which enable them to be linked to other expressions in the explanation. To say, for instance, that given a certain state of mental awareness, certain visual stimuli will cause a state of recognition, which will in turn cause movement within a certain range, is not to provide an explanation, in any serious sense testable, of that movement. Confronted by such a statement, one would probably assume that 'state of mental awareness' and 'state of recognition' were placeholders for fairly complex descriptions of brain states and changes in brain state, or perhaps for series of EEG readings accepted as signs of such states. But not until these appropriate descriptions are available is the state-

statement testable; and until it is testable, it is not so much a scientific statement in which some substitution of expressions has been made, as an unfortunate hybrid of doubtful sense and value.

I take this discussion to support the comment above on the significance of the scope of a theory: to show that failure of intersubstitutivity can be the consequence of attempts to apply a theory outside its scope, and that central state materialism is such an attempt. This, of course, is just what the central state materialist denies; but in this denial he incurs one, or possibly two, obligations. The first is to provide an adequate alternative to mental state descriptions. Since physicalist and dualist theories are significantly different, there must be problems in deciding when one provides an alternative, in the relevant sense, to the other; hence this notion of an alternative is not a clear one. However, one can offer examples. Science offers an alternative to magic, at least insofar as both are regarded as ways of understanding the world, and pursuing one's interests in it. Science is not, perhaps, a complete alternative to religion, but it might be argued that it offers alternatives in so many areas to which religion has laid claim, that, if it is accepted, the scope of religion is greatly diminished. The second obligation is to show that the alternative has a superior claim to acceptance. Until the first

obligation is met, over-extension of a theory will yield unfortunate hybrids like the example above. To meet the first obligation without meeting the second is to produce two systems of equally acceptable and non-intersubstitutable terms.

All these notions, of explanation, identification, scope and preference, bear on the task of improving (4) above. The basic analogy consists in this: that the objects and activities involved in games such as baseball can be identified and discussed in natural terms; but cannot be explained in these terms. They are explained by reference to the rules constituting baseball; and it is essential to this explanation that things initially identified in natural terms be reidentified in game-terms. It is the form of statements effecting this re-identification that (4) attempts to capture. The analogy suggests that these identification statements have more in common with statements of identity than with conditionals. It is because of this that they support limited intersubstitutivity, the limitation being the scope of the rules constituting the game. The identification in game-terms is the preferred or superior identification. The implications of game identification supersede those of natural identification where these conflict, as when two pieces in a board game are allowed, in game-terms, to occupy the same place at the same time. It should be noted that the basis of this preference is

not best explained as being the superior comprehensiveness or accuracy of the game-terms, though there is a sense in which they must be said to have this character. Rather, the undertaking to play the game _is_ the acknowledgement of this superiority: part of what it is for us to be playing the game is for us to be engaging in an activity which is not adequately explained without the use of game-terms.

As well as casting doubts on the conditional form, this analogy suggests that the form (1) above, which Ransdell attributes to Searle, may be more adequate than Ransdell suggests. When we say "That flash of light in the sky was an electrical discharge", our purpose is to identify the phenomenon in terms of the theory in which 'electrical discharge' has a place. It is not necessary for this purpose that we should, in the same statement, spell out the theoretical implications of the identification - that is what the theory is for, Similarly, when we say "Such-and-such a thing counts as a baseball bat", we identify the thing in a way that brings it under the rules; and we can do this without making the rules an explicit part of the identification statement. I am, in effect, distinguishing between those rules which constitute the formal structure of the game, and those rules through which that formal structure is applied. For rules of the second sort, a small revision of the formula attributed to Searle seems

sufficient, viz.:

(1') $C_n$ counts as G.

This approach is already familiar in the area from which I draw the analogy. Nagel, in his discussion of scientific explanation, having distinguished between theories and experimental laws in a way loosely parallel to the distinction between game terms and natural terms which I have employed, proceeds to characterise the components of theories thus.

> For the purpose of analysis, it will be useful to distinguish three components in a theory: (1) an abstract calculus that is the logical skeleton of the explanatory system, and that "implicitly defines" the basic notions of the system; (2) a set of rules that in effect assign an empirical content to the abstract calculus by relating it to the concrete materials of observation and experiment; and (3) an interpretation or model for the abstract calculus, which supplies some flesh for the skeletal structure in terms of more or less familiar conceptual or visualizable materials.[9]

I have not suggested any general need for a model, though I shall later suggest that the notion of a model is useful in certain sorts of case;[10] I have taken it that when the formal system Nagel mentions in (1) is dealt with by the set of rules he mentions in (2), the result *is* an interpretation of the formal system. However, the remaining apparatus of calculus and content-assigning rules - often called bridge principles - is clearly similar in form and purpose to the apparatus of formal game structure and rule of application proposed above. Thus my approach reflects what Hempel called the standard

conception of scientific theories.[11]

The comparison suggests certain criticisms. Hempel, in the paper mentioned, says of the distinction between internal principles and bridge principles that

> ... it should be explicitly acknowledged now that no precise criterion has been provided for distinguishing internal principles from bridge principles. In particular, the dividing line cannot be characterized syntactically, by reference to the constituent terms; for, as has been noted, both internal principles and bridge principles contain theoretical as well as antecedently available terms. Nor is the difference one of epistemic status, such as truth by convention versus empirical truth.[12] The distinction is, thus, admittedly vague.

Feyerabend, discussing a similar account of scientific theory in terms of an uninterpreted postulate system and correspondence rules quoted from Carnap, and regarded as the basis of objection to claims of incommensurability, takes its point to be ".. that new and abstract languages cannot be introduced in a direct way, but must be first connected with an already existing, and presumably stable, observational idiom"[13]; and goes on to say that this "..is refuted at once by noting the way in which children learn to speak and in which anthropologists and linguists learn the unknown language of a newly discovered tribe"[14].

None of these problems seem serious at this stage. Whatever may have been the case for Hempel's examples, it is quite clear that application or bridging rules in the form (1') identified above _must_ contain both

natural and game terms; and that formal rules in the form (2') identified above must not. At least, this second point would be clear if some clear position were established on logical or syncategorematic expressions, and if it were remembered that expressions which were capitalised to show game status in Ransdell's presentation, such as 'Walked', are not synonymous with terms left uncapitalized to show natural status, such as 'walked'. While it is not clear that rules in the form (1') are to be distinguished from those in form (2') as empirical truth from conventional truth, it is to be remembered that these forms were distinguished precisely because of a difference, if not in their truth conditions, at least in their withdrawal conditions. It should also be recalled that the apparent indeterminacy between empirical and conventional truth, arising in both Searle and Scheffler, provided part of the basis for the present approach.

Concerning Feyerabend's comments, it is not clear that the sorts of case he mentions provide a refutation of the claim that this distinction has the place proposed, even if we accept his account of its main point. Anyone who accepts that new and abstract languages or theories or games must be interpreted in a preexisting idiom through correspondence rules thereby confronts a problem of foundation: either the very first structure acquired must have been of such a different type as to require an entirely different analysis, or some abstract language must have been

learned without being in some degree translated into an antecedent idiom. Either alternative would damage the analysis. One might suppose that it is to this problem of foundation that Feyerabend's comments are directed, but this is not so. His concern with children is not with their first conceptual structures, but with later ones. He suggests that a later "concept and perceptual image of material objects" replaces a stage at which "objects seem to behave very much like afterimages", without this earlier view either explaining or providing evidence for the new notion of a material object. Indeed, the previous objects of perception, "... or things somewhat like them, still exist, but they are now difficult to find and must be discovered by special methods. (The earlier visual world therefore literally disappears.)"[15]. Similarly, the anthropologists and linguists learning the unknown language of a newly discovered tribe have an antecedent structure, but scorn to use it. They ".. remind us that a perfect translation is never possible, even if one is prepared to use complex contextual definitions. This is one of the reasons for the importance of field work where new languages are learned from scratch, and for the rejection, as inadequate, of any account that relies on 'complete' or 'partial' translation"[16].

I think that there is a problem of foundation for the sort of view I am attempting to formulate, and I

attempt to deal with it in a later chapter; but Feyerabend does not invoke it. This is because his suggestion that the child's new concept owes no debt to the old one, and the anthropologist's new language is similarly detached, is implausible on the face of it, and ill-supported by argument. The context of Piagetian theory supports the identification of developmental stages, but not the claim that an earlier stage has no significant relation to a later one. The afterimageish, indeed, sense-data-like objects of the earlier stage do not explain the notion of a material object, but this is not to be expected. The notion of a material object, however, might be used to explain them; and it can do so, through the very theories of Piaget and his school which are cited by Feyerabend. It would be odd to suggest that these earlier private objects were evidence for the notion of a material object; but given such a notion, we can explain what it might mean for a private object - a perceptual image of a material object - to be evidence of a commonly reliable kind for statements about particular material objects. Of course, one cannot see the world in two incompatible guises at once, and one might use this example, as I earlier used examples relating to motor cars, to suggest the difficulty of supposing that a conceptual change changes nothing but concepts; but this just emphasises the oddity of Feyerabend's using a case of alleged conceptual change as though it were a refutation of a general account of conceptual structures.

There is a similar lack of appropriateness about the other example. Even if we disregard more or less Quinean objections to the apparent view that only a perfect translation is better than no translation at all, and even if we ignore the question how the anthropologist's first language is to be kept out of the business of learning the new language from scratch, both of which questions affect the quality of Feyerabend's argument, there is still the point that noone has ever wished to say, of natural languages, that one could be learned only on the basis of prior knowledge of another. One cannot refute an account of the relationship of conceptual structures within a language by showing that this is not the way that different natural languages are related to each other.

In justice to Feyerabend, it should be said that in the passages quoted he is particularly concerned to meet an argument against incommensurability. He allows a distinction between conceptual apparatus and its basis, insisting only, in the Piagetian case, that the basis changes from stage to stage. But it is part of this view that represents the basis as in some sense "observational", and thus constant; and it appears to be his view that only by relativising observation to the conceptual structure which explains it can certain unfortunate consequences be avoided. However, it is not at all clear that the antecedent language must be

"a presumably stable observational idiom".. The language antecedent to a new conceptual structure need not be observation. Indeed, it need not be stable, though no doubt things are easier, and the new structure more useful, if it is. This point is made in the field in which Feyerabend wrote. Commenting on a concept of theoretical terms proposed by J.D. Sneed, Stegmuller says: "According to Sneed and unlike the common view, the difference between the theoretical and the non-theoretical does not coincide with the observational - non-observational dichotomy".[17] I shall not, however, attempt to pursue issues within the philosophy of science. I have taken it that the most obviously similar proposals and difficulties arising in the field should be noted; but I do not think that my more general concerns are best worked out in the terms of a restricted field of enquiry. These accounts of scientific theory and explanation provide analogy, not solutions.

The distinction between rule forms is perhaps more clearly seen in the second case which I wish to offer as analogous: the interpretation of an uninterpreted formal system. P.F. Strawson, using this as a way of looking at the activity of a formal logician, suggests that the logician's task may be seen as having two parts. The first part, he says, is the construction of a purely abstract system of symbols, involving the introduction of symbols, and of various kinds of rule

for their manipulation. He goes on:

> Such a system is called 'abstract' because no meaning is attached to the symbols over and above the rules of combination and transformation. The second part of the task will then be that of giving the symbols a meaning or interpretation; and for this it will be necessary to use expressions of which the meaning is regarded as already known.[18]

Here we have the distinction between natural terms and terms with a meaning established in a system of rules clearly drawn, together with the notion of establishing a relation between them. In interpreting the system, we establish its scope by determining the domain over which its variables range, and a limited intersubstitutivity in regarding elements of that domain as values of the variables of the system. In interpreting a system as sentential calculus, we identify the members of certain classes of sentence as formulæ subject to the rules of the system, in the process disregarding or restricting or expanding functions that those sentences may have had in informal discourse. In all these respects, this process is closely analogous to that of introducing a game, and to the process of scientific explanation discussed above. Of course, scientific explanation is not quite like the interpretation of a formal system; but one of the more obvious differences might reasonably be said to mask a significant point.

In the case of scientific explanation, we are commonly inclined to think of the observed phenomena coming first, and the theoretical explanation coming

second; in the case of formal logic, as Strawson's presentation suggests, we are inclined to think of the abstract system coming first, and its interpretation coming second. Both inclinations are open to criticism. Strawson thinks that we would be misled in so seeing the logic case, since "... in practice, the logician always approaches the task of system-construction with some particular kind of interpretation in mind"[19]; and the broadly speaking inductivist account of scientific explanation above has been vigourously attacked by Popper. If Strawson and Popper are correct, one might argue that the distinction between explanation and interpretation rested not on a difference between two processes, but on a difference between the interests and intentions of logicians and scientists. This is not entirely beside the point; but there is a further point which does not depend upon this kind of argument. It is no doubt true that, as a matter of fact, people encounter physical phenomena before they encounter scientific theories; and they could invent, or be shown, purely abstract systems before they found ways of interpreting them; and that the search for explanations and interpretations in such cases would be different, at least psychologically, and probably methodologically. But I do not wish to use the distinction between the formation and the interpretation of systems in the investigation of historical or psychological or methodological questions. I wish to use it as an instrument of analysis:

as a way of more clearly articulating a structure of rules which might be seen as constituting games, or ways of acting, or forms of life. I will regard it as suitable for this purpose if, understood in terms of the analogies I have discussed, the analogies invoked are sufficiently strong to provide a coherent way of talking about constitutive rule systems, and there is reasonable hope of further illumination.

I think that this discussion has shown a sufficiently strong analogy between explanation and interpretation on the one hand, and rules in the form (1') above on the other, to satisfy the first requirement. Concerning the second requirement, I will quote further from Strawson. After pointing out the respect in which his division of the logician's task may be misleading, he goes on to say:

> The reasons why it is illuminating are at least two: (i) it suggests, what we shall see to be the case, that a system constructed with one interpretation in mind may turn out to be susceptible of more than one interpretation (i.e., we may be put on the track of further formal analogies); (ii) it reminds us that we cannot be quite sure how the symbols of a system are to be interpreted until we have supplemented the explanations given with a study of the rules of the system. [20]

The significance of the possibility of alternative interpretation I trust will emerge in subsequent discussion; I shall not attempt to develop it at this point, though it might be noted that being "put on the track of further formal analogies" is highly desirable. The second

suggestion has more immediate application. The error of supposing that we understand the symbols of a system on the basis of their interpretation, without a full understanding of their place as defined by the rules of the system, is in effect the error which, according to Ransdell, threatens Searle. Noting that Searle says that gameterms are not merely labels, but mark something that has consequences, he says:

> But Searle does not follow up this important point to develop its full significance. Moreover, the intra-game import of the particular term he cites is not brought out.... In view of the fact that none of this is brought out in a general way, there is reason to doubt that Searle fully recognized the significance of the "consequences" he mentions.[21]

Ransdell's way of giving weight to the consequences leads to his proposal of the form (2'), in which the consequences form the consequent of a conditional which, as a whole, defines the gameterm which Searle appeared to define upon the antecedent alone. Now, Ransdell is at this point explicitly concerned with cases in which the connotation is expressed in gameterms; and for these cases I have found no reason to quarrel with his proposed refinement. But his reasons would apply, with even more force, to cases in which the connotation is expressed in natural terms. He takes it that the same refinement works in these cases. I have argued that it does not; and the distinction between a formal system and its interpretation provides a way of saying why it does not, and offers an alternative account of these cases, while reinforcing what was correct in his diagnosis. I take

this to be sufficient for the second requirement.

I propose, therefore, that we distinguish two kinds of rule in systems which might constitute games or ways of acting or forms of life. The first, which I shall call formal rules, can be regarded as having, ideally, the form (3) above,[22] and are constitutive of what I shall call formal games: abstract, uninterpreted systems. The second, which I shall call rules of interpretation, can be regarded as having, ideally, the form (1') above,[23] and relate formal games to independent domains of language. The definitions of gameterms, on this account, must also have two components. A gameterm may be said to be formally defined, in the manner suggested by Ransdell, by a formal rule: i.e., to predicate a gameterm of some subject is equivalent to asserting of that subject that it falls under a particular rule. It should be noted that this is a very schematic account. The relation between gameterms and formal rules need not be one-to-one. This, to define 'king' in chess would involve rules about initial placement, movement and capture, and also the rules relevant to 'check' and 'checkmate'. There is point in saying, as Ransdell quotes Strawson saying, that the basis of meaning is the whole structure of the game; there is, however, also point in a more limited notion of a definition. To define a term formally is to place it in a formal game. Its interpreted definition - its

definition in an interpreted game - will involve a link to natural terms. This link may be indirect. There is no reason to suppose that every gameterm must directly relate to some natural expression. We could not satisfy such a requirement for 'electron', nor is it satisfied for 'materially implies'. However, there are direct links in some cases. Thus, 'cricket bat' might be formally defined - loosely, and with no attempt to impose conditional form - as that which the batsman uses to strike the ball. Under the normal interpretation, the familiar sort of wooden artefact will count as a cricket bat, hitting a particular sort of spherical object with it will count as striking the ball, and various consequences will follow depending upon the outcome of this operation. In an alternative interpretation, a die might count as a cricket bat, rolling it might count as striking the ball, and various consequences might follow depending upon the outcome of this operation. As my use of 'normal interpretation' suggests, a purely formal definition, or one based on an unusual interpretation, would not normally be accepted as defining 'cricket bat'. Typically, the accepted definition of a gameterm will be an interpreted definition, and in the clearest cases will involve a normal interpretation, though there may be less clear cases in which a formal game is variously interpreted, or where more than one formal game is interpreted in the same domain.

SECTION 13 This account of rules meets requirements of generalizability and relevance.

At this point in the development of this account of rule-constituted structures, I should subject it to two tests: one of generalizability, and one of relevance. The account has used, as a basis, games in the ordinary sense of the word. In these cases, the distinctness of the game from our ordinary, non-frivolous concerns is generally clear enough; in contrast to our serious activities, the goals of games would not arise in the normal course of events, and would not interest us if they did, and the terminology and activities of games would not make sense without the rule-book. When we move to the sort of activity which, however conventionalized, is not thought of as a game, the importance of the rules appears to diminish. We feel we could understand the point of what is going on, even if we didn't know the rules; we might say of these cases, as Searle said of rules of etiquette, that the rules can only be regulative. I have already said something about these cases; but more is needed. If this feeling cannot be shown to involve a mistake, then we have an account of formal and interpreted games which is applicable only to games in the ordinary sense, and this would not serve the purpose I intend. The attempt to show such a mistake is the attempt to meet the test of generalizability. The test for relevance is the test for relevance to the purpose as I previously and provisionally stated it. I introduced the notion of a way of

acting, which I characterized as involving interactive bodies of belief and convention, which provided frameworks for individuation, identification and reidentification. I claimed, in opposition to Scheffler, that disturbances in the conceptual structures embedded in these bodies of belief and convention affected our ability to refer to individuals; and offered these claims as contributory to an account of a relativist position. It is necessary that the present account should permit the formulation and defence of similar claims, for if it does not, the relevance of this to earlier sections of my discussion is lost. I shall first consider the question of generalizability.

That an account of this sort is not restricted to games in the ordinary sense appears to be accepted by Ransdell, in the paper I have discussed. He accepts it, in his version, for the non-frivolous speech act of promising; he discusses its application to the case of law, and in passing suggests that it might apply to professional roles generally.[1] Of course, his main argument, that the account does not provide an analysis of speech acts generally, tends to the opposite effect; but this argument, even if it is sound in its own terms, is irrelevant to my present concern because of a difference between the notion of a rule-constituted way of acting and of a speech act which is implicit in Ransdell's own discussion. These notions are very different in

scope. The operation of law must clearly involve a variety of speech act types. There is testifying, which not only includes, in the case of testimony under oath, a kind of promising in the taking of the oath, but might also be said to include, in the case of expert testimony, cases of advising, another speech act type which Ransdell discusses; there is also rendering a verdict, and passing judgment. Thus the analysis of a speech act type may involve not just the association of acts of that type with a particular rule constituting certain behaviour as an act of that type, but rather giving such acts a place in a way of acting, which in its turn is analysed by reference to a constitutive rule set. The particular speech act type gets its meaning, one might say, from the whole structure of the game; hence Ransdell's conclusion, far from counting against the generalizability of this account of ways of acting, is what the account would lead one to expect.

There remains, however, the general question whether the sort of case which I initially suggested as providing examples of ways of acting can be regarded as sufficiently distinct to yield to the kind of analysis which can be applied to games.

Games, in the ordinary sense, are generally convenient examples of interpreted games in my sense, presenting few problems of individuation or identity.

More problems are to be expected when the notion of formal and interpreted games is used to organise talk about less arbitrary and well-disciplined spheres of activity.

At the beginning of this chapter, I suggested that some ways of acting might be identified through nouns referring to conventionalized forms of activities named by common verbs: 'banquet' deriving from 'eat', 'duel' from 'fight', and so on. It can now be suggested that the verbs are to be taken as natural terms relative to the games identified by the nouns: 'eat' is to be regarded as a terms of established reference through which a formal game is interpreted as dining; 'eat' stands to 'dine' as 'fight' stands to 'duel', or as 'compete' stands to 'play chess' or 'win' to 'checkmate'.

To bring out a further point, one might say: 'sword' stands to 'duel' as 'bat' stands to 'cricket'. One might suppose either that nothing of the sort that duellists call a sword or cricketers a bat had existed until they were designed to complete the interpretation of a formal game (as one might suppose that nothing of the shape of a conventional chess bishop existed until it was designed to complete a set of pieces); or that things of that general sort, and known as swords and bats, had existed before any formal game was interpreted as duelling or cricketing. On the former supposition,

'sword' and 'bat' are gameterms, which will be defined partly by their function in the game, i.e., in relation to other gameterms; and partly by their interpretation, i.e., in terms natural relative to the game. On the latter supposition, 'sword' and 'bat' will be natural terms with established reference, given a technical sense in the game to the extent that either the function or constitution of their referents is restricted by the rules. Thus a cricket bat may be recognisably similar to a rounders bat, but distinguished from it by being constituted in a specified way related to its function in the game; a duelling sword may be recognisably similar to other swords, but distinguished by the requirement, related to its function in the game, that it be the same in specified respects as another sword - a sword can be a sword in isolation, but a duelling sword only as one of a pair.

This resurrects earlier difficulties with Searle's distinction between constitutive and regulative rules. In the preceding paragraph, I treated the rules of cricket and duelling as constitutive, rather than as regulative. The rules of formal games cannot be regarded as regulative, since they do not refer to anything external to the game; and I am considering the interpretation of formal games as chess and cricket as being basically the same sort of operation as the interpretation of formal games as dining and duelling -

roughly, as creating the possibility of new forms of activity. For Searle, however, chess was a paradigm case of a rule-constituted activity, and cricket would no doubt fall in the same class; but these were to be distinguished from what he regarded as the regulative rules of etiquette at table, and no doubt also from a code duello. The first question about this difference of opinion is not whether the rule-sets in question are constitutive or regulative, but whether there is a significant difference between chess and cricket on the one hand, and dining and duelling on the other.

I have previously discussed the accounts given by Searle of the difference; what is to be stressed at the moment is its contingency. The point can be made in relation to the above distinction between a newly introduced gameterm, and a natural term given technical sense as a gameterm. In order to make the difference as sharp as possible, let us take as contrasting examples the use of 'pawn' in chess and 'sword' in duelling. We will suppose that 'pawn' is defined on the rules of a formal game and the physical characteristics of a conventional object; that the term has no use antecedent to or independent of the playing of chess; and that the behaviour of chess players is significantly different from any antecedent or independent forms of behaviour. We will suppose that 'sword' has established reference to a class of weapons; that 'duelling sword'

is introduced to identify a sub-set of these weapons on the basis of the restrictions mentioned above; and that the behaviour of duellists is significantly similar to that of people fighting with swords prior to or independent of the code duello. Searle might then be supposed to say, of chess and duelling, (i) that the former is, as the latter is not, a new form of behaviour; (ii) that the rules of the latter do, as those of the former do not, characteristically take imperative form; (iii) that the latter can, as the former cannot, be described in the same terms whether the rules exist or not; and (iv) that the rules of the former specify the behaviour, while those of the latter evaluate it.

Now, the contingency of the distinction consists in this. The suppositions above are reasonable, and, perhaps, more or less correct; but they could be false. One could suppose that the physical model of chess had antecedent and independent use in informal games similar to chess; and that weapons, and indeed personal combat, were unknown beyond the limits of a code duello. On these contingently false suppositions, it would appear that the code duello was constitutive, while the rules of chess were regulative. Thus the question whether a rule-set is constitutive or regulative is an historical question; it is a question about what happened first - about the ontogenesis of the activity - and it cannot be answered in the absence of

the required historical data.

This consequence brings out internal stresses in the criteria for the distinction. It is plausible to suppose that we cannot decide whether a form of behaviour is new without historical knowledge; it is much less plausible - indeed, it seems simply wrong - to suppose that we cannot tell whether we are specifying or evaluating without that knowledge. The obvious way to eliminate the stress is to eliminate the history: to treat the first basis of distinction above as concerned not with new, but with distinct forms of behaviour. We thus make it clear that we are concerned with the analysis, not the ontogenesis, of forms of behaviour. We also make it clear that our first concern must be with the individuation of forms of behaviour, and with the question what makes similarities and differences significant. The similarities between a squabble between armed ruffians and a duel between gentlemen might seem more significant to Searle than the differences. (And also, perhaps, to the ruffians.) To the gentlemen, the differences might seem more significant. This suggests that the gentlemen would take duelling more seriously, as a distinct form of behaviour, than either Searle or the ruffians. This might be taken to mean either that the gentlemen can individuate where the others cannot, or that the gentlemen take seriously regulative rules of good form that the others do not. And what

sort of question is it, which of these interpretations is correct?

It is tempting to see it as a question about a matter of fact - as the question whether or not duelling really is a distinct form of activity from fighting; but this is a circular path, for it is so if it is significantly different, and whether it is significantly different is just the question, at issue between Searle and the gentlemen, from which we started. The temptation, I think, is really that of a return to ontogenetic questions, for the matter of fact that might decide the issue is the historical one. But questions of the ontogenesis of games have been ruled out, so this is not a way to a solution. Rather, it is necessary to consider what account of a form of behaviour is accepted, and what this account requires to be said about the case. That is, expressions like 'form of behaviour', 'distinct' and so on are to be regarded not as terms the use of which is established through their reference to bits of human activity taken as given, but as part of a metalanguage - as terms established in a game interpreted as a theory of games.

On the account given here, the identity of interpreted games involves both the identity of the formal games of which they are the interpretation, and the identity of the referents in terms of which the formal games

are interpreted: let us say, the identity of an interpreted game has both formal and substantive aspects. Games may show similarities or differences in either respect. If a formal game is interpreted as a sentential calculus, and also as a calculus of switching systems, we have two interpreted games which are formally similar, and substantively different. If one formal game is interpreted as a legal code for human behaviour, and another is interpreted as a moral code for human behaviour, we have two interpreted games which are formally different, and substantively similar. Cases of formal similarity and substantive difference are not intrinsically problematic. Since the games involve different things - are, one might say, semantically distinct - they are not likely to be the source of practical confusion; it is easy enough for players to tell which game they are in. Similarly, the games do not conflict: the differences are those which arise from the game's being interpreted in different areas; and should they come together, say, in the event of someone's deciding to use the switching calculus to build a logic machine, their formal or syntactical similarity yields coherence rather than conflict. Cases of formal difference and substantive similarity are, however, a natural source of problems. The games have referents in common. Their terms will refer to what are the same objects under natural descriptions,

though not in gameterm descriptions, so the formal rules governing statements about these objects will be different. Nor does the discovery of formal diversity automatically yield coherence; it simply opens the question which game is being played. Thus, the question can be raised whether people ought to be penalized for breaking the law in certain circumstances, say, when they have a conscientious objection to the law in question. This is sometimes presented as a legal question, which it could hardly be; to see it as a moral question is to clear the air a little, but not to decide which game has priority.

So, since an interpreted game is by definition an interpretation of a formal game, two games can be identical only if they are the same interpretation of the same formal game. Thus duelling is at least formally distinct from fighting, and is therefore a distinct game. This point holds whether one regards fighting as a separately constituted game, or as a form of behaviour which is in some sense primitive or unconstituted. In neither case could the two activities be formally similar.

Together with this ground, that it is distinct, for regarding duelling as a rule-constituted activity, goes the third of the claims attributed to Searle above, that it could not be described in the same terms in the absence of the rules, because 'sword', taken as 'duelling

sword', is a technical term, deriving part of its sense from the rules. There is also, perhaps, some point to the claim that the natural form of the rules is that of an assertion rather than an imperative; but not the point that Searle seemed to intend. The basic effect of interpretation of a formal game is to impose form on the referents: by making them referents of gameterms one attributes to them the relational properties established in the rules. To say "These are duelling swords" is to attribute to them relations to each other and to the activity of duelling. To say "Take these to be duelling swords" may, depending on the context, be to do the same thing, but it is at least as likely to be to suggest that they are not duelling swords, but are the best that can be found in the circumstances. That is, the notion of the natural form of a rule may be explained through reference to its introduction through the interpretation of a formal game, and in this case the natural form may be regarded as an assertion. In what seems to have been Searle's intended view, however - that the natural form of a rule was that in which it was most commonly uttered - neither the correctness nor the relevance of the claim is clear.

The satisfaction of the fourth claim, that the rules should specify rather than evaluate, will be established in the same way. Indeed, it is not quite clear why Searle thought of rules such as rules of

etiquette as evaluative, even when he thought them regulative. What could they be supposed to evaluate? A code duello does not evaluate the quality of duelling, for this would make it self-referring; it does not evaluate the quality of fighting, since a stickler for the code could be an inept swordsman. Perhaps he thought it evaluated the form the fight took; but it does not - it specifies the form. It allows one to say, not that the fight was a poor fight or a poor duel, but that it was only a fight, and not a duel at all.

This discussion does, I believe, indicate a mistake in the tendency to regard human activities, other than games in the ordinary sense, as not amenable to analysis of this sort. The mistake is, however, a complex one; certainly not that of thinking that these activities are not rule-governed when in fact they are, for this would also be part of the same mistake. One aspect of the mistake is the tendency to impose on the instrument or process of analysis features which are already taken to be significant aspects of the domain to which the instrument is to be applied. I did this to some extent, and unavoidably, in attempting to sketch the view which I wished to oppose, by my use of 'serious' and 'frivolous'. No doubt we do commonly draw such a distinction; but apart from the question whether we draw it correctly, or indeed, what it comes to to say that we draw it correctly, it is not clear that it is a distinction that the analysis is required to reflect. Similarly,

and perhaps more pertinently, I have acknowledged the view that our behaviour in games is well-disciplined in a sense in which it is not in other spheres of activity. This is not always regarded as accidental. There is a common view on which, though it is proper for our behaviour in games to be rule-governed, our serious concerns are too serious for such regimentation to be acceptable, and the "rigidity" - a common epithet - of rule structures is more likely to frustrate than to further our purposes. Anyone sympathetic to such a view might well regard the proposed kind of analysis as inappropriate. This view is often confused: it is not always clear what story, alternative to one about rules, might be intended, or even what notion of a rule is in use; and in some cases it seems clear, even in the rhetoric itself, that substitution rather than elimination of rules is going on. But even if the view that the essentially human is essentially anarchical were clearer than I take it to be, it would not be to the main point. The question is not whether those engaged in an activity see it as, or say it is, rule-governed, but whether it can be analysed in those terms. If we found a chess Grand Master who declared that he was neither acquainted with nor interested in the rules of chess, but was inspired to act in certain ways when confronted by a chess board, we might decide that he was a liar, or invent a theory of sub-conscious rule-following to cope with the case; but we would not regard this as reason to withdraw an

account of chess as a rule-constituted game. Even if such cases became common, we would not be obliged to withdraw the account, though we might in such an event make different use of it. Generally speaking, it is not the case that a means of analysis must be consistent with, or take account of, a proposition maintained as true in the analysandum.

Another aspect of the mistake is a tendency to conflate analysis and explanation. Having, in appropriate terms, shown that some activity arises from some natural or instinctive behaviour, we may require of an analysis that it conform to this discovery. It is this, perhaps, that led Searle to say that rules of etiquette could only be regulative. But, as I have argued above, historical or ontogenetical accounts can yield only contingent, not logical or conceptual, relationships; and the formal structure of an activity may allow diverse interpretations.

These aspects are of a mistake which might best be identified as a failure to distinguish between formal rules and rules of interpretation. So failing, we import into formal systems features of the domain in which they are interpreted, or see objects in the domain as having in themselves the relations of the formal system. Confronted by a formula in Russellian notation, we take some of its signs as propositional variables,

though it might be otherwise interpreted; confronted by a matched pair of pistols from the eighteenth century, we take them to be duelling pistols, although from none of their properties could a code duello be inferred. In ordinary circumstances, this is not a mistake, but a natural feature of a well-established interpreted game in use; but it is a hindrance to analysis. Even attempts to break the spell of accustomed interpretation may go astray. Thus Frege, in a passage further discussed below, says that chess pieces acquire no new properties simply because rules are laid down;[2] and this, in a way, is clearly true. An object which might count as a chess piece has those physical properties that it has, whether it is so counted or not; so to count it does not change it, or make it subject to new physical laws. It is as it would have been had chess never been thought of, and it had been made for some other purpose. So far, this is a salutary reminder that the formal rules of chess can apply to such an object only through rules of interpretation, and it might have been perfectly satisfactory if Frege had not used 'chess pieces' to refer to the objects of which he spoke. But no list of those properties of an object which are unaffected by the rules of chess can be sufficient for it to count as a chess piece, and the formal rules and rules of interpretation relative to which it can so count confer upon it relational properties; and these rules cannot be at once invoked in its identification and dismissed as not affecting it.

I do not take this discussion as establishing the absolute generalizability of this basis of analysis; I do, however, take it to show that it is not restricted to games in the ordinary sense, and that certain general arguments for its non-generalizability are mistaken. Turning now to the test of relevance, I shall first consider the applicability of this account in terms of interpreted games to my earlier comments on individuation and conceptual structures, and to this end use the following example.

John plays chess by correspondence. For his game, he uses an ivory chess set based on medieval figures, while his opponent uses a set of boxwood turnings of conventional shapes. John determines, and signifies to his opponent, that he will move his king's pawn, which he picks out as a replica of a medieval pikeman, and his opponent as a particular boxwood turning. We can now say, of this pikeman and turning, that

(1) The pikeman is John's king's pawn

and

(2) The turning is John's king's pawn.

Taking it that (1) and (2) are identity statements, we can proceed, by transitivity of identity, to

(3) The pikeman is the turning,

which is also an identity statement. But about (3) we may feel some qualms, since there is a clear sense in which it is false; and this seems to throw strain on the

transitivity of identity.

Now, I do not offer this as a particularly difficult or puzzling case, though it is little removed from some of those with which Quine has made great play in discussions of referential opacity. The obvious answer is to say that (1) and (2) are not identity statements, but predicate something like '... represents John's king's pawn' of their respective subjects, in which case there is no inference to (3). This solution is in some respects uncongenial. It makes chess pieces into abstract objects when we are accustomed to think of them as concrete, and thus conflicts with the language in which we are accustomed to speak of them moving spatially, since although as abstract objects they no doubt have a reference frame, it is not clear that it is a spatial one. Moreover, it is a solution which would bind us also in face to face games, where it seems much clearer that statements like (1) are statements of identity. I do not think that these objections are finally telling, and will later argue that our common inclinations in this matter are mistaken; but this argument will depend upon prior application of the notion of interpreted games which is under discussion. What is clear is that a great deal depends upon whether we take (1) and (2) to be identity statements or not; and arguments upon this sort of question can, as I shall suggest below in a discussion of some arguments of David Wiggins, appear

question-begging. The arguments must draw upon some principle of analysis: the objections which I briefly sketched to the proposed solution of this minor difficulty take, broadly speaking, conformity to ordinary speech dispositions as the criterion of a sound decision. Let us see what the interpreted games account would suggest.

On this account, the formal game interpretable as chess is constituted by formal rules, of which (1) and (2) are clearly not members. (1) and (2) would be rules of interpretation, expressible in the '.. counts as ..' form, and hence statements of identification, supporting intersubstitution within the scope of the game. (3) is not a rule, either formal or interpretive, but a contingent identity - contingent, that is, upon the adoption of (1) and (2) - again licensing intersubstitution within the scope of the game. Thus, for statements which arise in the course or the description of the game itself one could use any of the identifying descriptions in (1) and (2) interchangably, perhaps for the sort of reason for which I earlier suggested one might substitute 'piece of wood' for 'bat' or 'thin end' for 'handle'; but for statements outside the scope of the game, say, concerning the histories or composition of the objects in question, intersubstitutivity fails. The reason that (3) raises particular qualms is, on this account, clearly that while (1) and (2), being rules of interpretation, explicitly relate ordinary or natural expressions to the formal

structure of the game, (3) is expressed entirely in terms of the domain in which the game is interpreted, and in this domain it is clearly false.

This is, in effect, a thesis of relative identity, by which objects may be identical in one domain and distinct in another; 'identity' here being defined by the principle of the indiscernability of identicals. This outcome is misleadingly illustrated by chess, since I shall later argue that chess pieces are part of a model, rather than an interpretation, of the formal game; but the thesis of relative identity is a consequence of the proposed mode of analysis, and a feature of the earlier discussion to which I am attempting to establish relevance, so it seems appropriate to consider an objection to it at this point. The thesis of relative identity was attacked by Wiggins in _Identity and Spatio-temporal Continuity_. He discussed a number of cases purporting to be of this type. My example, in which that which is the same chess piece is not the same physical object, appears to fall among the cases he called class 5: those where

$$(a =_f b) \ \& \ (a \neq_g b) \ \& \ (g(a) \vee g(b)) \ \& \ (g(a) \ \& \ g(b))$$

where 'f' and 'g' stand for substantive sortal concepts under which objects can be counted, and '$a =_f b$' reads 'a is the same f as b'. None of his examples quite match mine, and much of his detailed discussion is inapplicable. The general lines of argument which seem most applicable are those concerning ambiguity and the

difference between the 'is' of identity and the 'is' of predication; and these are best shown in the case in which "The Lord Mayor is not the same official as the Managing Director of Gnome Road Engineering Ltd., (indeed they often write one another letters) but he is one and the same man.[3]" Of this case he says that it

> .. exploits an ambiguity. Under one interpretation it is simply false. In fact Sir John Doe, that tireless official, is both Lord Mayor and Managing Director of Gnome Road Engineering. So they are one and the same official. The interpretation which makes the first part of the case true concerns what it is to be Lord Mayor and what it is to be Managing Director of Gnome Road Engineering. These Frege would have called concepts. And what the example then says is that to satisfy the one concept ... is not necessarily to satisfy the other.[4]

If we take this answer back to my example, it resembles in some respects, though not in all, the answer which I sketched above. There are clearly two objects, the pike-man and the turning. Both satisfy the concept of the chess piece in question, and since this is predication, clearly there are two such pieces. We then require some fast footwork to show that John does not have two king's pawns, or at least that he does not have them in a sense that contradicts the rules, just as we did in order to show that the same official could both be required and forbidden to take special interest in the well-being of Gnome Road Engineering. No doubt we can do so, by drawing further on the distinction between identity and predication; but let us return to the example in Wiggins to see how the need arose.

We were encouraged, under a light cloak of whimsy, to take 'official' as a predicate attached to 'Sir John Doe', rather than as a "covering concept" under which things were counted, and thus, unsurprisingly, to discover that Sir John counted as one rather than as two. On this basis we were led to see that, in effect, given the sentences "The Lord Mayor is the same man as the Managing Director of Gnome Road Engineering" and "The Lord Mayor is not the same official as the Managing Director of Gnome Road Engineering", the former was to be understood as an identity statement, and the latter as a second-order statement about the applicability of certain predicates which were mentioned in it rather than used. But how did we discover such a difference between such similar sentences? Both have the form required of identity statements by Wiggins, and it does not seem relevant that one is negated. The difference is the difference between 'man' and 'official'. But Frege would have called 'man' a concept as readily as 'official', so why should we not attempt some perverse interpretation of the first sentence which would raise problems just where Wiggins has smoothed them away?
It is now clear why 'Sir John Doe' got into the discussion, though it did not appear in the statement of the case. If we take it that proper names have reference but no sense, and build discussion around a proper name rather than around 'man', we settle questions of reference once and for all, and leave open only questions

about the sense of such terms as 'official', so that any problems can be presented as arising from the way misunderstanding occurs in considering the relation of predicates to an unproblematically individuated subject. However, if we adopt a description theory of proper names, say, one which renders 'Sir John Doe' as 'the bearer of "Sir John Doe"'[5], this foundation fails.

Now, it is not my concern at this point to defend a description theory of names, though such a position is implicit in much of my discussion, and my earlier discussion of individuation, identification and reidentification implies that descriptions of some sort are prior to names. My present concern is to show that it would be a misrepresentation of the position to say that I had advanced a certain theoretical basis of analysis yielding consequences, in the form of a notion of relative identity, which is shown to be false by arguments of the kind Wiggins advances, and that the theoretical base is thereby refuted. What is the case is that the theory I propose yields consequences incompatible with the theory Wiggins espouses, a theory which begins to emerge when we ask how his distinction between statements of identity and of predication is drawn. This would be a problem for me only if it were my obligation, as the proponent of a theory, to refute any theory with incompatible consequences. While no doubt such an obligation holds in some cases, and in experimental

sciences the principle upon which it is based is less an obligation than a definition of the activity, it does not hold in this case, since the principle - roughly, that alternative theories must conflict in the same domain - must be denied in any account of the theoretical framework of a relativist position. Hence I suggested above that arguments such as Wiggins's can appear question-begging in this context, since the principles on which their conclusions are based are as much a contradiction of the position I wish to develop as are the conclusions themselves.

There is a further point of some interest in Wiggins. In his discussion of the vexed case of one God in three Persons, he tries several revisions of Leibniz' Law. The last is a principle of the indiscernability of identicals, which says in effect that, if a is the same f as b, then a is ø _qua_ f, if and only if b is ø _qua_ f. He suggests that this form has the advantage of ".. indicating how f and ø would have to be related to secure a valid application of the intersubstitution of identicals"[6], a remark with obvious affinities to my comments above on the limitation of intersubstitutivity by the scope of a game. But he does not think it will do, for, although it seems to fit some cases, the revision seeks "... utterly to generalise this phenomenon, without doing anything to show us how to find a '_qua_ f' for every ø, or how to decide the

applicability or meaning of every such composite predicate"[7]. I am not sure that I understand the second problem. Cases like 'tall *qua* Japanese' seem straightforward; and though cases like 'green *qua* absolute' would be puzzling, there is nothing to show whether this is the sort of puzzle he has in mind. Perhaps the second problem is related to the first. This appears to be the suggestion that we might have on hand more ϕ's than f's; but the real point is no doubt the need for a principle by which certain f's and ϕ's were shown to be related, and others not, of which the second problem would be a consequence.

On my account, the best candidate for such a principle would be: "Identify the game which establishes the sense of 'f'". Suppose that we were offered 'heavy *qua* pawn' as an example. If 'pawn' takes its sense from our notions of physical objects, being used to refer to members of identifiable - by conventional resemblance - collections of medium-sized specimens of dry goods commonly manipulated by people, and with no significant debt to the rules of chess, the meaning and applicability of the expression is quite clear: it may be that the thing is heavier than the average pawn, or heavy enough to be awkward in hand. But if 'pawn' takes its sense from the formal rules of chess, the expression is meaningless, since 'heavy' is a symbol with no use in that context. On this account there could be no

principle less general or more substantive to be found than the prescription above; there would be as many specific principles as there were games. Nor would we look for direct relations between particular f's and ø's; they would be related through the whole structure of the game. On this view, Wiggins is led astray by the apparent presumption that only a single kind of direct relationship will meet the needs of the case. In this he resembles Ransdell, who, as I noted above, appears to assume that the notion of constitutive rules provides a basis for the analysis of speech acts only if kinds of speech act can be related one-to-one to rules. Such presumptions turn promising avenues into blind alleys. When they are abandoned, the problems which Wiggins found in his limitation of Leibniz' Law disappear, and are replaced by the problem of how the notion of a game is to be developed, with which I am now concerned.

I shall take it that the above discussion is sufficient to show the relevance of analysis in terms of games to the questions about individuation which I earlier raised, though not, of course, to decide them; and turn now, more briefly, to the other aspect of the relevance test: that of the resources this approach offers for restating and clarifying my earlier remarks about interactive bodies of belief and convention.

The distinction between belief and convention

has generally appeared as one between the empirical and the non-empirical; but the force of the distinction has varied from time to time. There has been a fact/value, or at least a fact/significance distinction. Some of the discussion in Chapter 3 suggests that the important distinction is between the general body of what might loosely be called "scientific information" and the considerations by which some parts of that information are selected as significant. Thus, there was the suggestion that selection was made of those pieces of information relevant to an interest in travel, producing a context in which the discussion of motor cars went on. This context could not be entirely "empirical", since the principles of selection were not; nor could it be entirely conventional. In this chapter, the distinction between natural and gameterms has occupied the ground, sometimes with an implicit association of natural with empirical or observational, and with an explicit association of gameterms with abstract formal systems.

Now, it seems clear enough, without further argument, that some aspects of the early notion of interactive bodies of belief and convention are reflected in the distinction between natural and gameterms, supported by the distinction between constitutive and interpretive rules. The distinctions mentioned in the last paragraph can be expressed in terms of the natural/

gameterm distinction, and the notion of an interpretive rule provides the basis of an account of interaction.

I take this as sufficient to show that the basis of analysis I propose satisfies the requirement of relevance. However, it seems equally clear that the present account needs development. Distinctions between fact and value are part of my concern; and I cannot offer as a general account of such distinctions the relation between natural and gameterms, since I have already argued that the terms of interpreted games do not evaluate anything in the domain, but rather identify it in a new way. Hence, when I commend a move at chess, the relation between my beliefs about the game and the conventions by which I appraise it is not to be unpacked in terms of interpretation, unless the notion of interpretation, and hence of an interpreted game, can be enriched by further exploration. This further exploration is the business of the next chapter.

CHAPTER 5 Interpreted games are systems of formal rules and interpretive rules.

SECTION 14 Formal games may be interpreted descriptively or normatively, and may be modelled.

A formal game, interpretable as chess, might be constructed by first establishing a frame of reference consisting of sixty-four ordered pairs, subsets of which, interpretable as ranks, files and diagonals, could be established, at worst by complete enumeration. Pieces could be introduced as initial positions and possible transpositions within this reference frame. Ultimately, a set of terminal configurations could be defined, the simplest of which would be checkmate. Clearly, such a formal game would be more complex than these sketchy suggestions indicate; but I do not think the complexity hides any serious problem. The rules of chess are statable; the complexity is just that which would be forced upon us by the loss of the convenient short cuts that the interpreted game offers - distinctions drawn ordinarily in terms of the colours of pieces, for instance, would take longer to state. This way of putting the matter suggests points that should be made about the notion of a formal game which I employ, and about the identity of games; and these points in turn lead to consideration of the notion of interpretation.

The way of introducing a formal game just suggested, while convenient, ties the formal game to a particular interpretation, and this could be dangerous. Even the notion that any formal game is tied to some interpretation may be misleading. It could lead us to overlook alternative interpretations, or to attribute undue importance to intended or actual interpretations. At first sight, this is not a serious problem: if the notion of games is to be used as an instrument of analysis, it will be used in the analysis of actually interpreted games. Uninterpreted formal games would not present themselves as objects for analysis. If, however, it is, as I earlier suggested, important not to confuse formal rules with rules of interpretation, it must be equally important not to adopt approaches which facilitate such confusion; and talking of formal games by reference to their interpretations seems to be such an approach.

In speaking of a formal game as that interpretable as chess, I seem to invoke certain criteria of completeness. It appears possible to say, of the sketch I offered, that it is not only informal, but incomplete, and to say in what respects it is incomplete. But from the fact that an account of the formal features of chess was incomplete, it cannot be inferred that a formal game, constructed to exhibit the features the account mentioned, is incomplete *qua* formal game. No

doubt certain criteria of satisfactoriness, say, concerning coherence, are applicable to formal games as such; but to express the question whether a certain formal game is interpretable in a certain way as a question about its completeness as a formal game would be to encourage just the sort of confusion I have suggested needs to be avoided. An uninterpreted game is to be regarded as a purely formal, abstract system of rules and signs. Its identity criteria are extensional: the identity of a game consists in its being constituted by a particular set of rules; of a rule in its being constituted by a particular sequence of signs. It is in this sense arbitrary in its composition: there are no criteria of completeness, no claims of omission or superfluity of the kind that might arise from considering it under an intended interpretation, that are applicable to it as a formal game.

One should, therefore, specify a formal game by a complete inscription of its rules, rather than by reference to an actual or possible interpretation. I shall, however, persist in the dangerous practice in which I have begun. The dangers are reduced when they are recognised; we should not confuse interpretive and formal rules if we remember that we are using the former to mediate our identification of the latter. Actually to specify formal games would be complex and wearisome beyond any obvious necessity; and beyond

mere questions of unnecessary use of space, the detailed specification of formal games would render necessary a consideration of questions which, though I shall briefly indicate them, are, I believe, outside the necessary concern of my present thesis.

I have spoken of formal games as systems of rules. As the use of 'system', 'rule' and 'sign' suggests, not just anything counts as a formal game. A sequence of sequences of arbitrarily chosen marks on paper can count as the inscription of a formal game only if the marks are signs, which is to say, at least, that their inscription is rule-governed. In the case of some signs, the rules for their use will be components of the game; but in order to perform this function, the rules must contain signs which are used in accordance with previously established rules. Thus a formal game, of the kind I discuss, is already the interpretation of a prior game. One might suppose that there had been a series of strings of marks, the components of which exhibited patterns of recurrence. Suppose the patterns to be arbitrary, but not accidental - the inscription of the strings is to be regarded as a kind of artistic exercise. Some of the marks might then be interpreted as signs of the predicate calculus, this requiring only that the rules governing these signs should yield those patterns of recurrence exhibited in

the strings by the marks to which this interpretation was given. This suggests that the connection between formal games and particular logical or syncategorematic systems is not in principle unbreakable. A formal game must exhibit some such system, but not any particular system. Thus I am not, in my discussion of formal games, committed of necessity to any particular system; though I am, of course, contingently committed to that system, whatever it is, which goes to constitute the formal games which, under interpretation, provide the language which I now use. I do not believe that the discussion of this contingent commitment is a necessary part of my present endeavour; nor is the discussion of what might be called preformal games which, lacking even formal sense, could be considered only as art objects, of any obvious value. However, it is worth noting that some logical and syncategorematic system is assumed in referring to that level of games which I treat as primitive.

The situation regarding the identity of a game changes when the formal game is interpreted. We might think of the formal game sketched above being interpreted by the assigning of reference to its signs by use of the '.. counts as ..' formula: an eight-by-eight squared board counting as the reference frame, each of its squares counting as an ordered pair, various objects counting as pieces, and so on. The range of possible

referents is limited by the sense that the sign has in the formal game. Other things than a conventional chess board could count as the reference frame - an eight-by-eight set of pigeon-holes, for example - but not _just_ anything other than a chess board. If one chose to count a flower-pot, say, as the reference frame, other signs in the formal game would be uninterpretable. Thus what were in the formal game, in effect, little more than rules for the inscription of signs, become in the interpreted game rules providing identifying descriptions by which the referents of terms can be picked out, or for determining the truth of statements in which the term is predicated of something.

It thus becomes possible to raise completeness questions about sets of objects, people or events in the domain in which the formal game is interpreted, on the basis of their identification in terms the sense of which derives from the formal game. We can, for instance, ask whether a set of chessmen is complete, a question which would make no sense applied to these objects identified under natural descriptions, unless the natural descriptions already involved some principle of collection, in which case the question would make sense, though not the same sense. As a related point, given the existence of chess as an interpreted game which we know how to play, we can use the practices established in this game as a basis for questioning the completeness or adequacy of

any statement or inscription of the rules.

Thus, for interpreted games, questions of individuation, identity and completeness are more complex. An interpreted game is an interpretation of a formal game, and the criteria applicable to the formal game - basically, criteria concerned with recurrence patterns - will remain necessary; but in the interpretation will no longer be sufficient. Taking as the paradigm of interpretation as this notion has so far been used the assigning of classes of physical or public objects to the signs of the formal game as reference, it is clear that individuation and identity criteria applicable to objects of these classes will also be necessary for the individuation and identification of the interpreted game, otherwise the notion of an interpreted game would not be that of a specific game, but only of a very broad game-type - in effect, the notion of a formal game as interpretable. It seems reasonable to suppose that these individually necessary sets of criteria will be jointly sufficient. Interpretation so conceived imposes a formal or theoretical structure on its referents, and in so doing creates questions about the correctness and completeness of the interpreted game. We can ask whether the statements about the referents which the game makes possible are ever true, or whether a revision of the game might not make possible more true statements about the referents. To interpret a formal game as a theory of

the motion of heavenly bodies introduces the possibility of observation and test by means of which the formal structure of the game might be revised.

But this possibility does not fit the example which was used to show the interpretation of a formal game: the case of chess. There are no observations we could make of chess boards and men which could have such consequences. Chess is an abstract game. The properties of the physical apparatus do not have the place in the game that the properties of heavenly bodies have in the game mentioned above. Indeed, one might say that the apparatus of chess has no essential place in the game at all. One might say rather that it provides a visible model of states of affairs; it aids visualization of current configurations, and generally serves as a mnemonic device; it has the function that Ransdell attributed to game-terms; one could dispense with the conventional apparatus, and write down moves on paper; given an adequate memory, one could dispense with the pencil and paper. This is not the role that heavenly bodies have in astronomy.

One must therefore ask whether it can be an adequate account of the interpretation of a formal game as chess that one assigns referents to the game terms; and also whether, if this account is inadequate, the inadequacy is due to a peculiarity of chess, or a general

defect in this kind of account.

I have suggested a distinction between giving a physical model of a formal game, and interpreting the game in physical terms. These processes were distinguished by reference to the ways in which it seemed proper to develop a "fit" between the formal game and the physical apparatus. The purpose of modelling a formal game is to exhibit the features of the game in a convenient form; any discrepancies must therefore be eliminated by modifying the proposed model. The purpose of interpreting a formal game is to exhibit a new pattern in prior phenomena; any discrepancies must therefore be eliminated by modifying the formal game. An arrangement of balls and wires, intended as a model of a theory of the structure of the atom, must be constructed in accordance with the theory; an atomic theory, interpreted as an account of the way the world works, must be adjusted to fit the phenomena which are accepted as referents of its terms.

Let us call this a distinction between a model and a descriptive interpretation - descriptive in the sense in which a formal game interpreted as a physical theory might be so called. One can then ask: are chess boards and pieces referents in an interpreted game, or models of a formal game? If the latter, what <u>are</u> the referents of the gameterms of chess? And in any

case, is the interpretation which yields chess descriptive in the above sense? A discussion of some aspects of formalism and its relation to human concerns which offers a starting point for discussion of issues of this sort is provided by Frege.[1] In a discussion of arithmetic, in which chess is put to illustrative use, he sometimes seems to take chess to be a formal game in something very like my sense, though he does not do so consistently. He says that chess pieces have no reference, in which they are analogous to the signs of formal arithmetic, another "mere game"; that a move in chess is "merely the transition from one configuration to another"; that to set someone the problem of deriving a theorem in a formal system would be like giving him initial positions and transformation rules in chess, he not having in this "the slightest inkling of the sense and reference of these signs, or of the thoughts expressed by the formulas"; that a chess problem involves the intellectual labour of "passing from an initial position to a given final position in accordance with the rules of the game".[2] He also says, of formal arithmetic, the following.

> Although intellectual labour would thus be expended, there would be wholly lacking that train of thought which accompanied the affair for us and actually made it interesting. It might be possible, but scarcely profitable; refusal to interpret the signs would not simplify the problem but make it much harder.[3]

If this is so of formal arithmetic, how much more must it be so of chess, for which the accompanying train of thought is not hidden, but never existed? No wonder such games

are mere.

So, for Frege, chess pieces do what the signs of formal arithmetic do. Or, to put it another way, if one used something analogous to a chessboard and pieces to model the configurations and transformations of the signs of formal arithmetic, one would not thereby give those signs a sense and a reference - one would merely do formal arithmetic with a different sort of thing. So, although the picture is not entirely clear, there is some point in saying that, on Frege's account, chess is a formal game, with which a physical model is associated.

This is not the customary picture of chess, though in some respects it resembles it. We do not normally think of a pawn as referring to anything; rather, we think of it as a referent of 'pawn'. Perhaps we are wrong in this. A theory does not refer to, or designate, or stand for, its model; but then, a model does in some sense stand for its theory, so perhaps the notion of a pawn having reference is not entirely incomprehensible. A move in chess is a transition from one configuration to another; but it is not merely that. It is a means to an intended end: to a goal. The transition is not pointless. We do not think of moves or positions in chess as expressing thoughts; but they have significance. Chess involves intellectual labour, and

may be valued for that, as sports may be valued for the physical exercise they provide; but not only for that. Merely to reach a final position, even a given final position, in accordance with rules, is not to play a game: when the driver says, "Move to the back of the bus, please", in obedience to this demand, and with due regard to courtesy, I shuffle through a series of positions in the bus until I reach one beyond which there is no further position, without any sense of playfulness. Frege's mere formal game is like chess, but not in every respect.

But the picture is worse when we turn to those comments in which Frege's view seems closer to the view that chess pieces are the interpretation of a game. He does not say that the rules of chess are modelled in the configurations and transpositions of pieces; he says that "... the rules of chess treat of the manipulation of the pieces". He objects to the view that "... the chess player ... assigns the pieces certain properties determining their behaviour in the game, and the pieces are only the external signs of this behaviour". He says:

> .. chess pieces acquire no new properties simply because rules are laid down; after, as before, they can be moved in the most diverse ways, only some of these moves are in accord with the rules while others are not. Even this accord does not arise from the establishment of the rules; it is only that we are unable to judge of this accord until we know the rules.[4]

Of course, some of this is rather peculiar. Without

becoming involved with the question what sort of thing he has in mind as a property, there is the obvious point that 'chess pieces' has neither sense nor reference until the rules are laid down, so the statement that chess pieces acquire no new properties through this process is at best careless. There is a similar lack of care in the apparent slide from speaking of moving pieces, which could be done in natural terms (Move those pieces from the table), to moves of pieces in accord with the rules, which seems more likely to involve gameterms; and from the legality of a move to our ability to judge the legality of a move. Of course chess pieces can be moved in the most diverse ways - put on a shelf, in a box, rolled across the floor, whatever - but there is no such thing as a move, in a game sense, which is not in accord with the rules. An illegal move is not a move at all. Of course we cannot know that a move is illegal unless we know the rules; but unless there are rules there cannot be anything of the sort for us to know. To say that the legality itself, as distinct from our judgment about it, does not arise from the establishment of the rules is simply silly.

The pieces cannot be said to acquire new properties, because they are chosen on the basis of the properties they have as physical objects, and no new properties of this kind are created by the formal game. But the properties that chess pieces have as physical

objects are irrelevant to their role in the game. It is important that they have properties that enable us to identify them; but physical objects commonly have. It is only the relational properties brought into being by the rules that matter, and this remains true whether we think of the pieces as part of a model or of an interpretation.

A more important point, suggested above, is that Frege underestimates the importance of goals, or rather, that he fails to see goals where he should. To prove a particular theorem, or give checkmate, is for him to reach a given final position, not a goal. Perhaps this is because the only kind of value he allows final positions to have is instrumental. The game must be applied; it must serve some end outside itself. But if we say that only instrumental goals count, then no goals count. Arithmetic is instrumental to science; but science must be instrumental to something outside itself by parity of argument; and the external ends that science might serve are likely to be of the same order as the goals of chess playing - something which we can take as valuable in itself, like comfort, pleasure, security or victory. To see the point of all games as lying in their being in this sense applied is surely a mistake. However, if this is a sound point to make against Frege, it is also a point that tells against the account of interpretation so far given.

If the difference between 'checkmate', understood as a technical term in a formal game, designating a terminal configuration, and the cry of victory "Checkmate!" has the sort of significance the above comments suggest, one needs a notion of interpretation other than what I have called descriptive interpretation, in which gameterms are correlated with prior things or events. The particular kind of event - the particular kind of victory - that "Checkmate!" signals cannot be a prior phenomenon. Perhaps interpretation requires victories as a genus; but the interpretation introduces a new species. Interpreting a formal game as chess has more to do with correlating its terminal configuration with such terms as 'goal' and 'victory', that is to say, with assigning to these configurations intrinsic value, than with correlating its terms with physical objects.

Thus the defect in the account of interpretation was not so much the stress on reference, as the failure to distinguish between kinds of referent. The ideal referent in a descriptive interpretation would be a publicly observable object or event. Although I did not explicitly restrict interpretation to such referents, the obviously paradigmatic cases tend to obscure the view of phenomena which would be, in Ryle's terms, referents of achievement-words. These phenomena are not unrestrictedly publicly

observable; they are observable only by an informed public. Anyone can observe a runner breaking a tape; only those who know about races can observe him winning one. To the extent that this was what Frege meant by his comments on rules, moves and pieces, his comments were correct and significant. To the extent that he meant to say that winning a race was nothing different from breaking a tape, he was wrong. Winning a race is breaking a tape in a certain social context, which includes the intelligibility of both descriptions. In recognising such a victory, one does not simply make a familiar phenomenon the referent of a new term; one recognises a new kind of phenomenon. Of course, one needs a relation to a natural term of some kind. 'Breaking a tape', or something of the sort, is correlated with 'winning'. But this is not exactly like correlating 'flash of light in the sky' with 'lightning', and explaining it as an electrical discharge. When we call breaking the tape winning, we do not explain it as a victory in a race; rather, we count it as a victory in a race, and its so counting shows the point of racing.

That this is a defect in the initial account is a point with general significance. It applies to games like cricket, in which the physical apparatus contributes to the game, as well as to abstract games like chess. Here also, the terms that refer to actions in a context of purpose are more central in the

interpretation than the terms that refer to things. The domain of possible referents for 'bat' and 'ball' is narrower than that for 'pawn' and 'knight'. As in the case of baseball, one could not play with a stick of spaghetti for a bat and a nine-pound shot for a ball. But this is because 'bowl' and 'strike' are interpreted as referring to a certain kind of action. The relative formal game can be interpreted as a board game by taking certain moves of pieces as referents of 'bowl' and 'strike', and substituting a randomizing process like rolling dice for physical causes. In such an interpretation, constraints on the interpretation of 'bat' and 'ball' would be no different from those affecting 'pawn' and 'knight'. Of course, cricket would then be a different game; but the point is that cricket is the game it is not primarily because some of its terms refer to certain objects, but because some of its terms refer to certain goal-directed actions.

This is not the notion of interpretation to which I above attached the expression 'descriptive'. A formal game interpreted descriptively would refer to objects and events rather than actions and purposes, nor would any referent count as a goal, achievement or victory. When Darwinian theory is presented, not in terms of random mutation and natural selection, but in terms of the development of characteristics which promote survival, and when the survival of individuals and

species is spoken of as though it were their purpose, in which they might succeed or fail, then certain changes take place in the game. The sort of argument that would suffice to establish that a species survived in an environment because of a particular characteristic would not suffice to establish that it had that characteristic in order that it should survive: to put the matter briefly, causal and teleological explanations differ in structure. Even reference could shift. The expression 'survival-promoting change', in a causal explanation need not have the same sense that it would have in a teleological one, nor is there any obvious reason to think that it must pick out the same characteristic.

In descriptively interpreting a formal game, we impose a particular pattern on phenomena, or make phenomena intelligible in a particular way, but we do not interpret any part of the formal game as an action, purpose or goal. To interpret formal arithmetic by making its signs refer to numbers would be to interpret it descriptively. This does not preclude the possibility that the game, so interpreted, might be useful in some goal-directed activity. Similarly, if a formal game were interpreted as a theory of physics, some of its referents might well be objects which had a place in goal-directed activities. One can use arithmetic to check one's bank balance, and physics to plan nuclear reactors; but keeping one's affairs in order and finding

sources of energy are activities in which such descriptively interpreted games are applied, rather than part of their interpretation.

If the problems I have found in Frege's discussion can be regarded as involving a particular mistake, that mistake would be that, although a distinction between the interpretation of a formal system and the application of a descriptively interpreted system is implicit in his remarks, it is never clearly drawn. Because of this, the possibility of distinguishing between descriptive and (what I shall call) normative interpretation did not arise, and chess was treated as an uneasy mixture of formal game,and trivial and pointless descriptive interpretation. On the account I have given, formal arithmetic is a formal game, and arithmetic is a descriptively interpreted game with many applications. Chess is best identified as a normative interpretation of a formal game, with which a certain limited range of physical models is strongly conventionally associated.

Normative interpretation, considered, as in the case of chess, as essentially involving the attribution of value to some aspect or stage of the formal game, leaves the individuation, identity and completeness of the game relatively untouched. As already noted, if the standard apparatus of board and pieces is regarded as

an interpretation, there is some tendency to see individuation and identity as complicated in the same way as in descriptive interpretation; but if this error is cleared away, the only remaining complication in the identity of the game is its involving the possibility of a particular kind of purpose or victory. But this does not introduce any notion of completeness or correctness criteria applicable to the game over and above those it had formally, since the particular purpose or victory kind involved can be identified only by reference to the identity criteria of the formal game.

Of course, chess is atypical. In the case of cricket, where the normal interpretation involves the assigning of physical referents to some game terms, decisions on the proper identification of a game may well be affected by information of the physical apparatus with which it is played. However, even here, the case is unlike descriptive interpretation in that the physical apparatus is designed to interpret the game, and does not introduce any possibility of observation and test by means of which the formal game structure might be revised.

Provisionally, one might say: in a descriptively interpreted game, the relations the game imposes on the public referents are external to their relata, and the individuation and reidentification of a game is extensional, that is, it depends on the individuation and

reidentification of the relata. In a normatively interpreted game, the relations the game imposes on the public referents are internal to their relata, and the individuation and reidentification of a game is intensional, that is, it depends on the individuation and reidentification of the relation. In some cases, like that of cricket bats and balls, the characteristics of an object may constitute what Strawson called logically adequate criteria for the existence of the relation.[5] In the case of abstract games like chess, where the physical apparatus is a model rather than an interpretation, the characteristics of the pieces could not constitute a logically adequate criterion for the existence of the relations established in the rules, since game terms are not defined in terms of the characteristics of such objects, and no causal relationships tie pieces of any particular sort to the game; but, where conventional patterns are sufficiently well established, it may be proper to say, in Searle's terms, that they are conventional indicators of the existence of the relation in question.[6]

The account I have given of chess differs from ordinary ways of talking, which I think in many respects, including some degree of inconsistency, are closer to that quoted from Frege. Chess is commonly said to be constituted by a set of pieces and the rules for manipulating them. This is not a problem in itself. It

is not a necessary condition of success that an analytic study should yield ordinary ways of talking. However, it should be reconcilable with them; and indeed should show that these ways of talking, if they are not in some way mistaken, are reasonable, given the result of analysis. Since the point of a model is to serve as an immediate focus of attention, and since in the case of the model of a game there are not the consideration which, in the case of theoretical models, lead one to maintain an explicit process of comparison between the object and the model, it is reasonable that, where an abstract game has been modelled in a certain way for a considerable period, the model and its object have come to be identified in ordinary ways of talking. That the identification is only a way of talking is shown by the readiness with which those accustomed to talk in this way can be brought to see the variety of possible models, and the in-principle-dispensability of physical models. What is not dispensable, though Frege's account endeavours to do without it, is the interpretation of the final state as a goal.

At the conclusion of the last chapter, I took it as evidence that further development of the notion of interpretation was required, that aspects of the important fact/value distinction did not seem readily expressible in the terms I had then presented. The distinction between descriptive and normative

interpretation improves the situation, bearing directly on a central area of debate. It offers an obvious basis for one kind of argument against the possibility of a value-free theory.

A descriptively interpreted game in which none of the referents were significant for any normatively interpreted game would be of no practical interest to anyone. This, of course, is a tautology. It is contingently very unlikely that such interpreted games would be developed, or would attract much interest if they were. This point is much clearer when it is seen that normatively interpreted abstract games like chess are excluded from the possible range of cases. Thus it is at least highly probable that any descriptively interpreted game will figure in some purposeful activity, and therefore possible that the process of interpretation was undertaken in the interest of that purpose, and that where latitude in interpretation existed, that purpose was decisive in the outcome.

There is, however, no basis here for the more radical version of the argument against value-freedom, which involves the repudiation of any significant fact/value distinction, for the distinction between descriptive and normative interpretation is, of course, itself a fact/value distinction. Still, the less radical

thesis develops strongly in this context. Although a descriptively interpreted game is, in the sense that the descriptive/normative distinction defines, value-free, the activity of descriptively interpreting a formal game is itself a goal-directed activity, which would under analysis reveal a basis in normative interpretation. Hence the less radical thesis is more than just the suggestion that people may have purposes in mind in non-obvious ways. It would rather be the suggestion that normative interpretation is a necessary part of any intelligible structure.

## SECTION 15 Expressions permitting reference are derived from interpreted games.

The preceding section introduced a distinction between descriptive and normative interpretation, based largely upon the difference between assigning physical objects or events as the reference of terms in formal games in the former case, and valued states of affairs of certain kinds in the latter. I shall attempt further to elucidate the general notion of interpretation, and the two types of interpretation I have distinguished, by a consideration of various relations involved in the interpretation of formal games, and of kinds of expression through which reference is achieved.

The notion of a formal game has been introduced as an abstraction from ways of acting. The reciprocal process - concretion, perhaps - would be that by which the formulæ of a formal game came to express propositions in an interpreted game or a way of acting: came to say something, or to be usable for the purpose of saying something. The best example of such a process is the interpretation of abstract formal systems as systems of logic, for even if Strawson is correct in the view, quoted above, that the designers of formal systems always have an interpretation in mind, it is easy to imagine the case in which he would be wrong. Now, it seems reasonable to suggest that there are

conditions to be satisfied in this imaginary case. To slightly correct a point made above, not just anything can serve as a model for the interpreted game chess. To propose that a flower pot model that part of the game which is conventionally modelled by a squared board would be to create a problem, which might be described either as the problem that nothing could then serve in the place conventionally filled by the squares of the board, or as the problem that nothing chosen in this place could stand to a flower pot in a relation in any way comparable to the relevant relation in the game. Similar issues must arise in the case of interpretation. On the account given, it is necessary in interpretation of a formal game that some expressions of the formal game have reference in the domain in which it is to be interpreted; but whether this is sufficient, or what would count as sufficient, are questions which require further consideration of the notion of interpretation.

Four kinds of relation are involved in this notion. First, there are the relations established between terms in the formal game. That there is such a set of relations is part of the notion of a formal game. Second, there are the relations holding between elements of the domain. On the assumption that something is available as a domain only to the extent that it is articulated, and that this requires prior interpretation, these relations will be exressed in terms of

the prior interpreted game involved. Third, there is the relation of reference in which the elements of the domain stand to elements of the formal game. Fourth, there is the relation, earlier called "fit", and significant in the comments immediately above, holding between the relations mentioned first and second.

The fourth relation, which I shall continue to call fit, is obviously crucial. It is a relation between sets of relations. It is like isomorphism; but it cannot be called isomorphism, since what the relata would thus be said to have in common is itself one of the relata. It is a difficult notion to characterise, since it and reference together in effect constitute interpretation; and, as the example above of alternative problem descriptions shows, it is not clearly distinguished in my account so far from the relation of reference.

It may be of some illustrative value to note that the expression 'fit' is used by Goodman, in the work earlier mentioned, in a similar way. He says:

> Briefly, then, truth of statements and rightness of descriptions, representations, exemplifications, expressions - of design, drawing, diction, rhythm - is primarily a matter of fit: fit to what is referred to in one way of another, or to other renderings, or to modes and manners of organization. The differences between fitting a

> version to a world, a world to a version, and a version together or to other versions fade when the role of versions in making the worlds they fit is recognised. And knowing or understanding is seen as ranging beyond the acquiring of true beliefs to the discovery and devising of fit of all sorts.[1]

In the account I have given so far, that we can introduce forms of description, and use them in the making of true statements, is "primarily a matter of fit" in the sense that it is the mark of the successful interpretation of a formal game. I shall go on to characterise other relationships between games which turn primarily on the relation between a formal game and the domain in which it is interpreted, so that something like fit to "other renderings, or to modes and manners of organization" is also part of my concern. However, I do not wish to consider "fit of all sorts", but only fit of that sort that the notion of interpretation of games gives rise to. Further consideration of this notion, particularly as it involves a descriptive/normative distinction, seems desirable.

For this purpose, suppose that we have a formal game F, having as elements the terms a and b; that we have a domain D, including in its elements f's and g's; that 'All a's are b's' is a rule of F, that no f's are g's in D; and that the first interpretive rule proposed is 'f's count as a's'. To the proposal 'g's count as b's', can we

respond by accepting the proposal, and going on to point out that F should be revised so as not to require false statements; or should we reject the proposal on the ground that, since all a's are b's, and no f's are g's, g's cannot count as b's? On the first response, fit and reference are clearly separated, so that 'a' and 'b' have the force of proper names, and 'All a's are b's' has the force of a contingent hypothesis. On the second response, fit and reference are taken to be indissolubly related, so that 'a' and 'b' have descriptive force, and 'All a's are b's' has the force of a tautology or a definition.

The previous paragraph presents these responses as though they were mutually exclusive, and some seem to have taken them to be so. Thus Scheffler, in the arguments which I discussed in Part I, seems to have chosen the first response; but the alternatives recur in the claim of indeterminacy between empirical truth and definition, which I there took to be a significant feature of his discussion. I wish to argue that, although the alternative responses are in a sense mutually exclusive - one could not make both responses to the same case at the same time - they are in a very important sense mutually dependent. Some further consideration of the business of assigning reference to expressions may serve to make this point more clear.

Reference presupposes the individuation of referents, and individuation, by the argument advanced in Part I, entails identification under some description. Putting aside for the moment the question of names, the basis of identifying description is the common noun: the basis of an identifying description of Fido is 'dog'. This example seems to be of a case in which the extensionalist decision which I attributed above to Scheffler seems quite reasonable. Of course, 'dog' is related in various ways to other expressions like 'cat' and 'vertebrate' and 'kelpie'; similarly, Fido is related in various ways to her environment. No doubt, we could not suppose 'dog' to be useful if there were no other expressions to which it was related; nor would we take seriously, except as a rather odd sort of philosophical example, the notion of a universe which consisted only of Fido. But these considerations do not seem to intrude too seriously on the relation between 'dog' and Fido. They bear on the general business of reference of this sort, but they are not a problem for the particular case. If we can list the non-relational properties that define 'dog' - if we can give, so to speak, construction specifications for a dog - and if Fido has these properties, then we can identify Fido as a dog. There does not seem to be a problem about fit. If we can identify Fido as a dog, it does not matter if some things which we took to be part of being a dog turn out not to be so: we have still a basis for reference.

Indeed, it may be only because we have achieved a basis for reference that we are in a position to find out that we cannot achieve fit.

But 'dog' is not the only noun that might do the job. We can refer to Fido using 'sheepdog' or 'lapdog'; we can refer using 'companion' or 'pest'; we can refer - puzzlingly in this context - using 'Fido'. In the first case, the properties by virtue of which she qualified for the extension of 'dog' remain relevant, but to these are added other and relational properties, and these relations must be satisfied if the identification, and hence the reference, is to be effective. In the second case, the properties she has as a dog are relevant only marginally, or not at all; but a broader range of relational properties, in this case more explicitly involving the notion of purposes or interests and their satisfaction, is required for effectiveness. In the third case, it is not clear that any properties, relational or otherwise, are relevant, unless we are to count such properties as having been named Fido, or being the bearer of 'Fido'.

So the cases are uneven. We can refer to Fido as a dog, or as Fido, without regarding ourselves as thereby committed to any particular view about the fit of the formal game involved and the domain; but to refer to Fido as a sheepdog or a companion seems to

involve some serious commitment along these lines. If the relations between 'sheepdog' and 'sheep' and any other relevant terms in the formal game are such that the things Fido herds - supposing that she does herd something - cannot properly be referred to as sheep, then we cannot refer to Fido as a sheepdog. If 'companion' implies a certain kind of relationship to another individual of some sort, and if the domain does not provide any referent for 'companion', then we cannot refer to Fido as a companion.

I should stress, at this point, that the comments above are <u>not</u> equivalent to the comment that certain predicates are not true of Fido. Since in fact we can, and normally would, identify Fido as a dog, it would be natural so to read the above sentences; and I have contributed to this by using 'Fido' as though it were a sort of noncommittal pointing device. But my comments above should be read as applying to a case in which <u>only</u> the expression under consideration is available as an identifying description. In such a case, if we could not identify anything as a sheepdog or as a companion, we could not identify anything at all; and, given that reference presupposes identification, we cannot refer. Hence "We cannot refer to Fido as a companion" is to be read not as "'.. is a companion' is false of Fido", but as "'Fido' names something only if that thing is identifiable as something other than a companion".

Let us sort out and identify more clearly the cases I have mentioned. Cases of the first kind, in which Fido is identified as a dog, I shall say involve categorizing descriptions; cases of the kind in which she is identified as a companion I shall say involve evaluating descriptions; cases of the kind in which she is identified as a sheepdog I shall say involve mixed descriptions; and cases in which she is referred to as Fido I shall call naming. One can then order these kinds of description as follows, basically in terms of the range and kind of properties of the referent involved in effective reference.

In naming, unless we adopt some kind of description theory of names, we assume nothing about the properties of the referent. Of course, there must <u>be</u> properties, since if one could not predicate <u>something</u> of that named, a christening ceremony could not be effective; but no particular class of predicates is involved. Even on a description theory of names, the posession of a property such as being the bearer of the name is not helpful.

In categorizing descriptions, non-relational properties of referents are involved. The ideal examples of non-relational properties would be sensible qualities such as colour. The use of 'sensible' of

course suggests a relation, but I think not a trouble-some one. Roughly, the point would be that while there might be implicit and indirect reliance on the notion of individuals other than the one in question, to recognise a non-relational property would not involve the explicit or direct recognition of any other individual.

In evaluating descriptions, only a particular kind of relational property is involved: properties establishing a relation to some individual in terms of a purpose or interest of that individual. Of course, there must be _some_ property of the referent by virtue of which it stands in that relation, and this property may be non-relational; but to have such a property is not in itself sufficient to support identification under an evaluating description, nor need the evaluating description contain or entail reference to any particular property.

In mixed descriptions, both relational and non-relational properties of the referent are involved.

Now, although I have grouped names and categorizing descriptions together, as those kinds of expression for which the development of a fully satisfactory fit does not seem necessary in order that the expressions be used to refer; and I have grouped evaluating and mixed descriptions as those kinds in which it does

seem that fit is necessary for use in reference, this is not a simple opposition of pairs. Names are not tied to categorizing descriptions in any direct way. In terms of the distinction between individuation and identification, naming appears unique in involving only individuation, while the other forms are all identifications.

However, according to the argument I offered in Part I, individuation entails identification: we cannot pick something out without picking it out as something. The claim to identify may be very tentative, and the sort of thing very general, but that does not affect the point. On this account, naming is parasitic upon the forms of description mentioned; and since categorizing descriptions are the only form that I have allowed to be available in the absence of fit, when names are also usable, it seems that there must be some strong relation between them: perhaps the feeling that a name attaches in some very direct way to its referent is reflected in the feeling that it must be more closely tied to descriptions which say what the referent is than to those which say what value it has, and this common availability of categorizing descriptions and names in difficult circumstances looks like supporting evidence for the feeling. But I think this is a mistake. On the earlier assumption that the availability of a domain requires that there be an interpreted game prior to the formal game to be interpreted, there must in any case be

available descriptions which permit reference to those objects or events or states of affairs which are to fill the first space in interpretive rules. If interpretation of the new formal game is successful, the names can be regarded as applying to individuals identified under the descriptions thus made available; if it is not, we retain our grip on individual referents of the names under their old descriptions. This, I assume, is what made it possible for Scheffler to distinguish as sharply as he did between individuation and categorization, and regard reference as undisturbed by changes in systems of categories. On this account, individuation cannot be independent of every system of categories. If there were no interpreted games, naming would not be possible. But since we are not generally required tc suppose that all interpreted games are in question at once, the point is not forced on our attention: even when Scheffler pushed his argument to the point of supposing differences in individuation, he preserved the notion of <u>some</u> common set of descriptions. Still, when terms in an interpreted game are used to refer to entities reference to which would not have been possible in prior interpreted games, e.g., 'electron' or 'neutron' in modern physical theory, the supposition that these theories were dismissed seems clearly to involve loss of reference. Even if we supposed that we had a pet electron called Fred, we cannot suppose that dismissal of the relevant theory

would leave us with something for which we had a name, but no category or hypothesis. 'Fred' would be a useless noise; at most, we might have a vague sense of loss.

There is thus a clear relation between the earlier assumption that interpretation required an articulated domain, and the claim that individuation entails identification. If interpretation involves providing terms with reference, and if this requires the individuation of referents and hence the identification of referents prior to the interpretation, then any interpretation must be regarded as presupposing another interpretation. This generates a regress, which could be vicious. In fact I believe it is not; the argument for this view will be presented in Chapter 7.

If we now divide the four forms of description in a different way, putting aside naming and mixed description on the ground that each is, in its own way, a derivative form, we are left with categorizing description and evaluating description, which offer a clear contrast. The former does not, while the latter does, make fit a necessary condition of reference; consequently, the former does, while the latter does not, permit significant revision of the formal game as part of the process of interpretation.

This contrast, of course, relates to the family of contrasts which constitute the fact/value distinction. The contrast between empirical or observational and non-empirical characteristics, the previously mentioned contrast between internal and external relations, those contrasts involved in discussion of the is/ought gap, the the naturalistic fallacy, and even the problem of other minds: all these come together at this point. To the extent that empirical or natural or observational or public properties of individuals coincide with what I have called non-relational properties, or those properties characteristic of individuals whose relations are generally external, all these contrasts turn on the same distinction: that between the sort of statement that is in principle confirmable or disconfirmable by reference to properties of this sort, and the sort of statement that is not.

The contrasts are based on a distinction which invites discrimination between 'Fred has a new coat' and 'Fred ought to have a new coat', between 'Truth-telling tends to maximise the balance of human pleasure over pain' and 'Truth-telling is good', and between 'Fred behaves as I behave when I'm in pain' and 'Fred's in pain'. And this is, in effect, the difference between those statements in which truth-telling, or Fred, is identified on the basis of categorizing descriptions, and those involving evaluating descriptions. To refer to Fred as

one having rights, or as a conscious agent, or to refer to truth-telling as a morally significant activity, is to refer in ways which presuppose a game context which is not revisable on the basis of any external relations into which the referent may enter; to refer to Fred as having a certain sort of covering, or to truth-telling as a phenomenon with causal consequences is not. Thus categorizing descriptions can be thought of as extensional, since their reference is protected from changes which might be made in the game from which they derive their sense, or formal definition; evaluating descriptions, whose sense requires satisfactory fit for the whole game, cannot. To illustrate these features of the contrast, we might take as an example of categorizing description science, considered as a value-free activity, in which reference to individuals under any common description provides the basis for an interpretation of formal games in which fit is achieved by formal revision, and the sense attributed to game terms depends upon the revised form of the game. As a comparable illustration of evaluating description we might take chess, on the earlier account on which physical apparatus is a model rather than an interpretation, in which primary reference is to situations as valuable, and individuals are identifiable only in terms of their going to constitute these situations.

Thus we have a general account of interpretation as the establishment of the possibility of reference

relations between expressions of a formal game and elements of a domain; and a distinction based on the relation between the establishment of reference and the establishment of fit between the formal game and the domain. Where the establishment of reference entails the establishment of fit - where evaluating descriptions are the basis of identification - we have what I have previously called normative interpretation. Where the establishment of reference does not entail the establishment of fit - where categorizing descriptions are the basis of identification - we have what I have previously called descriptive interpretation. This is, in effect, the standard fact/value distinction, underlying notions like those of an is/ought gap mentioned above. It is also the distinction underlying the problem of other minds, taking this problem to concern the question whether reference to individuals as agents is a reference based on evaluating description, or an hypothesis based on categorizing description of some object.

This further comment supports those comments on the fact/value distinction which I made at the end of the last section. The fact/value distinction is far from simple and clear-cut. Purely descriptive interpretation, for instance, is most easily understood as an abstraction from a way of acting. A developed body of theory is the result of choices which cannot be explained, or even acknowledged, in the terms allowed by descriptive

interpretation. This is the difficulty with Popper's World 3. If it is to be really independent of the subjects and agents and purposes and interests of World 2, it must somehow manage to map unproblematically onto World 1, and the only thing which could do that would be World 1 itself. Of course, we can abstract theories from the context in which they were developed, and we frequently have good reason to do this; but such abstracted theories are not independent. Similarly, purely normative interpretation is best regarded as an abstraction. Even in the favourable case of chess, on the given account, one abstracts from the contexts in which the playing of chess goes on and is understood; and these contexts involve descriptive interpretation, if only to give some basis to the notion of a player.

But the fact/value distinction, though not clear-cut, is not undrawable. It is not impossible to consider either the descriptive or normative aspects of a way of acting in the abstract. There will be limitation, though different limitations, in each case: the success of reference based on categorizing descriptions, and the more or less conjectural external relations between referents, will not be problematic, though the significance of the reference may be; the significance of reference based on evaluating description will not be problematic, but its success may be, to the extent that successful reference depends upon non-relational

properties of, and external relations between, the referents. The point can be illustrated by a brief comment on reference based on mixed description.

Perhaps the best examples are artefacts, which offer both characteristic patterns of non-relational properties, and a function established in a context of the purposes and interests of agents. We can provide construction specifications for, say, a knife, which, while showing areas of indeterminacy, will not be less adequate than those we could provide for a dog; and we can do this on a minimal basis: a small child can learn to recognise dogs and knives at sight. So 'knife' could be a term with reference clearly based on categorizing descriptions; but generally is not. If something qualitatively identical with my pocket-knife were produced by some freakish combination of natural forces, it is not entirely clear that it would *be* a knife. By a suitable agent, it might be *used* as a knife, and in time it might come to be, by that agent, *counted* as a knife; but this is not quite the same thing. Similarly, a flat rock or a tree stump might be used as a table without being a table, even if, at times, similar objects had been made of stone or wood, and these objects *were* tables. To be a knife is to be, *inter alia*, a possible referent in a way of acting in which cutting is a significant action. At the same time, although we could perhaps do something with the notion of a knife as a

cutting instrument, abstracting from all questions about what sort of process, in descriptive terms, cutting was, what sort of things there were to be cut, and what sort of instrument permitted the process, we could not do very much. 'Knife', considered as a purely categorizing description, would allow us to pick out things, but not to determine the significance of picking out those things; considered as a purely evaluating description, it would allow us to talk about significance, but not to attribute significance.

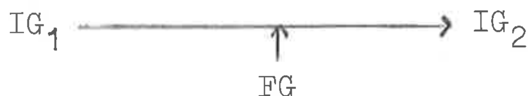
Thus reference based on mixed description should be considered not as a mixture of in-principle-independent purely descriptive and evaluating forms, but as the basic form of reference in a way of acting, from which categorizing and avaluating forms may be abstracted. This restates, in different terms, my earlier claim that this thesis is to be seen as an exercise in the analysis of ways of acting, rather than as an account of their ontogenesis. It also makes it clear that the notion of a way of acting is not identical with the notion of an interpreted game, and is to be further explained. I shall explain it as a system of interpreted games; this account is the business of the next chapter.

CHAPTER 6 Ways of acting are systems of interpreted games.

SECTION 16 Interpretation generates structures of interpreted games, in which 'way of acting' may designate units of rational discourse.

I shall now proceed to consider some of the implications of the foregoing for an account of the structure of a way of acting. First, I shall establish a more clearly defined body of terms for the discussion of such putatively significant units.

On the account given, what might be called a molecular unit of sense and reference is constituted of a domain established by at least one interpreted game, a formal game, and the interpreted game which results from interpreting the second in the first. This could be represented diagramatically after the fashion in which Toulmin, in The Uses of Argument, represented an argument proceeding by substantial rather than by analytic rule, viz.,




$IG_1$ and $IG_2$ being interpreted games, and FG a formal game. This is a useful model, since the place occupied by 'FG' above was in Toulmin's examples occupied by a statement providing the warrant by which the argument proceeded. In a very similar way, FG provides a warrant by which

predicates drawn from $IG_2$ can be inferred on the basis of predicates drawn from $IG_1$; and a formal game can be readily seen as the basis of a system of warrants of the sort Toulmin discussed.[1]

I shall say of such cases as that illustrated that FG consists of constitutive rules, and that $IG_2$ is an interpretation of FG based on $IG_1$. This gives to 'constitutive rules' a sense similar to that used by Searle and discussed above, but with a difference which that discussion foreshadowed. I shall not distinguish between constitutive and regulative rules as he did in distinguishing between chess as a new activity constituted by constitutive rules, and dining as an old activity - eating - regulated by regulative rules. In any case we will expect to find something that stands in a similar relation to a rule-constituted activity to that in which eating stands to dining, the relation in question being that represented by 'based on' above.

I shall use 'interpretive base', or more briefly 'base', to refer to parts of the domain - of $IG_1$ in the diagram above - necessary for the interpretation of FG as $IG_2$. As some remarks above suggest, the precise limits of the base in any given case may be hard to determine. The base will include whatever is necessary for the individuation of things to be

identified by expressions of $IG_2$. It may or may not need to include means for the identification and re-identification of these things. The criteria for reidentifying a golf ball, although of great importance in the game, involve those properties which it has as a physical object, rather than any properties conferred on it by the rules of the game of golf. On the other hand, the criteria for reidentifying the Prime Minister of Australia are not those for reidentifying any particular man, but rather those arising from an account of the office. Of course, if we happen to believe that Malcolm Fraser is the Prime Minister of Australia, we may find it more convenient to reidentify him; but this is only a matter of convenience, and we could be led astray by falling behind on current events.

I shall say that an interpreted game presupposes its base. This is roughly equivalent to saying that the base must be assumed, or taken for granted, or regarded as necessary (synthetic a priori, perhaps) within the interpreted game. It is similar to the Strawsonian notion of presupposition, taken to be that 'p presupposes q' is defined as 'that p has a truth-value implies that q' in that sentences in the interpreted game make true or false statements only if the base is accepted; and in that the breakdown that occurs if the presupposition fails affects reference: to motor cars in the example

which I discuss in Part I, and to John's children in Strawson's case.[2]

I shall say, in cases such as that in the diagram, that $IG_1$ is prior to $IG_2$; and generally that $IG_i$ is prior to $IG_j$ if and only if $IG_j$ presupposes all or part of $IG_i$. Thus priority is a transitive relation. It therefore provides the basis of a vertical or hierarchical structure of interpreted games.

Two or more interpreted games may have a common base, or at least a common prior interpreted game. Thus, an interpreted game which gave a use to a notion of competition might provide the base for a number of games, in the ordinary sense of the word. This might be made to yield a more satisfactory notion of a game than a Wittgensteinian account in terms of "family resemblances". Family resemblances, after all, hold between members of a family; my double may not bear a family resemblance to me. The notion of a family resemblance as an overlapping of sets of characteristics, without the unifying focus provided by blood relationship in the normal use of the expression, is not illuminating. The notion of a common base can fill the gap without requiring games to have any single common feature.

Where interpreted games are related by a common prior interpreted game, I shall say that they are

materially related. Where they are related by being interpretations, on different bases, of the same formal game, I shall say that they are formally related. Both material and formal relations are transitive, and, with relations of priority, allow the formation of more complex systems of interpreted games. The possibility of identifying such relations between interpreted games does not involve the assumption that games so related must be consistent or compatible with each other. As distinct from priority, which involves a notion of fit as part of the notion of interpretation, formal and material relations may involve stresses. Competing physical theories, for instance, are materially related; formally related games would not compete in this way, but could be a source of error in the way that an ill-chosen or over-extended analogy or metaphor could.

In the terms so far explained, I shall now offer a preliminary account of notions of a way of acting and of a form of life, and further develop the account of these notions in this and the following chapter.

I shall give to 'form of life' that sense which it has in some of Winch's uses discussed in Part I, where it is used to speak not of some distinguishable portion of experience or understanding, such as religious as distinct from scientific life, but of life as some sort of more or less coherent whole: as that "... unity in ..

multifarious interests, activities and relations with other men"[3] which allows us to say of these distinguishable portions that they are portions of the same thing. I shall indicate this sense in the present context by defining a form of life, relative to a given interpreted game, as the sum of all those interpreted games having priority or material relations to the given game. I deliberately exclude reference to formal relations here. In cases like that which I gave above as an example - of sentential calculus and a calculus of switching systems as formally related - their relation in one form of life can be established through priority and material relations: roughly, both can be presented as interpretations in a domain of human behaviour. Hence, in such cases, reference to formal relations is unnecessary. It would be necessary only where games were related in no way other than formally. For now, I shall simply assert that a form of life cannot contain two interpreted games which are only formally related; the point will be further discussed below. Hence appeal to formal relations in the account of a form of life is either redundant or mistaken.

It will be part of the point of my further discussion to argue that while a form of life defined in this way is a useful notion, and not, for instance, one within which irremediable breakdowns in communication need be supposed to occur, it is a context which permits the

occurrence of disputes for which no rational resolution is possible. Now, we can think of domains of discourse for which this is not true. Games, in the ordinary sense, commonly offer such "rational domains". Any question which can arise within a game of chess can be answered, in principle conclusively: the point, or one of the points, to be observed in designing games is to approach as closely to this ideal situation as possible; often this is done by introducing the notion of an umpire, not so much to give the correct answers in awkward cases, as to constitute a standard of correctness. Now, being in this sense a rational domain, or at least approximating to a rational domain as an ideal, was a characteristic which I attributed to ways of acting when, in Part I, I first introduced this expression as an informally technical term. The irregularities encountered in pursuing a regress of reasons were, I then suggested, the result of reaching the limit of what I have now called a rational domain, and being consequently obliged to shift into a new context.

I have just said that forms of life, as I have defined the notion, are not rational domains; I said at the end of the last chapter that we must regard both descriptive and normative descriptions as isolatable only by abstraction, and shall further argue below that it follows from this that an interpreted game can be a

rational domain only under certain circumstances. Hence if 'way of acting' is to have the sort of use which I initially intended, it must identify a unit more complex than a single interpreted game, and less complex than a form of life. It will help in the task of saying how such a unit might be identified if I turn now to consider certain kinds of internal tension which might arise in a form of life, and affect its status as a rational domain.

## SECTION 17 Three kinds of relation between statements and statement sets create internal stresses in a form of life.

I shall distinguish between contradiction, incompatibility and dissonance, which I shall regard as relations holding between statements or statement sets.

Contradiction holds only between statements in the same interpreted game. A contradiction, in the form 'p and not-p', requires that 'p' have the same sense in both occurrences, and consequently that the terms constituent of 'p' have the same sense. The sense of an expression in an interpreted game has a formal and a material component, the former from the relations established in the relevant formal game, and the latter from the domain in which the formal game is interpreted. Thus distinct interpreted games must be distinct either formally or materially, so expressions drawn from different interpreted games cannot have the same sense, so statements in different interpreted games cannot contradict each other.

Where interpreted games are materially related, and result from normative interpretation, they may deliver statements which are not on the face of things in conflict, but which cannot hold concurrently. Draw

poker and five card stud are formally different, and both are normatively interpreted in the same domain of people, purposes and objects. Statements of the duty of the dealer, in one case to deal five cards to each player face down, and in the other to deal one card face down and one card face up to each player, are not contradictory, since they do not occur in the same game; but if we were to suppose them to be carried out concurrently by the same agent, and the result to be described in the language of the domain, this description would involve a contradiction. I shall call statements or statement sets having this sort of relation to each other incompatible.

There are also statement pairs which are not contradictions, and which do not necessarily lead to contradictions in the language of the domain, but which are nevertheless in conflict. I shall suggest a more complex example for this sort of conflict.

Among educational theories, one can distinguish between those which take certain significant characteristics of learners or content to be culturally neutral, or in some sense objective, and those which take such characteristics to be culture relative. The characteristics in question might be picked out by accounts of intelligence or of knowledge; the culture

in question might, in different cases, be identified by reference to ethnic group or social class. Thus, some more or less traditional theories have tended to identify a capacity of learners, measured as IQ, and a category of worthwhile learnings, as independent of cultural context; other, more or less radical theories, have regarded these as relative to cultural contexts[1]. Since the languages involved are primarily descriptive, they are correctable against phenomena in the domain, and are not to be separated by relating them to contradictory pairs of statements in the language of the domain, each one of the pair representing the expectation or prediction which might be made on the basis of one of the theories. Such a situation would lead only to the revision of one theory or the other. It may seem that, like card games, these theories would be incompatible: that when action was taken in normative contexts involving the theories, the descriptions of these actions in the domain would be contradictory, but this need not be so.

On a traditional elitist theory, the proposed response in a situation in which many learners are of low intelligence is the development of separate programs, directed to activities of a sort judged to be appropriate to these learners; on a cultural relativist theory, which sees a different rather than a low intelligence, the proposed response is the development of a

culturally appropriate program. The programs, and the learners directed to them, may well be the same in both cases. There may, of course, be differences; the point is that it is not necessary that there should be. It seems possible for theorists of both types, confronted by the same domain, to put their theories into effect concurrently without generating incompatible situations. One might have to hear their accounts of what is going on in order to discover that there is a significant difference between them. I shall call statements or statement sets of this sort dissonant.

Dissonance is the sort of relation which has figured in much relativist and anti-relativist debate. On the one hand, there is the somewhat positivist inclination to say that since on either theory we pick out the same things and take the same action, the alleged distinction between the theories must be illusory; on the other hand is the inclination to say that since the theories are different, it cannot be that we pick out the same things at all, or take the same action, in each case. Neither approach is congenial: one wants to say that there is a difference, but not the sort of total difference which would force us to say that the different theories were being applied in different worlds. On this account, what is the same in each case is the set of individuals identifiable in the domain, and used as

the basis of interpretive rules; what is different is the kind of individual identifiable in the interpreted game. Both the sameness and the difference are significant.

It should be noted that dissonance is a product of descriptive interpretation, and incompatibility of normative interpretation. Dissonance and incompatibility can hold between interpreted games without interfering with their relations of priority or material relations; therefore both can be internal to forms of life in the light of the above definition of this notion.

As some of the above comments strongly suggest, either incompatibility or dissonance, taking the form of ideological or more or less metaphysical disputes respectively, may provide occasion for investigations leading to relativistic conclusions. In the case of dissonance, where formal structures are reviseable under interpretation, it may be argued that in a sufficiently long time it could be expected that dissonant interpreted games would approximate to each other; the denial of this view would fit the pattern suggested in Part I for epistemic relativism. In the case of incompatibility, where this prospect of approximation is not open, opposition to the view that the claims of one interpreted game could be established would fit the pattern of what

I there called linguistic relativism.

The above comments indicate the areas in which various tensions are possible: contradiction only within an interpreted game, and incompatibility and dissonance only in more complex systems of games. It does not follow, of course, that these tensions are acceptable in the areas in which they can arise. Contradiction counts as an error or a defect, and demands correction. But, as the examples indicate, incompatibility is in at least some cases an entirely unproblematic state of affairs, and dissonance is at least a state of affairs which seems to be tolerable in a way that outright contradiction is not. The process of more clearly defining a use for 'way of acting' will require further examination of the question of the extent of acceptability of these forms of tension; and the sense in which a form of tension may be taken as acceptable must bear upon questions about relativism: questions about the extent to which such tensions are acceptable or eliminable are in effect questions about the status of relativist theses.

SECTION 18 In ways of acting, certain relations between games are necessary, and certain forms of internal stress are impossible.

I have said that I wish to use 'way of acting' to refer to a rational domain of discourse, that is, to a domain in which any question which is askable is, at least in principle, rationally answerable. The intended distinction between such a rational domain and the context in which it may be set is not, of course, that between rationality and irrationality. The suggestion is not that the wider context is irrational, but that the rules and contextual assumptions which, within the rational domain, provide the basis on which questions can be formed and resolved, cannot be presumed to hold in the wider context. In essence, a rational domain of this sort is like Carnap's notion of a linguistic framework. For Carnap, internal questions were those which could be asked and answered within a framework; questions which could not be answered, and could not properly be asked within the framework, were external questions, which made sense in a wider context in which the linguistic framework was set.[1] Similarly, I wish to identify as a way of acting a domain within which certain internal questions become possible, and within which certain internal stresses, potentially preventing answers, do not arise.

However, the notion of a rational domain provided by Carnap's linguistic frameworks is too thin for my purpose. I have argued before, in my discussion of Kekes, and shall argue further in Part III, that that which is basically assessable in terms of rationality is a course of action, and the attempt to create a "strong" sense of 'rational' applicable to systems of belief is a mistake. Consistent with this, and with my original introduction of 'way of acting' as involving interactive bodies of convention and belief, I seek to identify by the term domains which permit the identification of courses of action: in which notions of purpose expressed in the terms of normative interpretation, and of belief expressed in the terms of descriptive interpretation, interact. Such a notion of a way of acting is required for my general purpose; but it is also necessary that the notion of a rational domain be understood as involving such interaction, if a rational domain is to be independently intelligible: that is, if it is to be possible to make sense of there being such a system without supposing it to be a part of some more extensive context. The existence of a criterion of independent intelligibility marking off a smaller unit than a form of life would obviate the necessity of the language of a form of life to be an indissoluble whole; and this substantially affects relativist theses.

I have already sketched an argument to the

conclusion that descriptive and normative descriptions, and hence, of course, interpreted games which yield either kind of description, can be isolated from each other only as abstractions. That is, I have suggested that neither descriptively interpreted games nor normatively interpreted games are independently intelligible, and that a way of acting must include both. I shall argue this in more detail.

Let us consider a limiting case of a way of acting which is identical with a form of life, so that no wider context can be invoked. Suppose the way of acting to be the simplest molecular unit: one prior interpreted game accepted as given, and one further game interpreted in this domain. Suppose further the way of acting to involve only one form of interpretation: to be either purely descriptive or purely normative. We may now ask: is the final supposition acceptable?

To aid consideration, we will take as an example for the purely descriptive case two empirical theories, the subsequent related to the prior as explanation: say, a piece of commonsense realism including descriptions identifying visible objects such as lightning flashes, and a theory identifying them as electrical discharges. For the purely normative case, we will take chess as a particular form of

competition and victory, based in more general notions of competition and victory, and treating physical apparatus as a dispensable model. The former we might regard as a bit of a Popperian world 3; the latter as something to be encountered in a kind of Berkeleyan idealism.

The point of giving these samples their accustomed names, and by suggestion their accustomed contexts, is that it does seem that we can identify and make sense of the intended examples if we take them as abstractions from a wider context. We can, in the former case, abstract from the contexts of purpose in which explanation is carried on the explanations themselves; and in the latter case abstract from the context of physical players and equipment the sort of purposeful activity for which the one uses the other. It is part of this ability to abstract that we can suppose that the abstracted descriptive theories could be the same through variations in purpose, and the abstracted purposes could be the same although the descriptive setting of players and pieces were different. No doubt this is so; but what is required now is something stronger: that we suppose that these cases could be the same if their normative or descriptive contexts were not merely different, but absent. This supposition is harder to entertain.

Continuing, for the moment, to use the notion of abstraction as a crutch, we can see that the purely descriptive case seems more comfortable. The abstraction involved is more positive, in the sense that we can say what is to be included rather than just what is to be left out, than seems possible for the purely normative case. We are used to thinking of descriptive theories as coming in more or less discrete, competing or complementary chunks; and we have plenty of terms, like 'physics', 'chemistry','Newtonian mechanics' and 'quantum mechanics' to mark the borders. We are also accustomed to thinking of these expressions as referring to things which are what they are without regard to the interest, or lack of it, that we may have in them. Movements for social responsibility in science, if they do nothing else, at least emphasise the presumed separability of descriptive theories from normative concerns. We can, it seems, build fences which mark off reasonably clearly specified patches of ground.

But if marking off descriptively interpreted games is like building fences, marking off normatively interpreted games is more like focussing a camera. Such comparable expressions as we have for marked-off chunks, like 'aesthetics' or 'morals', don't seem to work as tidyly; but this is not the worst of it. Our normative concerns are not, even in principle, separable from our descriptive games. If we focus upon them

so finely that the descriptive context is eliminated, we lose the benefit of interpretation, being left with a formal structure. The notion that we ought to be kind to others, without any account of what others are to provide a basis for an account of what kindness to them might consist in, is not a working notion - it is a formal pattern which has yet to be turned into a working notion. An abstract game like chess offers the best chance we could expect of independent intelligibility; but without a descriptive account of the domain, its purposes remain formal purposes of the kind that Frege found so unsatisfying. To continue the metaphor: we need some depth of field in our camera; we can focus on normative concerns, but we need to see with some degree of clarity the descriptive context of these concerns if they are to make a picture rather than just a pattern.

Thus it seems that an abstracted purely normative game could not be treated as independently intelligible; hence our limiting case of a way of acting, which has no wider context, could not be purely normative. But now, is the purely descriptive case really different?

The territories marked off by the terms noted above, as separable scientific domains, are not so reliably distinct as they can, for some purposes, be made to appear; but again, this is not the worst of it.

There is the point about the kinds of reason for which we develop particular parts of particular theories: the fences can look odd, if we cannot see the purposes for which they were built. Theories about electricity cover more than lightning flashes. They cover phenomena which might not be recognised in a prior game until the subsequent game directed attention to them; and perhaps phenomena which could not be identified in the prior game at all. What do we suppose sets the limits of our purely descriptive way of acting? So long as it was regarded as an abstraction, we could note, then forget, that it arose from a certain kind of significance. Lightning flashes arrest the attention, and are the sort of thing we want to explain; but once we forgo this approach, can the game involve anything less than the whole story? And what would count as the whole story? Popper's world 3 could be incomplete, but this is because it is the notion of an abstraction. It is what would be left of our enquiries if we were taken away, and its limits are explained when our purposes in producing it are understood.

By contrast, Plato's world of forms is not, or at least is not intended as, an abstraction, but rather as something independently intelligible; and it is not at all clear what it would come to to say that it was incomplete, or that there could be an independently intelligible bit of it. A purely descriptive way of

acting identical with a form of life could not be thought of as an abstraction, and would have to lean more to Plato than to Popper. It would have to tell, in some sense, the whole story of the universe. I do not think this is an intelligible requirement: such an oddity could only be something like the universe mapped onto itself, which is not a useful notion. So we cannot allow a purely descriptive way of acting either.

It follows that a single interpreted game could not constitute a way of acting. The molecular unit mentioned above could, upon a certain condition, that is, that the interpreted games involved were not both the product of the same kind of interpretation. We can thus establish the minimum complexity of a way of acting. The other question affecting the usefulness of the notion concerns the maximum allowable complexity: whether there is anything permissible in a form of life which there could be reason to exclude from a way of acting. The kinds of tension discussed in the last section provide the basis for such a distinction.

I said of the notion of dissonance that it arose from descriptive interpretation, involved neither contradiction nor incompatibility, and led to disputes of a generally metaphysical sort. I cited an example from the field of educational dispute; I might as readily have cited, as examples of dissonant structures and the

disputes thereby occasioned, scientific and religious belief systems, and the quarrels of Kekes's protagonists Sinch and Rench. On this basis I would argue that religious and scientific belief structures are incommensurable but not incommunicable, and that they can arise within a form of life, but not within a way of acting.

I take it that two systems may be said to be incommensurable when no statement from one can be directly compared, for truth or falsity, with a statement from the other, and there is no independent system within which reasons can be offered for preferring one system to the other.

On my account, if science and religion are dissonant structures, they result from the interpretation of different formal games in a common domain. The interpretation is in each case descriptive, so that each formal game has, in the process of interpretation, been revised to achieve a fit with the domain. Statements issued within these fields will have in common those elements of meaning drawn from the domain, and appearing as common areas of reference; but will diverge sharply in the formal component of their identifications. Thus no scientific statement will be translatable without loss into, or intersubstitutable with, a religious statement,

though there will be statements, expressed in the terms of the domain, which will be recognisable in both structures, say, as providing the basis of interpretive rules. Thus Sinch and Rench have much language in common; but there is no statement in scientific terms which means what Rench means when he says "God is love"; and no statement in religious terms which means what Sinch means by "$e = mc^2$". There are no pairs of statements such that one comes from each structure, one is the translation of the other, and they can be tested together against the domain.

Similarly, since each has achieved effective interpretation, each has found ways to fit his gameterms to the domain. It is thus not possible to use the domain as an independent structure in which they can be compared. It may be suggested at this point that this is a bad example of dissonance, and that contradictions in the language of the domain arising from the conflict of science and religion are notorious. I do not think that this claim holds. Some apparent cases involve not the language of the domain, but a question-begging use of the language of one of the dissonant structures: claims that religious propositions ought to be dismissed as not scientifically testable fall into this class. Also, it should be remembered

that the claim concerning dissonant theories was that normative incompatibility did not necessarily follow from descriptive difference; it was not that the descriptive difference was such that normative incompatibility could not arise. This last claim would be quite untenable, of course, since normative incompatibility can arise from structures which have descriptive elements in common, as aspects of the debate over the issue of conservation show. The best cases for the objection would be those in which contradictions in the domain arose from diverse claims on such matters as the age and origin of the earth, or the manner and order in which it acquired its biological furnishings. However, scientific findings in these matters become, from a religious point of view, merely aspects of the domain which must be accomodated in the interpretation; generally, the upshot is to treat previous religious comment on such matters as involving some kind of mistake.

However, although on this account of religion and science as incommensurable it follows that Sinch and Rench cannot translate each other's claims into their own terms, it does not follow that they cannot come to understand each other's claims. I do not mean by this that they understand in any sense involving sympathy or argeement; the sense in which they can understand each

other can be precisely specified. By hypothesis they have in common the language of a domain. Each dissonant structure is created by the interpretation of a formal game in that domain. Any formal game is in principle equally intelligible to both Sinch and Rench, and the processes of interpretation are common to both their systems. They are distinguished therefore by nothing but the fact that, in expanding their systems, they chose different formal games. This difference does not imply that they differ in their ability to understand systems, even if it is taken as a difference in commitment. Thus it is possible for one to take the other through the process of interpretation which the one initially followed, so that, whatever one might wish in the end to say about possibilities of conversion or commitment, it is at least clear that the other could learn to peform every operation in the interpreted game to which he had been introduced. I would call structures incommunicable only when this sort of teaching was not in principle possible; and this could occur only where the structures in question were not materially related, or related by priority.

From the above it follows that dissonant structures can arise within, and only within, a form of life. Since the form of life including a given interpreted game includes all those games related to the

given game either materially or by priority, and since dissonant games are materially related, two games can be dissonant only if they are constituents of the same form of life. It might be noted in passing, as a corollary of this, that games can be incommunicable only if they are not constituents of the same form of life.

Now, the fact that dissonant structures are incommensurable entails that they cannot occur within a way of acting, if a way of acting is to be a unit of rational discourse, or a rational domain, in the sense previously discussed. To return to my earlier, educational example of alleged dissonance, it would be possible for questions to arise for which no rational method of resolution was possible. The question: "Is Fred in trouble because he's culturally displaced, or because he's of low IQ?" may be of this type. To explain Fred's low score by reference to cultural background, with supporting evidence of correlation between Fred's performance and that of others from his group, is still ambiguous. It may be the claim that intelligence is culture-relative, or that Fred comes from a group with low intelligence. One can choose which claim is intended, but not produce evidence which shows grounds for preferring one. To find a test on which Fred's group scores better than others, and call that a test of intelligence relative to the culture of

Fred's group, solves the problem only if the test is generally accepted under that description, which it need not be. To fail to find a test returning such results would prove the other view only if it were accepted that Fred's culture was well enough understood to permit the design of an appropriate test, which it need not be. Generally, although we can describe the situations in which something might be accepted as resolving such questions, and thus in which one of the apparently dissonant structures was abandoned, we can also see that it is not necessary that such situations arise: there is always a way in which the challenged structure can be maintained. Hence a rational domain cannot include dissonant structures; and the discovery of dissonance is at the same time the distinguishing between rational domains, or, in the terms I have used, between ways of acting.

There is thus a clear point in continuing to use 'way of acting' in addition to the other technical terms I have suggested: the point that none of the others picks out domains as rational. It has been possible to make this point by reference to dissonance alone; but it is also important to see to what extent a way of acting can accomodate incompatibility.

I used card games as an example in explaining

the notion of incompatibility I wished to use. If ways of acting can be complex in the sense in which card playing is complex, then the example shows that incompatibility is possible within a way of acting. I think that ways of acting can be complex in this sense; and indeed that something like incompatibility is necessary. The rules which go to make up interpreted games, where they yield identifications of actions, must be incompatible in the sense explained above. This is simply to say that one action, under its identification in the terms of the domain, must exclude alternatives: one cannot simultaneously deal and play out a hand of bridge, for instance. Even if the game identified only one kind of action, it would be necessary, if that action were to be anything determinate, that it was incompatible with some possible kind of action. This is merely the point that actions of different kinds must differ in some respect. Thus, given the earlier claim that ways of acting must involve a normative aspect, it is clear that incompatibility between single rules is a perfectly ordinary and unproblematic affair, required in an interpreted game as a condition of significant identification, and necessary within a way of acting.

Incompatibility between interpreted games - as, say, between different card games - is not in any obvious way more problematic; and if such incompatible

systems are in some cases to count as parts of a way of acting, no significant tensions within the larger system seem likely to ensue. Indeed, one might say: incompatibilities of these sorts within a way of acting are necessary conditions of the way of acting's providing varieties of action and the possibility of choice. One may go on to wonder whether the inclusion within a way of acting of other incompatible systems, such as systems of etiquette or of morals, could also be seen as providing the possibility of choice, though the notion of choice involved in the case of moral systems might seem at first sight more significant than that between card games.

Certainly, it does not seem to be the same notion of choice in all cases. The choice of a card game, within the context of card playing as a way of acting, is one for which, at least in principle, reasons can be given. It is not hard to justify the choice of patience over poker, for instance, in a context in which the characteristics of both games and the availability of only one player can be established. Even in cases which we would normally say were of mere inclination, it seems that reasons for choice might be given in terms of characteristics of the player which were reasons for him - a facility for bluffing which he enjoyed exercising, perhaps.

In the other cases, things are more complex.

The attempt to choose a code of etiquette may be only part of the more problematic matter of choosing a social context: to change one's patterns of behaviour within the same context may be identifiable in that context only as a defect in manners, not as a change. When one turns to the case of a moral system, the question whether there are genuinely alternative moral systems is a matter of established dispute, so the question whether one could have any choice in this matter, or in what sense one might have a choice, is more difficult. At first sight, one is inclined to say that a way of acting could include diverse card games, but not diverse moral systems. These differing cases should be further examined.

It will help to consider three sorts of case rather than two. I shall consider, as distinguishable cases, a card game, in which certain distributions of cards counts as a victory, a code of etiquette, in which a certain way of eating counts as dining, and a moral code, in which certain kinds of action count as right action. Since these are all normative interpretations, the rules under which identifications are made can be broken: an action falling under a rule can involve a breach of the rule. Where an action taken as falling under a particular rule was in breach of that rule, it would commonly be called cheating in playing cards,

discourtesy in dining, and immorality in the moral case. In each case, the opprobrious term indicates that the action in question fails to qualify under a certain description, but assumes that the action is properly considered as falling within the scope of the particular rule-set in question. One way to bring out the difference between the cases is to consider what we might find it proper to say if that assumption were incorrect.

In assuming that a particular action fell within the scope of a particular rule-set, we might err in two ways. It might be the case that the action fell within the scope of a rule-set of the same sort, but not that set in particular; or it might be that the action did not fall under any rule-set of that sort at all. At least, while we look at the card case, these possibilities seem clear enough.

It is quite clear, when we consider a particular card game, that there is a variety of such games, and hence other rule-sets, or in my terms interpreted games, of the same sort. If asked what constitutes a sort in this matter, we will be able immediately to point to the common interpretive base and the common notion of victory - or at least the very similar notions of victory - on the basis of which this collection of

interpreted games can be formed. It would certainly be possible that an action which we had taken as falling in the scope of one game properly fell in another; and, since the scope of each game is quite sharply limited, and a considerable range of actions falls outside any game of this sort, it is also possible, though perhaps less likely, that we had mistakenly taken as falling within the scope of a game an action which was not part of any game of the sort: tidying the cards, or making a pattern with them, perhaps. In any of these cases, the mistake in judgment is readily detected. The restricted scope and relatively precise rules make it a fairly simple matter to test any action by checking its context against the requirements of the rule-sets. Thus the inclusion of incompatible interpreted games of this sort in a way of acting would give a context in which there was a variety of games of the same sort, and in which questions concerning which game was going on, and which game should be played, were answerable, and the notion of a rational choice between games was a sensible notion. This might be called intentional incompatibility; and its occurrence in a way of acting is quite consistent with its being a rational domain.

In the other cases, this is less clear. We do recognise alternative codes of manners, and note that conventionally correct behaviour varies from time to

time and from place to place. Hence we can recognise a sort of rule-set here, and thus the possibility that a piece of behaviour has been assigned to the wrong rule-set. Further, we can reasonably claim that codes of manners are sufficiently limited in scope for there to be kinds of behaviour not properly assigned to any of them, so that we could also err in taking an action to be subject to such a code when it was not. But although the mistake of assigning behaviour to the wrong code is possible, it is not likely. Codes of manners take in a community in a way in which games do not: they are part of the character of a community, and part of the pattern of mutual expectation and belief which make up the communication system of the community. To conform to the manners of a community is less a matter of second-order politeness than a necessity of communication. Hence the likelihood of a mistake is less; and it is not quite so clear that a declaration of allegiance to an alternative code serves to excuse discourtesy.

Also, the question what areas of behaviour are outside the scope of codes of manners is difficult. To some extent, the answer varies with the code: whereas we could say what was not a card game readily, it may be that only from within a code of manners can we say what is not a matter of manners, and then only for that code. Because of the uncertain scope, and the part played by

such codes in forming the identity of a community, it seems that in the context in which we can recognise a variety, we cannot develop the same notion of rational choice. One might make a rational choice of the community in which one was to live, and thereby commit one's self to a code of manners; but within a community, the question what codes there were or which should be adopted would be odd questions, and the notion of a rational choice between codes is odd, even if we suppose alternative codes to be known. I shall call incompatibilities between codes of this sort contingent incompatibilities; on this account they allow a notion of variety, but not of rational choice. Perhaps the most interesting example in the class would be natural languages: the recognition of variety does not allow a rational choice of which language to use in a particular transaction, since success in the transaction is typically contingent on the use of the correct language.

The final case, of morals, differs quite clearly from the others in that it has been strongly argued that the notion that there are alternatives, or that 'moral code' names a sort with more than one member, is itself an intellectual or a moral mistake. The scope of a moral code has been argued to be unlimited, in the way implied by the requirement of universalizability. It follows from such a position that there could be no variety in such codes, and consequently no notion of

rational choice applicable to them. Now, I do not wish at the moment to claim that such anti-relativist positions are right or wrong. I wish only to make this point: that the issue is still debated is sufficient reason to say that questions that arise concerning the variety of and choice between moral systems are not rationally resolvable in the required sense, or at least, have not yet been shown to be. They cannot therefore be included in a rational domain. So, if alternative moral codes are possible, they are not possible in a way of acting if that is to be a rational domain; if they are not possible, then they are not possible in a way of acting, and could appear to exist there only through some mistake. Such incompatibility could be called necessary incompatibility; whether the necessity arises from there being only one coherent moral system, or from its being characteristic of moral codes that they claim unlimited scope does not affect the present point.

This discussion places the limit of allowable complexity in a way of acting somewhere between the admission of a variety of card games and the admission of a variety of codes of manners, the choice of this limit being defended by reference to the preservation of the rationality of the domain. This is a vague borderline, and further clarification of the notion of a way of acting would require the discussion of further

examples. However, for my present purpose it suffices. Indeed, even the distinctions I have tried to mark are open to debate. It might be said that codes of manners are far more like card games than my comments, and the reference to natural languages, would suggest; and that moral codes are closer to conventional codes than I allow; and such arguments would change the limits of a way of acting, even when they are marked so coarsely as they are above. I do not think that this matters. My general thesis is not affected if the limits of a way of acting are moved one way or the other, so long as it is agreed that there is point to marking off a rational domain which may be less extensive than a form of life. I would lose this point only if it were held that no choices were rationally resolvable, or that all were. I think that the distinctions I have drawn are clear enough to stand against both these positions; it is of relatively small moment then just where the lines are drawn.

Since it becomes now a tautology that situations in which relativistic theses might hold cannot occur within a way of acting, we must turn to a form of life, in which contingently and necessarily incompatible, and dissonant interpreted games, must be held to occur, since all such relations presuppose material relations, and materially related games share a form of life.

CHAPTER 7 A form of life is a system of ways of acting.

SECTION 19 The notion of a form of life can admit the necessity of a foundational way of acting; but in accepting the existence of a foundation we limit the possibility of specifying it.

I have proposed that 'form of life' be used to refer to the most complex unit this account allows, consisting of all interpreted games related materially or by priority. Such a statement is incomplete: if relativist and coherence theories are to be intelligible, one needs rather to say that a form of life consists of all games related to a given game, so that the possibility of alternatives is not ruled out before we begin. An answer to the question what might count as a given game can be drawn from the following discussion of the problem of a foundational game.

On my account, an expression has sense in a way of acting, or as an abstraction from a way of acting; and the sense that it has is the product of a formal game, and an interpretive base established in a prior way of acting. Thus an expression has sense only if there is a way of acting prior to that in which it has sense; and since this must also be true of expressions in that prior way of acting, there is here the start of an infinite regress.

The regress appears vicious: it cannot terminate in a way of acting of the kind so far described, since expressions in this way of acting would not have sense, it would not allow any expression to have sense. Hence, an account of a basic way of acting, or some basic structure capable of supporting a terminal way of acting, is required.

The problem of foundation or basis is common enough, and the solution offered here has antecedents in and relationships to solutions offered in such areas as theory of knowledge; but there are two points to be noticed about the occurrence of the problem in this context. The first is that a basic structure is formally necessary. Whether there is a problem of foundation for meaning which is perfectly general, in the sense that it would arise no matter what account of language and the world we adopted, I do not attempt to say; though one might argue, in a more or less Wittgensteinian mood, not that there was no such problem, but that attempts to state it were themselves senseless. If, however, we talk about the way expressions have sense in the way I do here, the problem of foundation is unavoidable. The second point is that the structure under discussion is a structure of meaning rather than a structure of knowledge. No doubt the two are related; but the account required here is not of something we can

regard as certain, and hence as a sound basis for further development of knowledge, but of something we can regard as primitive, and hence as a sound basis for conceptual elaboration.

I shall be interested in two questions: first, what account can be given of a basic way of acting or a basis for ways of acting; and second, whether the requirements for a basis could be satisfied in more than one way - whether there is only one conceivable basis upon which a structure of meaning could be raised. Although the questions seem to fall logically into the order in which I have placed them, I think that there is advantage in taking the second question first, and I shall approach it through a consideration of the somewhat inconclusive answer suggested by Strawson's discussion in *Individuals*.

He offers an account of a basic conceptual structure which includes the concepts of material bodies and persons, regarding this as a piece of descriptive metaphysics, establishing the nature of our conceptual scheme as it is. He discusses alternative accounts of our conceptual scheme as it is, particularly with regard to persons, and offers reasons for rejecting these alternatives. He discusses an alternative conceptual scheme - of an auditory world - by way of contrast; and perhaps another, if his remarks on quasi-Leibnizian

monadology can be taken in this way. It is less clear what he wishes to say about alternative schemes, largely because his interest is in our scheme; and the alternative sound world is offered as a model, in the process of constructing which we may see more clearly aspects of the scheme we actually have. The question whether such alternative schemes are in any serious sense possible is of no interest to him; and at the point which he takes to be crucial - the issues raised by solipsism - he abandons the model. However, his attempt to construct it can be made to contribute to an understanding of the present issue.

If it is accepted, as I think it clearly should be, that many points about ways of acting can be made in terms of the systems of concepts through which the way of acting is constituted and understood, then there are several respects in which Strawson's story is at least compatible with the one I have attempted. The priority he assigns to the concepts of material bodies and of persons is not priority in the sense, defined on interpretation, which I have used; but it has similarities. For Strawson, a class of concepts C is prior to, or more basic than, a class C' when we can identify some object(s) using a member of C' only if we can identify some object(s) using members of C, and C is basic or absolutely prior if there is no class C" such

that C" stands to C as C stands to C'. The concepts of the interpretive base of an interpreted game would stand to the concepts of the formal game interpreted as that interpreted game on that base as relatively basic in this sense; and I have suggested that some set of concepts must be absolutely prior. One can also note that Strawson's stress on the importance of the identification and reidentification of particulars is similar to mine; and the significance he attaches to the concept of a person as an individual to which both physical and mental predicates are attributable has point in common with my account of ways of acting as involving both descriptive and normative interpretation. This shows quite clearly when, in considering a problem about the possibility of ascribing what he calls P-predicates - predicates attributing or entailing the attribution of states of consciousness - he advises concentration on a group of predicates "... which involve doing something, which clearly imply intention or a state of mind or at least consciousness in general, and which indicate a characteristic pattern, or range of patterns, of bodily movement, while not indicating at all precisely any very definite sensation or experience".[1] Of the examples he gives, 'playing ball' is perhaps closest to the list of verbs which I earlier offered as indicating possible ways of acting.

Given this similarity, it ought to be possible

to make sense of Strawson's procedures in the terms I have used, and to discuss his conclusions on alternative basic conceptual schemes, insofar as he reaches any; and this is possible. Perhaps the most important difference to be dealt with is that, although he is interested in a basic system of concepts, he is not interested in a hierarchical structure raised on this base. The notion of identifiability-dependence which he discusses[2], which I above compare to the notion of priority which I have used, is capable of generating such a structure, but he does not use it for that purpose. His classes of predicates[3] could not be identified with the sets of predicates established in interpreted games or in ways of acting. They might, I think, be identified with sets of all predicates at the same logical level - i.e., at the same number of removes from the base - without regard to relations of contradiction, incompatibility and dissonance which might hold between them, or between the statements in which they have a place. I do not think this is a crucial difference; one need only say that, having a different purpose in mind, he has been content with an account which ignores details of the hierarchical structure for which its general principles allow.

It is part of the story that I shall later offer that we might construct, within a given context, a model of an alternative context, that the language

associated with this model could not be used in the given context, that the languages would be incommensurable, and that this process might illuminate the structure of the given language. This might be offered as a translation into my terms of some of Strawson's remarks. He discusses the sound world as a model, for the sake of the light it casts on our own system. He says: ".. we need no more claim to be supposing real possibilities than one who, in stricter spheres of reasoning, supposes something self-contradictory and argues validly from it".[4] The reference to self-contradiction is probably highly significant, given his general view that sceptical arguments, and perhaps some revisionary metaphysics, involve the repudiation of positions which they presuppose. Of course, the frustration of a presupposition, on his account of presupposition, would be something other than strict contradiction; the something other might be caught by the notion of incommensurability.

Already there is some explanation for the difficulty of eliciting from Strawson's discussion an answer to the question whether alternative bases are possible. Since he, and indeed anyone else, works within a system which has a particular base, he can only give accounts, or construct models, of alternatives. Consideration of whether we might use such alternatives, or whether anyone does, would no doubt count as "... speculation about what would really happen in

certain remote contingencies", and falls outside his concern, which is with our own scheme. There is an impression of implicit countenancing of alternatives here, conveyed by the use of 'contingencies', and by recurring references to our scheme, or the scheme in terms of which we think about particulars. One feels that he has chosen to examine our scheme, but might have done otherwise, had the difficult task seemed worthwhile. After some attempt to produce, in the sound world, an analogue of the unified four-dimensional reference frame which he takes to be of central significance in our scheme, he says:

> Evidently, in making such suppositions, one would be trying to produce as close an analogy as possible of the actual human condition. But the fantasy, besides being tedious, would be difficult to elaborate. For it is too little clear exactly what general features we should try to reproduce, and why. It might be better at this point to abandon the auditory world, and face the issues .. in closer connexion with the ordinary world.[5]

I think there is some confusion here. Part of it arises from his apparent view that he picks out a particular conceptual scheme by his use of first person plural pronouns - that 'we' and 'our' serve the demonstrative function that he discusses elsewhere - and I shall return to this point; but more immediate questions arise from his comment.

Why should we try to produce as close an

analogy as possible of "the actual human condition"? Indeed, what exactly is the force of the suggestion that one might do so? The sound world was introduced in an attempt to answer the question whether there could exist a conceptual scheme which was like ours in that it provided for a system of objective and identifiable particulars, but was unlike ours in that material bodies were not the basic particulars of the system; and this was glossed as a concern with whether we could make the diea of such a scheme intelligible to ourselves. Perhaps the underlying line of thought is that there are close relations between the actual human condition and the conceptual scheme which humans - or humans to whom 'we' and 'our' are applicable, if this is a smaller group - actually have; between this scheme and any alternative scheme such humans could make intelligible to themselves; and between ann such scheme and the world in which it might arise. Hence, we might say, in the absence of sufficiently close analogy we will not get an intelligible idea; and this does lead to Strawson's concern about what would count as sufficiency. The question then seems to be: what would be a list, sufficient for the purpose at hand, of general features of the human condition?

I think that this way of putting the matter is misleading, and the difficulty is hidden by the use of 'the human condition'. In ordinary use, it would draw

our attention to such general features as the socio-economic conditions in which humans live, or their relations to the super-human. This would cause no problems; but of course these are not the kind of general feature meant. If, as is surely the case, it is rather the conceptual features of the human condition that matter, we might conclude that being material is a central feature of the human condition: that our basic scheme is of concepts of material objects is hardly surprising, since humans <u>are</u> material objects, albeit of a special sort. But this is just the aspect in which disanalogy is intentional; and where can we go from there?

When we look again at the analogies on which Strawson insists, we find that they are of a certain sort. The alternative scheme has to provide objective and identifiable particulars, which he argues involves the possibility of reidentification and a way of cutting out solipsism. The characteristics of material objects which count, and which provide the basis of the analogy, are those characteristics which support and reflect the four-dimensional reference frame: their being relatively enduring three-dimensional objects, their being in only one place at one time, more than one of them not being in the same place at the same time, and so on. That is, they are just those characteristics which Russell thought so conveniently systematic as to

give rise to suspicion, and which he and others have attributed to a process of logical construction, rather than to their being characteristics of objects of acquaintance. I would wish to say that Strawson has sorted out the formal aspects of the conceptual scheme. If one can put aside for the moment the difficulties related to the scheme's being basic, I would say that his model-building enterprise could be represented as his having analysed out of an interpreted game the constituting formal game, and set out to see what would happen if that formal game were to be interpreted on a different base.

On this account, one can see what features are to be reproduced, although 'formal' rather than the misleading 'general' would be the expression to use. And 'formal', as well as indicating the kind of feature one wants, also helps with the question why. We have a choice of interpreting an old formal game on a new base, or interpreting a new formal game on an old base. On the one hand we are wondering whether our formal structure would survive a change in what might be called the taken-as-given base; on the other hand we are wondering if the taken-as-given base might be understood or elaborated in a new way. The first is a speculative enterprise, unlikely to have practical impact except in the science-fiction context of an attempt to establish communication with an entirely alien entity, and most likely to lead to

fuller understanding of the formal game in question. The second may be only speculative; but it could be a practical enterprise, intended to effect some change in our ways of acting. There is no doubt of what Strawson is doing, and hence of why it is the formal features of our interpreted games and ways of acting that he wishes to reproduce.

The other part of the confusion is in the notion of an alternative conceptual scheme itself. It involves questions like: in what way would contingencies affect basic schemes? and what would make an alternative scheme a real possibility? and how many actual or possible schemes might we suppose there to be? And these in turn raise the question what is going on when Strawson purports to pick out a particular scheme for attention, and just how that scheme is picked out.

On the face of it, there is no problem: it is "our" scheme, the one "we" actually have. But if the scheme is picked out by its relation to us, what does 'us' pick out? And is there any them? Now, Strawson criticises those who use first person singular possessive pronouns in a more or less Cartesian version of mind/body dualism. He says that the attempt to speak of the experiences related to other bodies in the way that my experiences are related to this body collapses because 'my' has no real place in the statement: on the account

intended, all experiences are associated with "this" body.[6] Would an analogous argument tell against his talk of "our conceptual scheme"? Are 'our' and 'we' vacuous in such talk; and if this is so, is it important?

Taking a criterion of non-vacuity to be that we can distinguish between our conceptual scheme and theirs, the terms will be non-vacuous only if an enterprise like Strawson's modelling could be successful, and if the model is seen as a real possibility in the sense that it is seen as the conceptual scheme of a possible them. In dismissing the need to consider alternatives as real possibilities, Strawson says in effect that it does not matter if his first person pronouns are vacuous.

But now, suppose that we do take the auditory scheme seriously, and regard it as the scheme of a possible them: let us call them audions. Suppose further the remote contingency that we find that there really are audions. It is still not the case that we distinguish between the auditory and the ordinary scheme on the basis of a prior distinction between us and audions; rather, we can distinguish between us and audions only because we can distinguish between the schemes. Nothing in the ordinary scheme allows peculiar auditory phenomena to count as the discover of audions.

In the absence of an auditory scheme seen as a serious possibility, auditory phenomena will be handled by our scheme, with any peculiarities covered by notions of illusion or error. One might consider, by way of analogy, the resources conceptual structures offer for not discovering the existence of ghosts. The analogy is feeble, of course; but in the nature of the case, any analogy which does not involve an alternative scheme considered as a serious possibility must be feeble.

In the same way that we cannot first discover audions, then discover their conceptual scheme, since we need the conceptual scheme in order to identify anything as an audion, so we do not identify ourselves, then note that we have a particular conceptual scheme. 'We' has a use once the scheme is available, to refer to a category of particulars identifiable within the scheme. Thus 'we' does distinguish something - Strawson's remarks suggest humans - from other things, so references to "our" conceptual scheme do not appear vacuous; but the appearance is misleading. We are distinguished from dolphins, if dolphins in fact have a conceptual scheme, and from Martians, if in fact there are any; but not by our conceptual scheme. Strawson's line of argument seems to suggest that we could distinguish in that way only if we could make their schemes intelligible to ourselves, that we could do this only to the extent that they had pretty extensive features in common with ours,

and that to the extent that they did, dolphins and Martians would be seen as sharing the human condition: as being, in effect, more of us. Generally, in the absence of alternative schemes considered as sericus possibilities, we could recognise a non-human them as having a conceptual scheme only if we could recognise it as having our - i.e., the - conceptual scheme, at least at its basic level.

From one point of view this does not seem important. Strawson was not investigating non-human intelligence; he was discussing the conceptual scheme: the one within which he worked, and within which, as a matter of necessity, any reader who understood him also worked. And that is a significant enough "we" for his purpose. But from another point of view, it is important. Exotic devices like Martians and audions allow us to make points which often have a more prosaic application. Thus Quine, discussing indeterminacy of reference in the context of translation tasks for the language of a conveniently different tribe, remarks that we can reproduce the effect at home: we can attribute principles or ontological categories to speakers of our own language, and interpret their speech dispositions in the light of that attribution.[7] Similarly, we can attribute the ways of acting to which we are committed to others, at home or abroad, and interpret their behaviour accordingly. Once

we have done so, we have by-passed the question whether the attribution was correct or not. We may not be able to make all their behaviour fit the pattern, but this does not matter so long as we have resources to diagnose or explain away divergences. Probably our conceptual scheme will be rich enough to provide these resources; but if it is not, we can add them: these are the situations which call for new theories in psychology or sociology or anthropology in order to make features of the behaviour of others intelligible in terms of the scheme we have to hand. It is, I think, this capacity to see divergent groups as being, really, the same as us, which leads to a concern with a possibility of error commonly expressed in relativist terms. The capacity, and hence the concern, is greater the more rich and efficient our conceptual scheme is; and the concern is not diminished by the "discovery" that there are no groups that are _really_ divergent, for this is just what is to be expected if the concern is well-founded. The real difficulty raised by such a concern is this: if the scurce of concern is that our basic conceptual scheme has the capacity to over-ride possible alternatives, and it is a feature of this situation that we cannot tell, in a given case, whether anything has been over-ridden or not, then the concern is perverse in criticizing what cannot be avoided, and futile in pleading a cause which cannot be identified.

In sum, the Strawsonian position on the possibility of alternative basic conceptual schemes seems to be that they are possible, but that their construction would be problematic, tedious and pointless. In the reconstruction of his position which arises from the attempt to restate it in my terms, the possibility of alternative bases is in effect the possibility of interpreting in a different domain the formal game which in its given interpretation constitutes the basic conceptual scheme: or, to put the point more carefully, the formal game which, under interpretation, goes to constitute the basic way of acting in which expressions formally defined on that formal game have a use. The problem of alternatives is that they can be related only formally to any way of acting that we can understand. That is, the problem is not, as Strawson suggests, that we find it hard to tell which bits of the human condition we are to transfer; it is rather that we can have no notion of what we are transferring the bits to. Strawson's sound world is perhaps as close as one could get to performing the impossible feat of identifying a familiar domain with no formal debt to actual ways of acting.

Strawson's difficulty, or at least the difficulty which I attribute to him, indicates what must be the nature of the solution to the problem of foundation

which I set out to discuss in this section. Given that a conceptual scheme or a way of acting is basic, it cannot be explained by reference to any other schemes or ways: it must be self-explanatory, or self-evident,or, in another manner of speaking, it must show itself, or show what cannot be said about it. And given further that one abstracts from the formal structure of this basic unit, as I suggest Strawson does implicitly, and as I wish to do explicitly, what one has left does not even have formal features to show. Nevertheless, I think it is clearly possible to draw a formal/material distinction here. I do not think there is any special difficulty in talking about the formal component. A formal game is a formal game, regardless of the level at which it is interpreted, and there is no difficulty in talking about formal games in this case which is not perfectly general.The formal features of material objects, most clearly those which relate to a four-dimensional reference frame, have been discussed by others besides Strawson; and the fact that he wishes to make this the formal structure of a basic scheme does not mean that he must differ on its nature from those, such as Russell, who did not. So the notion of a formal game remains as clearly applicable in the case of a basic way of acting as it was in any other case. For the material component, I shall employ the term 'the given'. Having introduced the term, I will offer some account of its use, in order to stop the run of several hares.

In identifying something as given, it has sometimes been intended to identify that something as a source of certainty, or of incorrigible statements. This is not my intention. In line with the sense in which I have said that an interpretive base is taken as given in the relative interpreted game, the material component of a basic game must be taken as given, or taken for granted, or assumed. To say that it is given is not to say that those who so take it have chosen so to take the right thing. It is just to say that what is so taken in this case is not part of a prior way of acting, and consequently is not accessible to certain kinds of judgment: in particular, judgments concerning its corrigibility or certainty. Nor can it be my intention to suggest that whatever is identified as given must have that status in every form of life, since such an identification, being relative to the basic way of acting of a particular form, cannot be supposed to have any force in any possible alternative. Nor is it the case that the given cannot change. To be converted from the sort of commonsense realism that Strawson espouses - 'to be converted to' being distinct from 'to become acquainted with' - would on this account be to come to take something different for granted, as part of a revision of one's view of the relations of priority within one's form of life. The apparent constancy of the given is due to the fact that there is nothing to be said about it, except in a way of acting.

We cannot suppose that questions about the given are to be answered by isolating it and examining it, for the process of isolation must be that of abstracting from the formal context which which allows the given to be characterised. One can usefully think of the given only as an abstracted component of a basic way of acting, and either accept accounts of it as presupposing the formal structure, or treat it as a "something-I-know-not-what". Questions about the given must therefore be dismissed as senseless, or translated into questions about the formal context.

The last point provides the principle of termination for the regress through priority. To say that the question "How is the interpretive base of this game constituted?" must in some case be treated either as nonsense or as a question about the structure of the relevant formal game is to say that the regress through priority has a foundation; to identify the game in question is to identify the foundation. In these terms, Strawson might be said to offer an account of a foundational way of acting involving a descriptively interpreted game yielding the possibility of identifying physical objects, and a normatively interpreted game yielding the possibility of identifying actions.
Faced by challenges to the foundational status of this system, arising from scientific theory and phenomenalism,

he responds by invoking a notion of priority which he uses in an attempt to show that these challenges arise from a misunderstanding about formal relations. Although I shall suggest in the next section that his arguments point to certain weaknesses in the position he adopts, it is clearly consistent with my redescription of his position that he responds as he does. However, the attempt to present an alternative system by first offering a description of the given must, on my account, be a mistake. I shall suggest, in Part III, that we may be able to do a little more about the notion of alternative forms of life than merely argue their formal possibility; but to begin with a given articulated in terms of concepts drawn from the context with which we want to make a contrast must be the wrong way to go about it.

Thus I wish to say that the regress which I introduced at the beginning of this section is not vicious, that it can be terminated by a notion of the given, that this notion is of a limit of analysis rather than of a special sort of identification, and that the claim that the material aspect of some context is given is equivalent to the claim that that context is prior to all others. On this basis, it is possible to consider further questions about a comprehensible form of life, and the possibility of alternatives.

SECTION 20 Intelligible disputes must occur within a form of life, in which they may concern priority; but there is a formal possibility of alternative bases, and hence of alternative forms of life.

The general difficulty created by the attempt to ask questions or make statements in which characteristics of a basic scheme, or of the system in which it is set, are explained as characteristics of various components of the basic scheme, appears in many forms. Thus Quinton[1] associates the quest for foundations of knowledge which I have disavowed with the sort of procedure I have adopted, and makes general comments about the way any such procedure is likely to turn out. He considers a notion of "immanent metaphysics" in which" the relation of dependence can be conceived as the converse of epistemological priority", and notes that phenomenalists can appeal to this to reverse the dependence-relation between impressions and material things. He says:

> An immanent metaphysics, so conceived, is thus a development of a reductionist thory of knowledge. Such a theory of knowledge arranges the various broad categories of things in a hierarchy ordered in respect of epistemological priority. At the base of the hierarchy are the truly concrete, self-subsistent entities, which can be said and known to exist without reference to things of any other category ... The process of arriving at such a system must begin with a <u>prima facie</u> set of categories, each composed of things whose existence is established or refuted in the same way ... The outcome of a successfully

> completed undertaking of this kind will be a hierarchy of logically related categories. It will constitute a general picture of the world, particularly in picking out some category or categories of things as basic.[2]

Things in the basic category, he tells us, "logically require nothing but themselves in order to exist"; things in derived categories will be sorted out as dependent or fictitious and illusory, depending on whether they can be "defined or supported, at some remove or other, by basic entities". He then suggests that ".. an interpretation can be found for many of the surprising-looking assertions of metaphysics", the surprising-looking ones being, apparently, those which conflict with commonsense or scientific understanding. The explanation is that they are ".. very general statements about the logical relations between concepts (and, in the case of basic ones, between concepts and the world)".[3]

Now, certainly a form of life, on my account, exhibits a hierarchical structure based on a notion of priority with features in common with Quinton's notion of epistemological priority; but if on this ground it is regarded as a development of a reductionist theory of knowledge, the result is disappointing to say the least. At the limit of analysis we have a formal component which is, in the sense earlier discussed in relation to formal games,arbitrary; and a given material component. Perhaps what I have called "given" corresponds to what

Quinton calls "the world"; but if we accept this, and even if we accept talk of logical relations between concepts and the world, nothing follows, for there is nothing to be said about the given once it is abstracted from the basic structure.

And there are other problems. If things in the basic category are elements of the formal and material components of a basic way of acting, there is little point in the suggestion that they "logically require nothing but themselves in order to exist". One might be a Platonist about formal games, and of the given one might as well say this as not; but no interesting point is made in this way. Perhaps this is a perverse way of understanding 'things'. Perhaps one should think rather of the kinds of particular which a basic way of acting allows to be identified. However, this does not end the problem. Part of the function of 'logically' is no doubt to make clear that he is not saying that material objects - a basic category for both Strawson and Quinton - are in some matter of fact sense ultimate objects, a view which would be contrary to science and, for most kinds of material object, contrary to common sense as well. But then, what is the positive force of 'logically'? Well, it has to do with the notion of epistemic priority: the point is that nothing else has to be said or known to exist in order that material objects should be said or known to

exist. But this transmutation of 'epistemologically' into 'logically' hides the rather Berkeleyan point that for objects to have this status does require something else to exist. It requires the existence of knowing subjects of the right sort; and this, I think, is a logical rather than an epistemological requirement.

Nor is it clear that, on my account, things in derived categories are to be sorted out in quite the way he suggests. Of course, possible ways of acting may be sorted out as acceptable or unacceptable. The rejection of, say, religious ways of acting as "fictitious and illusory" is certainly possible, and is as much a part of a form of life as of an epistemology. But the notion of ways of acting does not allow it to be done in the way that Quinton suggests. The notion that entities of derived sorts are defined at some remove or other by basic entities can be expressed in the terms I have used. To the extent that the process of interpretation involves assigning referents, identified in the terms of the domain, to the expressions of a formal game, and to the extent that interpreted games form an orderly hierarchy on a basic interpreted game, one might say, at least of some kinds of entity, that they were in this sense defined, at some remove or other, on basic entities. But they are not defined by basic entities: the major load is borne by the formal game which constitutes the game in which they are introduced; and there is no

reason to suppose that the domain is selective: incompatible and dissonant ways of acting may involve reference to entities which are defined in this sense on the same domain, without the domain offering any basis of preference.

Even less does a domain provide preferential support for particular ways of acting. True-or-false statements issued in a way of acting may, together with some selection of constitutive rules and rules of interpretation for the way of acting, entail some statement, or some member of a set of statements, in the language of the domain, and in this way be supported by them. The relation involved is similar to that between Ryle's process words and achievement words - indeed, some of his cases are of just this sort.[4] Having said that Fred won a race, and having, as suppressed premisses, a number of understandings of the business of racing, the kind of race this was, and the conventions in operation, we are in a position to deduce that Fred broke a tape, and other things as well: that he must have been running in competition with at least one other runner, that he must not have started before the gun, and so on. All these statements, and a number of others, are expressible in a domain in which racing, as a way of acting, might be introduced. The truth or falsity of such statements is relevant to the truth or falsity of 'Fred won the race'. But what makes them relevant is the

constitution of racing as a way of acting.

Ryle seems rather to suggest otherwise. On his account, winning is an achievement, and we must not mix this up with a process, and we must not neglect the many factors, including intention, which constitute this achievement; but the impression is that we might, with sufficient care, say that he won without crossing into a new language zone. But no such list of things that Fred did, and ways in which he did them, will be equivalent to the statement that he wom the race, so long as we are restricted to the language of the domain. The point is easy to miss because it is easy to slip into the account what were called, in Ransdell's discussion considered above, game-terms: we may say, for instance, that Fred's intention was to win. And then, since the sense of 'win' which we intend is set by the rules constituting racing, we have acknowledged the new way of acting implicitly, and can readily suppose that it has some special relat-ionship with the domain, such that the domain supports the interpretation of the events as a race, rather than as something else.

But this is just carelessness. We start with the knowledge that it is a race - that we are looking at an instance of a certain way of acting - and in the normal course of events there is no reason whatever for us carefully to distinguish between expressions which

have sense within that way of acting, and expressions which we can use in the domain. The question whether there might be a way of acting other than racing, of which the current events might equally be an instance - an exotic religious observance, perhaps - is one which does not normally arise. Generally, we know perfectly well in what way of acting we are involved; only when we are observing a group, in the belief that it is exhibiting a way of acting, but without knowing what that way is, does this kind of question naturally arise. Hence, although as I argued in Part I, observed cultural difference cannot be made to constitute evidence for relativism, it is not surprising that some of the impetus for such views should come from a consideration of anthropological cases. It is in such cases that Winch, for example, has been concerned that we might take the observed way of acting to be a way of acting of ours, whichever one it might be to which the observations could be made most nearly to correspond, and neglect the possibility of asking what way it really is.

However, in the present context, the main point is that, although some of the evidence relevant to judgments in relatively subsequent ways of acting may come from relatively prior ways, the relatively subsequent ways are not supported, as ways of acting, against possible competing ways, by relatively prior ones. Hence a form of life is not an expression of a

reductionist theory of knowledge in the way Quinton suggests.

Nor, it should be added, is my account a "general picture of the world". It is rather a general picture of general pictures of the world. Ways of acting are not categories of things, if only because of the necessary normative component attributed to them. An account of a form of life may include an account of a hierarchy of categories of things, but it is not identical with it. A way of acting may involve the acceptance of a category of things; but the nature of the involvement and the relation of the category accepted to prior ways of acting is less simple than Quinton's account makes it appear. The introduction of chess involves the acceptance of chess pieces as things, but I have preferred an account on which expressions in the domain refer only to models of these things; and normative and descriptive interpretation do not relate introduced expressions to expressions in the domain in the same way. It is tempting to make use, for my own purposes, of the absolution that Quinton grants Strawson.

He says of Strawson that:

> He does not argue that, say, bodies are *epistemologically* prior to mental states, but that they are *referentially* prior. He does not hold that discourse about mental states owes its significance and justification to discourse about bodies, but rather that there can be no identifying reference to mental states

> unless a technique for identifying reference to bodies has been mastered. Unless his referential priority can be shown to entail priority of the more familiar epistemological kind there is no more than a formal parallelism between his system and the kind of ontology I have ascribed to Ryle. 5

Since I have disclaimed a reductionist theory of knowledge, and noted some similarities between Strawson's hierarchy and mine, I might borrow this way out of a commitment to epistemological priority of "the more familiar kind". But it would not do; indeed, I cannot see that it will do for Strawson, either. His willingness to have his referential priority called ontological priority, in circumstances where others might have wished to speak of epistemological priority, to which I shall refer below, raises doubts about the difference which Quinton claims to recognise.

More generally, it is hard to see how dependence of reference, which rests on dependence of identification, is to be separated from questions about significance. What could it come to, to say that talk of mental states was significant in a situation in which we could not identify anything as a mental state? Surely Strawson's point is precisely that, in the conceptual scheme we actually have, talk of mental states <u>does</u> owe its significance to talk of bodies. Without that basis, we could not know what we were talking about if we tried to talk about mental states. Nor can

questions of justification be separated from questions of reference and significance, provided we remember that it is the justification of claims made in the terms of a piece of conceptual apparatus that is at issue, rather than the justification of the apparatus itself.

Of course, it is attempts to justify the apparatus itself which characterise the kind of system to which Quinton compared Strawson's. Such attempts are commonly concerned with the attempt to produce logically equivalent translations, say, of statements about objects into statements about sense-data, or of statements about nations into statements about individual people. Statements drawn from different ways of acting with priority relations will not form logically equivalent pairs of this sort. A statement in an interpreted game has no logical equivalent in its domain, no matter how complex and detailed the offering may be. No statement set about what a runner did and felt and thought, no matter how complex, from which all racing game-terms were excluded, would be equivalent to the statement that he won a race. Such a statement-set would have to be enriched by the addition of the formal game constituting the new game and its interpretation; and this is not the kind of complex statement-set that reductionist epistemologists have generally sought.

It is a consequence of my account that knowledge

can have no foundations, in the sense in which reductionist epistemologists may have intended. It may be necessary, if statements issued in a way of acting are to be true, that some statements in a prior way of acting are true, though, as noted above, this necessity is not represented by a simple entailment between the statements in question; but it cannot be sufficient, for the truth of statements in the prior way of acting may equally support statements in alternative, incompatible or dissonant ways. The same holds for significance: it is in my terms a tautology that the existence of a basic way of acting is a necessary condition of any subsequent way's being significant; but it does not provide a sufficient condition for the significance of a subsequent way. At each step, significance is a function of the constitutive formal game; and this, as the discussion above indicates, is also true of basic levels, where questions of the significance of the given have to be answered in terms of a formal component.

However, it is also a consequence of my position that intelligible disputes can occur only within a form of life, that is, in a context in which they are materially related at some number of moves or other. The purely formal relationship which might be supposed to exist beween different forms of life at their basic levels - the relationship which might have held

between our conceptual scheme and the alternative which Strawson attempted to construct in the sound world - does not support the notion of a dispute. Strawson's attempt hides this point, for in drawing upon the notion of sound he was, of course, introducing a material relation, by providing a base for his formal game which we could recognise, although it was not the base on which we were accustomed to interpret that game. But to suppose a formal relation to be the only link to an alternative form of life is to suppose that we have recognised a form without having any idea of what it is in which the form is exhibited. I do not think that this is a coherent supposition.

It follows that no significant or intelligible dispute can be between forms of life: all must be within the form in which they are expressed. Of those disputes which might be likely to resist solution, I have suggested that some might arise from the existence of incompatible and dissonant ways of acting. Such disputes will be intelligible because the opposed positions are communicable, and stubborn because the opposed positions are incommensurable in the terms offered by the domain. A further type of dispute, suggested by Quinton's comments and illustrated by Strawson, is that over the proper priority ordering of interpreted games. To argue, as Strawson does, that private experiences and the unobservable entities of physics are dependent

particulars is to engage in this kind of dispute. But it cannot be a dispute of the kind that Quinton suggests, in which only the way we talk about things, and not questions of significance and justification, or knowledge and existence, are at issue. The features of a way of acting which might be collected by 'referential priority' are those which, I have argued in earlier chapters in this Part, arise from the notion of interpretation, that is, from just that concern with hierarchical relations in the form of life as a whole that generates notions of epistemic orontological priority; and this is just the sort of interest which Strawson professes, and calls descriptive metaphysics.

Of course, Strawson does dissociate himself from the notion that when a category of things is picked out as basic, this means that to talk of other categories of thing is just an abbreviated way of talking about things in the basic category, and he makes the point by saying that he uses 'basic' strictly in terms of particular-identification. But he goes on to say that it is ".. unobjectionable to use the expression 'ontologically prior' in such a way that the claim that material bodies are basic particulars in our conceptual scheme is equivalent to the claim that material bodies are ontologically prior, in that scheme, to other types of particular"[6], and this point is one that he had already made in greater detail.[7] So although Strawson wishes to

reject that very simple notion of reducibility and support from below which I have also rejected, it is far from clear that this involves rejection of concern with all the significance and justification relations that Quinton suggests. Indeed, if Quinton were right, it would be very hard to see the point which might be served by noticing referential dependence.

This being said, however, it does seem to be the case that Strawson at times takes the notion of referential priority to be as separate as Quinton says that it is, and with unfortunate results. Thus, he is generally concerned to argue that the concept of a person is of something to which both physical and mental predicates are applicable, the latter on the basis of logically adequate criteria.[8] He also wishes to cite private experiences as being as close as possible to ".. pure cases of .. direct identifiability-dependence".[9] Now, even when we allow for some difference between a private experience and the sort of thing predicated of an individual by consciousness-attributing expressions, there is tension here.

It is true, in a way, that we can identify a particular private experience only if we can identify a particular person; but to identify something as a person is to identify it as a subject of private experience. This is not like the other case which

Strawson cites, of the unobservable theoretical entities of physics. Of those he could argue that we must have the notion of material objects first, and could come by these theoretical concepts only subsequently; but we cannot have the notion of a person that he wants without having the notion of private experiences. If the concept of a person is basic, and the concept of a person is, *inter alia*, of a subject of private experiences; if, to put this point in another way, the private/public distinction must be basic because it cannot be introduced in a system which starts solipsistically, then private experiences are basic, not dependent particulars. The claim that they are referentially dependent becomes trivial. It cannot be expressed as the claim that material bodies are ontologically prior to private experiences without suggesting precisely what Strawson wishes to deny: that persons are put together out of two components of diverse origins.

What I think is to be said of referential priority is this. The kind of priority which Strawson has pointed out is readily explained as a product of interpretation. Generally, that an identification can be made in the terms of an interpreted game entails that some identification can be made in terms of the domain in which that game was interpreted. Our ability to use a gameterm depends on our ability to use some natural term; and this dependence is expressed in interpretive

rules. But this would suggest that talk of mental states, that is, of consciousness, that is, of persons is a game interpreted in a prior domain of material objects. Strawson needs to accept this in order to support his view of referential priority; but it also provides an account of the introduction of the concept of a person which conflicts with his desire to take that concept as primitive, even in the limited sense that "... it is not to be analysed in a certain way or ways". 10

In the end, I do not think it is possible to establish the limited and tidy kind of priority to which Strawson sometimes seems to aspire, and which Quinton thinks he attains. Questions of reference and identification, of significance and existence, cannot be pried apart in this way; and questions of priority in general are as radical a form of metaphysical question as there can be, so long as the question is to remain substantial and intelligible. Strawson pretends to do less than he does, in order to avoid the appearance of trying to do more than he can. In his view, a basic conceptual structure is not a foundation of certainty, in terms of which all subsequent questions will be decided, after they have been reductively analysed; Quinton agrees with this, and so, though in different terms, do I. But to make this point, it is not necessary, and not even sensible, to pretend that we can isolate questions of referential priority from everything else.

In fact, the most important aspects of Strawson's position go into my terms quite neatly. A key task of what he calls descriptive metaphysics is, in my terms, the identification of the given - that is, of those questions which must be interpreted as questions about a formal structure, and the features of that structure. Anything other than this, that is, anything which might count as revisionary metaphysics, must then fall into one of three categories. It must be either a mistaken account of the form of life in which it is issued, a futile proposal for a change in that form, or an idle suggestion about the logical possibility of unintelligible alternative forms.

Concerning the first category, Strawson's points are clear enough, as when he diagnoses one kind of mistake as arising from a misunderstanding of the function of a first person possessive pronoun. For present purposes it does not matter who is really mistaken: the important presumption is that the opponents understand each other, and that they will therefore eventually contradict each other, and that contradiction implies a mistake. Were it possible that the opponents were espousing dissonant or incompatible theories, this conclusion would not hold; but different accounts of the given cannot be either incompatible or dissonant, since these notions require a relatively prior domain.

Cases in the second category are futile because the proposals cannot be supported without apparent contradiction. The proposal must be made in the language of the form of life in which it is being made. In that language, it is the proposal that certain answerable questions are unanswerable, and *vice versa*. In effect, it is either the proposal that we speak in an unintelligible way, or an unintelligible proposal. Given a certain structure in a form of life, we cannot within that form give an account of an alternative which presents the alternative as intelligible. In effect again, the proposal must be a simultaneous invocation and repudiation of the same structure; and this, of course, is what Strawson wants to say that phenomenalist proposals are.

To the third category Strawson is kinder than I: he takes it that his discussion of the sound world, however tiresome, is at least discussion of an alternative. But as I have suggested above, if the example works it is because we can say a great deal about sounds - talk of sound constitutes an articulated domain - before we attempt to interpret a formal game in it as delivering a notion of reidentifiable individuals; it is therefore not an alternative proposal for the given. Any proposal which was not defective in this way could only present the formal structure and the logical possibility of its being basic in a form of life. This, for any purpose of Strawson's, is idle.

I have been concerned, in this and the preceding sections, to exhibit the claims that we can employ a notion of a form of life in which the conceptual structures of a particular way of acting are foundational, without regarding this foundation as determining the subsequent structure; and that any intelligible questions capable of giving rise to relativist answers, and indeed, any intelligible questions at all, must arise within a form of life. It follows from this that we can consider only one form of life, although we can accept at least the logical possibility of other forms under a purely formal description. These claims are not far removed from elements of Strawson's position, and I have proceeded by attempting to show the effect of reexpressing his views in the terms of my thesis.

The point of this approach is not to secure support from his agreement: even if such support were appropriate, the substantive agreement I have shown is patchy at best. The point is rather that the claims go to set the limit of any philosophical enterprise: they ahow what, upon my account, is the limit of intelligibility. It must be possible then to give an account of any philosophical work in these terms, one itself concerned with philosophical limits is an appropriate candidate, and if my approach has advantage, it ought to permit diagnosis and explanation of error. In these terms the claims have been exhibited and supported.

SECTION 21 The systems described in the previous chapters of this part permit and support statements of relativism as arising from the arbitrary nature of elements of the systems.

Formal games are arbitrary in the following sense. For any relations established between expressions by the formulæ of a formal game, there are no possible correctness checks. Nor can there be any question whether the set of rules given is complete or adequate, without some criterion of adequacy external to the formal game. Only in the light of an intended interpretation could these questions be answered, or even intelligibly raised. The identity criteria of a formal game are purely extensional.

Since there can be no significant question why a formal game is the formal game it is, and no basis for preferring it, as a formal game, to any other; and since there are indefinitely many possible formal games; and since formal games provide the category structure for sentences in interpreted games; then there is no reason to be given, in terms that are formal in the sense that they refer to formal games, why any particular category structure is available. Thus there is no formal reason for supposing that any category structure can be

regarded as the structure of reality; and hence, since I have not allowed that the structure of reality is given, no reason for supposing that what is independently or "really" the case is to be expressed in terms of any particular formal game. Since any putative expression of what is the case must be expressed in terms of some formal game, it follows that, if the structure of reality is not given, any formal game in use is one of a number of alternatives, and these alternatives are incommensurable, though not incommunicable. This is, in effect, the position which in Part I was identified as linguistic relativism, though it might as well be called ontological relativism.

This form of relativism is not immediately applicable to a way of acting, taking it that a form of relativism is immediately applicable to an area if and only if it provides an answer to questions about the decidability of the truth or falsity of statements issued in that area. A formal game yields statements only under an interpretation. Contingency is a product of interpretation, and questions about the decidability of a statement are in part questions about its interpretation. Moreover, since a way of acting is an interrelation of interpreted games, decidability may not be a function of any single interpreted game.

This form of relativism seems to have force as

a second-order theory, immediately applicable to formal games, to the effect that for any formal game there are incommensurable alternatives; and mediately applicable to ways of acting, to the effect that for any way of acting there are alternatives which may be incommensurable, on the premise that it is the formal features expressed in an interpreted game, rather than substantive or interpretational or experientially given features, which ultimately determine the identity of ways of acting.

Any interpretation is arbitrary in the following sense. For any formal game, and any domain in which it is interpreted, there is no necessity that that formal game be interpreted in that or in any other domain, or that that domain be interpreted by that or by any other formal game. This is most obvious in the case of games in the ordinary sense. It is not necessary that we should have had any activity with the formal features of baseball; nor is it necessary, had the physical apparatus of baseball occurred naturally, that we should have invested it with any significance at all. It is less obvious, but still perhaps plausible, in the case of more serious human activities, and more or less recondite explanatory theories. It is perhaps least obvious and plausible in the case of theoretical structures like commonsense realism. There is, however, no logical difference,unless we say that, in order to avoid

Platonism, we must allow that some formal game must be interpreted in some domain.

Normative interpretation is also arbitrary in the sense that the fit between the formal game and the domain is imposed upon the domain. There can be no significant question, in the case of chess, whether the conditions of victory in a game fit the formal conditions of checkmate: to find a difference would be to cancel the victory claim, not to detect a lack of fit. Where physical apparatus and its causal properties are clearly part of the interpretation, as in cricket or baseball, it may seem that there could be a failure of fit; but this is only apparent, since the notion of regulation or "real" equipment can be used to rule troublesome cases out of the game context. There can, in these cases, be problems about when someone should be said to be playing a game, but these are not to be confused with questions about the fit of the game. Similarly there are, as I shall consider in a later discussion of the is/ought gap, questions about when someone should be said to be acting in a particular context of moral judgment; but these are not to be confused with attempts to ask whether the particular context fits the world properly.

Descriptive interpretation is not arbitrary in this sense; failures of fit are possible, but remediable by revision of the formal game. Since such revision

is occasioned by the priority accorded in this kind of interpretation to relations between referents in the domain, and these relations must be expressed in the terms of the domain, we have in this case a fit imposed upon a formal game by a prior interpreted game.

Since descriptive interpretation is not arbitrary in this sense, and is dependent for its continuing acceptability upon contingent features of the domain, the fit between the formal game and the domain is in these cases typically underdetermined. Fit is developed under interpretation, rather than, as in the case of normative interpretation, established in the process of interpretation. To regard the formal game as reviseable under interpretation is to regard the formulæ of the formal game as contingent, or at least to regard the sentences which express those formulæ under interpretation as contingent. This is to treat the requirement of fit, holding between relations between expressions of the formal game and relations between referents in the domain, as a requirement for a truth/falsity relation, holding between sentences of the interpreted game and states of affairs in the domain. Thus the correspondence theory of truth is an expression of the principle of descriptive interpretation; it expresses as a relation between language and non-linguistic reality a principle by which relatively formal features of language are to be adjusted against relatively substantive features.

It can be readily seen that something like Scheffler's principle of indeterminacy between empirical truth and definition must arise in this case. An interpretive rule will give a quasi-definition of a new term; but the sense of this term would be fixed only if the fit of the interpretation were fixed, and fit is under-determined. To say this is in effect to say that sentences in a descriptively interpreted game are typically underdetermined, in respect of truth/falsity, by the evidence at hand; thus questions of truth/falsity and of the sense of theoretical terms are tightly related in descriptive interpretation.

None of this is a problem in itself, unless one wants to say that attempts to conceive of the world objectively are a problem in themselves; but problems can arise when the state of underdetermination is seen, at a philosophical level of interest, as damaging the capacity of expressions to refer. There are at least two related ways in which capacity to refer might be affected - ways which might be generally characterised as involving reference to inferred entities.

The first way, involving what has been called "open texture"[1], arises within commonsense realism. To refer to an individual as, say, a table, is, it is said, to commit one's self to an hypothesis, or to a set of hypotheses. The reference is successful only if these

hypotheses are true. That it might, had that attempt at reference failed, have been possible to refer to the individual as something else, is not generally helpful. It would have been odd for Macbeth to have asked if this were an hallucination he saw before him, with the handle towards his hand. It is not clear that hallucinations have handles; but more to the point, it would have been a different question. The question he did ask might sensibly have been addressed to Lady Macbeth; this question could not, without supposing particular circumstances: that it was not an hallucination, and that there was nothing before him, with or without a handle, except the thing he mistakenly inclined to think was an hallucination. One recourse, in the face of this kind of problem about maintaining confidence in reference, has been to phenomenalism: to the choice as a referent of something taken to be identifiable categorically. Or one can temporise: thus Moore, maintaining the truth of 'This is a human hand', but declining to choose between commonsense and phenomenalist analyses of the sentence. But the shift from commonsense realism to phenomenalism is not just the revision of a formal game, meanwhile maintaining reference to the same individuals under different identifications. It is the interpretation of a different formal game in a different domain: in effect, the reversal of a priority relation.

The second way arises within more developed

theories, when expressions refer to unobserved or unobservable entities. The obvious example is perhaps physics; but the same pattern seems to occur in religious doctrines. In physics, and the loosely similar case of religious doctrine based on argument from design, we have apparent reference to entities, unobserved in fact or unobservable in principle, in terms of which observable phenomena of the domain are to be explained. In both cases, there is the obvious challenge to provide justification for this ontic postulation, over and above the instrumental justification of the formal structure as serving interests within the domain. An ontological argument, though sometimes offered, does not meet the case, for this can only show what are the features of the formal game, which are not in question, and are revise-able under interpretation in any case. No observational justification is possible, of course; and pragmatic justification is not suited to distinguishing between ontology and utility. One recourse is to instrumental-ism; but this is less a revision of the formal game than a withdrawal of the interpretation.

The statements, the decidability of which is called into question in these ways, might be represented as being of the form, respectively, 'X is an A' and 'There are A's', the first, subject/predicate form being regarded as displaying the necessary identification

function of a potential referring expression 'A'. Questions about the decidability of both forms are legitimate.

Questions about the truth of statements in the first form would normally be regarded as askable and answerable. Such questions about statements in the second form might, in certain circumstances, be regarded, after Carnap, as external questions, and therefore unanswerable in those circumstances. However, such questions, which, again after Carnap, are questions about the acceptability of linguistic frameworks, are in my terms questions about the interpretability of formal games, which in turn are questions about the relations between a formal game and a domain established by a prior interpreted game; as such they are askable, and various answers may be attempted.

The phenomenalist answer to the question suggested about the decidability of statements in the first form - changing the game for one in which the referents are better behaved - offers a solution with difficulties. The difficulties most readily expressed in the terms used here are (i) that commonsense realism is logically prior to phenomenalism - that is, that the priority of public over private object talk is not reversible; and (ii) that it cannot provide translations of ordinary statements - that ways of acting cannot be

analysed in these terms. An obviously opposed answer, the more or less Austinian or Wittgensteinian insistence on the general adequacy of these statements in the conditions under which they are made, succeeds in explaining their function within a way of acting; but by in effect taking 'way of acting', or some analogous expression, as a primitive, it appears to argue to clarity from obscurity, by ignoring or treating as unaskable questions which, on the face of things, can be and have been asked.

For statements in the second form, instrumentalism and realism offer comparable alternatives with broadly similar effects.

Any alternative, or pair of alternatives, would suffice to protect reference; but, as noted above, the choice of an alternative cannot be fully justified on either formal or substantive grounds; and experience, that is, the development of a way of acting on a set of choices, will tend to confirm itself, by limiting the terms in which difficulties can be expressed. So again we have alternatives: alternative arguments which are inconclusive. To regard these arguments as inconclusive is not to say that we cannot know that something is independently the case, but to say that we cannot know that we say what is independently the case. This is the position earlier called epistemic relativism. The

examples suggest that epistemic relativism can take two forms: it can concern the relations of priority between interpreted games, or it can concern the difference between an interpretation and a model, if we can think of the instrumentalist as saying that we should use a formal game as a model of reality.

In normative interpretation, in which fit is imposed on the domain, the possibility of alternative imposed structures is readily conceivable. Since the supposition that the domain can accept alternative formal games under normative interpretation implies that one cannot discriminate between formal games on the basis of evidence from the domain, and since formal games are not directly comparable, this seems to lead directly to a fairly strong version of linguistic/ontological relativism, of which moral relativism would be an example, with no clear possibility of epistemic relativism appearing. But I think the position is less simple than this.

Judgments issued in terms of a normatively interpreted game, say, that Fred has achieved checkmate, or that George has acted wrongly, are generally readily defensible by appeal to the relative game: we can show the way a rule applies to the case. Problems arise when we attempt to defend the system of rules itself. This is also the case in descriptive interpretation;

but there the interpretation is not arbitrary in the same sense. Confronted with this problem, one may either introduce a new basis of defence - moral intuition or revelation, perhaps - or bring the interpreted game as a whole into the scope of existing defences by making it the referent of an expression in a descriptively interpreted game, as happens in utilitarian theories, and even more clearly in theories using a notion of social construction.[2] There are difficulties with the second alternative. While utilitarianism may serve as a defence, the notion of social constructs, if so used, merely shifts the problem. If it is descriptively true that pleasure is the only intrinsic good, then there is a basis in those terms for discrimination between normative claims, and a defence for some system of such claims. The defence will be tentative and inconclusive, of course; but this is just the consequence of successful transfer to a descriptive context. But explanation of moral systems as social constructs merely broadens the context for which a basis of discrimination or defence is needed. Some form of historical determinism is a currently popular answer. This can provide a basis for discrimination, and a defence of any system which can be conceived of as a characteristic of the terminal state of historical development; but apart from the problems associated with the notion of a terminal state, this approach has in an acute form the problems associated with supposing all

interpretation to be ultimately descriptive, which I have discussed above.

Generally, one might say: the first, or intuitionist kind of solution, comparable to the adoption of realist accounts in descriptive interpretation in that it tends to dismiss questions about the 'There are A's' form, solves this kind of problem at the cost of intensifying problems related to the 'X is an A' form. Intuition and revelation deal effectively with such problems only if there is massive consensus - which is to say, only if they do not arise in a pressing way. The second solution, comparable to the adoption of phenomenalist or instrumentalist accounts in descriptive interpretation in that it reduces problems about the 'X is an A' form by making it subject to the formal and substantive criteria already established in the domain, does so at the cost of withdrawing the proposed pattern of interpretation, and either inverting priority structures, or treating the formal game as an ontologically misleading model for some range of phenomena in the domain. Thus one has basically the same possibility of epistemic relativism as in descriptive interpretation, with similar differences based on priority and on the choice between interpretation and modelling.

As a final comment on this Part, it might be noted that the kinds of relativist position I have

attempted to display do not themselves go to constitute ways of acting. Liberal, tolerant, permissive or pluralist principles, like repressive ones, neither support nor follow from the arguments I have offered. Relativism as I have presented it is an account of the logical limits of the possibility of ratiocinative resolution of differences. It has nothing to say about what is to be done when these limits are reached. Such questions can only be answered in the context of a particular way of acting.

Some of the uses to which the structure I have proposed, with its relativistic implications, can be put I shall attempt to suggest in Part III.

# PART III

The proposed structure allows a consistent approach to a range of issues.

CHAPTER 8 The structure proposed allows accounts of rationality and objectivity which accept their association with scientific contexts without supposing them to be necessarily so related.

SECTION 22 Common accounts of rationality present ontological determinations in the guise of logical distinctions, and thus obscure and conflate issues.

On the account that I have given, a form of life is a system of ways of acting; and a way of acting is in turn a system of interpreted games, of which the molecular unit is a formal game, its interpretive base, and the resultant interpreted game, interpreted games being related materially and by priority both within and across ways of acting. Interpreted games which are dissonant or necessarily incompatible are not permitted to occur in the same way of acting; however, divergent ways of acting may share their domain, and must have a domain in common at some number of removes. Thus a form of life may exhibit dissonance, and intentional, contingent or necessary incompatibility, though only intentional incompatibility is clearly allowable within a way of acting.

Ways of acting are not incommunicable or

mutually unintelligible, even though there may be no process which could count as translating from the terms of one way of acting into the terms of the other. A way of acting is made intelligible to a non-participant by an account of the relevant formal game and of its interpretation in the common domain. That a participant in some way of acting may thus find dissonant or incompatible ways of acting intelligible does not imply that he may come to participate in them. His decision either to do so or not is non-rational in the sense that nothing available to him in the prior domain could constitute grounds for rational choice between dissonant or, at least, necessarily incompatible ways of acting. However, in rejecting such ways of acting he does not behave arbitrarily or whimsically. Being *ex hypothesi* already participant in a way of acting at the same priority level, he is already committed to rules and identifications relative to which the alternatives must be contradictory or incoherent. That this commitment may itself be in some sense arbitrary does not count against the claim that his rejection of the alternative is rational. Thus Kekes's Sinch, coming to understand Rench's faith, says in his heart that it is not acceptable, and in so doing reasons soundly; though Kekes, as I shall suggest below, mistakes the source of the soundness. On the other hand, acceptance of the alternative must, on this account, be an arbitrary act; the new rules are not available to support the rejection of the old until

the old have been rejected. I do not intend to deny that various apparent conversions may seem susceptible of explanation; my point is just that such explanations do not count against the claim of incommensurability of ways of acting which I make in these cases. Some explanations are in terms not available to the individual concerned: religious explanations of someone's coming to or leaving the faith cannot constitute reasons for those concerned. Explanations which draw upon the interests and motives of the individual tend to cast doubt on the claim that conversion occurred; explanations which draw on general principles attributed to the individual tend to cast doubt on the way the new alternative has been identified. If I join or leave a faith out of self-interest, I may well be a hypocrite; if I do so to serve the interests of the poor, I may be misdescribing my interests in calling them religious.

On the same account, alternative forms of life are possible, incommunicable, and incommensurable. A form of life takes its identity from the formal component of its basic interpreted games. Since formal games are not essentially related to any particular priority level, any formal game could, in principle, perform this identifying function; and thus there are as many possible forms of life as there are possible formal games. However, since a necessary condition of there being an actual alternative form of life is that its formal base should

have some material component, and since the material component of a basic interpreted game is given and uncharacterizable, such an alternative cannot be made intelligible.

I do not mean to maintain that one could not undergo conversion to an alternative form of life, or come to participate in it by learning it "from the inside". One has, after all, come to participate in a form of life; if other forms are possible, one might have come to participate, in the same way, in a different form of life; and it is not clear on what grounds one could say that conversion was impossible. But this is not so much a positive reason for accepting the notion of conversion, as an acknowledgement of the difficulty of making the notion clear enough to see what could be done with it. It is not even clear whether one supposed to have undergone conversion could be supposed to be aware of the fact. It is a very unclear notion in this context, and I see no reason for becoming involved with it. For my purpose, it is sufficient to see that an alternative form of life must be incommunicable: that is, that it cannot be made intelligible, as an alternative, to one not already participating in it.

At best, we can construct metaphysical models, revisionary in Strawson's sense, of alternative forms of life:[1] Ayer's sense-datum language might be taken as the

model of the language of a form of life in which phenomenalism was basic. But, to the extent that the metaphysics is revisionary, the model must appear logically flawed, as Strawson argues that the phenomenalist model is logically flawed through its reversal of priorities between public and private objects; and hence we could never encounter a range of phenomena which we took to be the actualization of such an alternative form of life. Neither, of course, could anything recognizable as a dispute or disagreement constitute a reason for supposing alternative forms of life to be involved, since disagreement presupposes the communicability of alternatives. Incommensurability, of course, follows from incommunicability.

Thus it seems that the notion of alternative forms of life is logically coherent, but of no practical philosophical use. I think that this is substantially true, and in general I take it into account by not attempting to make any use of the notion in discussion of the sorts of issue with which I have been concerned. However, a certain qualification should, I think, be noted. First, if the notion of alternative forms of life is logically coherent, then 'form of life' does not function quite as 'I' and 'my language' and so on were said to function by Wittgenstein:[2] it permits a gap between 'my world' and 'the world' which he could not

allow.

Second, the argument which supports the view that there is no genuine use to be made of the notion of an alternative form of life turns on the claim that the given cannot be characterized; and in one sense this claim is false. What is given on the Strawsonian view is, in part, characterizable as physical reality, or the universe of physical objects; what is given on the phenomenalist view is characterizable as the content of sense experience. Now, these are not, of course, neutral or independent characterizations. They involve particular systems of concepts within a form of life; though they differ in their identification of the form of life, they must both acknowledge their contextual debts; and the possibility of such characterizations cannot ultimately tell against the incommunicability thesis.

Yet although they are of necessity issued within the same form of life, so that their attempt to give an account of what is taken as brute fact in the form must be intelligible only within the form; and although the incommunicability thesis requires that one of them be a mistake, being in fact a characterization of something introduced in a relatively subsequent interpreted game; we might, by regarding them both as parts of metaphysical models, draw an interesting conclusion from comparing

them. The conclusion might be put by saying that the brutishness of brute fact seems to be a variable rather than a constant quantity.

Intuitively, there seems to be much more lee-way for descriptive interpretation in the context of sense experience than of physical reality: the chance of equally successful dissonant articulations of sense data seems much greater than of dissonant articulations of a world of physical objects. The logical fact behind this intuitive feeling is, as of course it must be, a fact about the formal differences between the models: the notion of a sense datum requires a less restrictive formal structure than the notion of a physical object, if only in that it does not require the distinction between public and private. Hence it seems to leave room for a variety of possible universes, solipsistic or otherwise, all equally compatible with brute fact, which are ruled out if the brute facts are those of physical reality. This seems to make very radical alternatives communicable: the notion of a solipsistic form of life seems communicable to a non-solipsistic phenomenalist, for instance.

Now, to use 'form of life' as I did in the last sentence is not entirely honest; for to character-ise the given in terms of sense data is to characterise

it in terms of a certain formal structure; that is, to characterise it in terms of the basic interpreted game of a phenomenalism-based form of life, on the assumption that the notion of such a form of life is intelligible. This could show, not that phenomenalism is a better basis for recognising or communicating alternative forms if life, but only that a phenomenalism-based form of life would be potentially richer in dissonance than a physical object based form; and even a comparison of this sort, since it can only be between the features of a form of life and those of a logically flawed model constructed within that form, must be highly suspect. But the only point that I wish to make is that what we can do through models of alternative forms of life to make such alternatives intelligible seems, at least intuitively, to be contingent on the nature of the metaphysical model itself, and upon the outcome of our descriptive metaphysics.

One might then wonder whether there is a possible piece of metaphysics, descriptive or revisionary, which expanded our ability to talk about alternative forms of life, either by offering a more flexible model, or by presenting our own form of life as one permitting the communication of radical alternatives, at least approximating to alternative forms of life. The discussion above suggests - and I would put it no more strongly than that - that the necessary feature of

such a piece of metaphysics would be that it permitted a characterization of the given which involved minimal structural commitment; ideally, that it involved commitment to the proposition that the given was structured, without commitment to any proposition stating what that structure was. I think that such a story is possible. It is not a part of my present thesis, and I shall not attempt to develop it in any detail, or to srgue for it. However, the possibility of such a story is, I think, of some interest in the context of my present thesis; I shall, therefore, in the final chapter, offer it in barest outline, and make some comment on what I take to be its implications.

This account of the sorts of relations possible in and between forms of life provides a basis on which certain questions which have arisen in the course of my discussion can be understood and answered.

I have previously considered a general anti-relativist argument based on the view that the alternatives proposed by linguistic relativism would be mutually incommunicable and hence unrecognisable; and a more specifically directed version of this argument, used by Kekes against Winch. Upon both occasions, I said that a more satisfactory account of a form of life might allow this argument to be met, despite its apparent force. Its apparent force is, I think, adequately

explained by the comments on alternative forms of life above: if 'form of life' is to be understood in the way I have suggested, it is clear that there are serious logical difficulties in the supposition that any sort of individual might be recognised as participating in a form of life alternative to that in which the observation was made; and related difficulties in the suggestion that an alternative form of life could be understood in any sense at all.

However, the suggested structure of a form of life is such that, through the occurrence of dissonance and the stronger forms of incompatibility, a form of life may contain ways of acting which are communicable but not commensurable, and these alternatives may be identified in terms of linguistic relativism. Relativism is possible because the language of a form of life comes neither as an indissoluble unity, in which an argument proposed in one area has effect throughout the whole fabric, nor as a concatenation of discrete "language games" between which communication is impossible, nor as a collection of languages between which translation might be attempted, but rather in distinguishable and related ways of acting; and because the domains in which subsequent games are interpreted contribute meaning, rather than certainty or even evidence, to the subsequent interpreted games. So there is an area of common meaning which allows participants in different

ways of acting to communicate; but the common meanings established in the domain do not have a quasi-epistemic priority, setting a standard meaning from which subsequently introduced terminology may deviate only minimally, if at all.

The above comment addresses the problem in more or less the form it was given by Kekes. It may be instructive to look again at Kekes, and see what of his argument appears correct in the terms I now wish to use.

He said that his scientific Sinch and religious Rench talk to each other in English. Their discourse is conducted ".. in a natural language upon which they superimpose a technical vocabulary". Although they may use 'rational' in divergent technical senses, these senses

> ... derive from the original, nontechnical use of 'rational'. The usual procedure for conducting such arguments as that between Sinch and Rench is to show how the contested sense of 'rational' is related to, how it derives from the original, nontechnical senses of 'rational'. And these senses Sinch and Rench understand in common merely in virtue of being competent users of English. Rench's answer to Sinch should be to attempt to trace out step by step how the ordinary senses of 'rational' came to be employed in religious discourse. Sinch, if he wishes to object, may do so by trying to make the case that the religious use of 'rational' constitutes too radical a departure from the ordinary uses; he may say that religious apologetics have no right to use the word 'rational' because its technical sense is separated by an abyss from its ordinary sense. 3

Now, on my account, Rench and Sinch must have in common a way of acting which provides the domain in which, at some number of removes, their religious and scientific games are interpreted; and if we take this common domain to be Strawsonian, it will no doubt be one which the vocabulary of ordinary or basic English serves quite well. The process of interpreting a formal game does, in a sense, superimpose a technical upon a natural vocabulary. This is the distinction between natural and gameterms again, with the correct implication that the technical vocabulary is dispensable, in that the prior way of acting is capable of functioning without it. However, Kekes attempts to do more than this with the notion of a technical vocabulary.

The precise status attributed to the technical vocabulary by Kekes is not quite clear. The only term he discusses is 'rational', and this is a special case. He could hardly suppose that 'electron' or 'god' were, as he supposes 'rational' to be, terms with an ordinary sense which came to be employed in a technical sense. Perhaps he takes it that technical terminology needs to be supported from below in a way most clearly shown in the case of 'rational'. The strong sense of 'rational' with which he is concerned requires a rational norm; and the rational norm he has in mind is fairly clearly that any beliefs presupposed or implied by norms of behaviour should be true, i.e., should correspond to the

facts. And the facts in question, we must assume, are those that are facts in the common way of acting. This is where scientific statements are ultimately grounded, where religious statements ought to be grounded, and, most importantly, where the notion of rationality is grounded.

More than a trace of reductionist epistemology is creeping in now. Not only does the prior way of acting provide the domain necessary for the interpretation of subsequent games; it is also taken to provide a benchmark by which successful interpretations can be identified: to get too far away from the benchmark is to go wrong. This is not the sort of priority relation which my notion of interpretation can be made to support; but it can be explained as an error, produced by confusion over the way in which a term like 'rational' can operate across ways of acting. Indeed, the confusion and error is fairly explicit in the passage quoted. 'Rational' is said to have ordinary senses, and original, non-technical uses. It is said to have a technical, contested sense in religious discourse, though whether this is a distinct technical sense, or an ordinary sense employed in a religious context, Kekes does not make at all clear, although the question seems to bear upon the force of his argument.

To tidy this a little, let us suppose that

'rational' has one sense with which we are concerned: the strong sense which Kekes most wishes to preserve. What, then, might distinguish the use of 'rational' in the prior way of acting from its use in a religious game? Not a difference in sense: in both cases we will endeavour to act only on the basis of beliefs which correspond to the facts. What will differ is the applicable notion of a fact. It is precisely that purpose of the rule-constituted contexts of interpreted games to introduce new descriptions under which people or things or events can be identified; that is, to make possible new kinds of fact. And this, of course, is why an interpreted game cannot be supported from a lower level: the notion of a fact which it introduces is not available at that level. Also, this is why we cannot test a technical sense of 'rational' against an ordinary sense: first, because there are not technical and ordinary senses at all; and second, because the ordinary use, which is use in a context determined by a particular notion of the kind of thing which can count as a fact, does not establish any benchmark of rationality. To "trace out step by step how the ordinary senses of 'rational' came to be employed in religious discourse" would be to trace a pattern of priority relations which might be quite extensive and complex, and would lead from the notion of facts possible in commonsense realism to whatever notion of fact could be supported in a religious context.

Whether a greater abyss would separate these categories of fact than separates commonsense notions of fact from the kinds of fact which could be countenanced in relativistic quantum mechanics I do not know. The point is not that the apparent scientific bias which leads Kekes to miss the possibility of the question is particularly wrong; it is that he is wrong about the whole notion of meaning relations, having confused them with epistemic relations. Because he did not bother to distinguish in any useful way between 'sense' and 'use'; and because, although he defined 'rational' in terms of norms, he did not bother to distinguish between a change in a definition and a change in a norm; he manages to talk as though all the questions which might be asked about relations between ways of acting could be put in the form of the question whether one term has been taken too far from its ordinary uses. The concept of rationality is, no doubt, central and important; but I doubt that it can be made to carry quite such a load.

What worries Kekes, and is expressed through the qualms felt by Sinch, is really the question what sort of odd thing Rench might be accepting as a possible identifying description of a fact. This is not a worry about the sense of 'rational'. It is not even a worry which could be very well expressed in terms of a common ground in Strawsonian persons and public objects. It is a worry most easily expressed in terms of scientific

realism: in terms of an interpreted game which already determines a range of possible facts, and does so in a way at least dissonant with religious games. In other words, it is Sinch's science, not his basic English, which puts him at odds with Rench. We knew that from the beginning, of course; it was Kekes's confused misdirection that led us to think that the tension was between the religious game and its domain.

Indeed, one may suspect that the whole notion of a theory of rationality, as Kekes uses it, is a massive mistake. Being rational, whether this means behaving in accordance with a norm, or with a rational norm, is not the sort of thing about which we need to theorise. Of course, we might wonder whether, say, Lukes was correct in thinking that certain conditions would have to be satisfied by any language we could come to understand, and we might see the relation of this point to the question whether we could identify something as behaviour, rational or not;[4] we might see that questions like "What is truth?" and "What is true?" affect the way 'rational' will be used. But we need not see that as a reason for putting theory of truth and theory of meaning and various unspecified physical and metaphysical theories about the world into one box, to be labelled theory of rationality. A move so likely to promote confusion is best explained in my terms as arising from an inclination to take certain theories

so much for granted as to see them as constituting the explanation of the sense of 'rational'. Thus one is not entirely surprised to find that Lukes's "rational (1)" criteria resemble the verifiability principle of a kind of logical empiricism.

The undoubted importance of the concept of rationality is variously and oddly represented. Lukes, talking about universal or rational (1) criteria of rationality, refers to "... criteria of truth (as correspondence to reality) and logic ... which simply <u>are</u> criteria of rationality"[5]. Now, what Lukes is really concerned with here are two questions which he does not distinguish: what conditions must be satisfied by anything properly called a language? and what conditions must be satisfied by a language in order that a particular group should recognise it as one? He takes it that some sort of verifiability principle together with some two-valued logic provides an answer to both questions, which it fairly clearly does not. This is perhaps why he supposes that Winch might deny such a notion of rationality, even though Winch has argued that rationality is an indispensable concept in any language, for very much the sort of reason Lukes has offered.

The notion of rationality covered by rational (1) criteria seems to be in effect the "weak" sense of

'rationality' used by Kekes, for which he attributes responsibility to Jarvie and Agassi. They say:

> Let us attribute rationality to an action if there is a goal to which it is directed; let us attribute rationality to a belief if it satisfies some standard or criterion of rationality which has been adopted, such as that it is based on good evidence, or is beyond reasonable doubt, or is held open to criticism, etc. When we attribute rationality to a person we can mean either: he acts rationally, or he believes rationally, or both.[6]

Weak rationality then becomes rational action, and strong rationality becomes rational action based on rational belief; and this is the basis for Kekes's notion of weak rationality as behaviour in accordance with some norm, and strong rationality as behaviour in accordance with a rational norm.

But let us look at this "weak" rationality. Why, exactly, is it to be called weak? Apparently, because of its permissiveness. It allows people to be called rational no matter how peculiar their norms might be. Magic can be presented as rational in this weak sense, whereas the strong sense allows us to see that only science is rational.[7] Now, we must first see that this is not a distinction at all unless it records some kind of bias - generally, a scientific one. There is no difference between action according to a norm and action according to a rational norm until we have a working notion of rationality. The Jarvie/Agassi account says that we achieve this by adopting some

standard or criterion of rationality. But this is nothing to Kekes's purpose, for action according to a norm which satisfies some standard is no different from action according to some norm, until we are clear on what makes the standard a standard of rationality, a point which may be hidden by the congeniality of the examples Jarvie and Agassi choose to mention. To define 'rationality' by a standard or criterion arbitrarily chosen would not suit Kekes; but to justify the choice of a standard or criterion without falling back into circularity is an achievement which proves continually elusive. Thus Lukes, having spelled out a particular notion of correspondence with reality, seems, in an immediate parenthesis, to allow alternative notions of reality and/or correspondence with it. It is for this reason that I equate his universal sense of rationality with Kekes's weak sense; he can tighten it up only through the requirement that any language have criteria of correspondence to reality "which we share with it",[8] and what turn out in effect to be attributions of scientifically oriented criteria attributed to "us". To shift from a weak to a strong sense of 'rationality' is, as I have already suggested, to shift from considering the sense of the term to considering its use in a context where certain notions of reality operate; but we do not accept such a context because it involves rational belief - rather, we know which beliefs are

rational when we accept a context.

It helps to see what the "weak sense" of 'rationality' is about, to see that what is not rational in this sense is what is not goal-directed or norm-governed: in effect, behaviour which is mere movement, in that questions about the reasons for the behaviour do not arise. That is, it is the "sense" of 'rational' in which it is opposed to 'arational' which is at issue; and of course it includes everything to which we might apply 'irrational'. But this does not mean it is a permissive sense, or a sense which is weak in any important way. 'Weak' is applied to it because the scientific preconceptions of Jarvie and Agassi and Kekes are going to be packed into the other "sense"; Lukes, whose positivistic preconceptions were linked to goal-directedness through the notion of necessary conditions for a (recognisable) language, called the rationality (1) criteria universal rather than weak; but the point is the same.

Rationality (1) criteria are those which must be satisfied if the rational/irrational distinction is to be drawable at all; weak rationality is the state of affairs which must obtain if the passing of judgments in terms of rationality or irrationality is to make any sense at all. This is the concept of rationality which Winch thinks is necessary to the existence of any language

although it may not be represented by the appearance of a word in that language which functions as does 'rational' in ours. In saying of a collection of people that they constitute a society with a language "... we imply formal analogies between their behaviour and that behaviour in our society which we refer to in distinguishing between rationality and irrationality".[9] Whether this notion should be called a sense of 'rationality' I do not know; it is odd to think of the sense of a word that might not occur. I should prefer to say that what is here being called rationality - the existence of at least formal patterns which lead us to identify something as a society, a language, a pattern of reason-regarding, norm-following, goal-directed behaviour - might as well be called a form of life.

A form of life is a rational structure in this "weak" sense: the notion of a form of life is the notion of patterns of behaviour pursued for reasons; and as the limit of intelligibility a form of life is also universal in the required sense. But then, the question whether there are alternative forms of life cannot be the question whether there are alternative weak senses of 'rationality', for this question makes no sense. This is a way of saying that alternative forms of life are incommunicable.

Alternative "strong" senses of 'rational'

would be communicable, no doubt. Since to have such alternatives would be to have available alternative bodies of belief, they would require ".. that hard core of meaning that allegedly different norms of rationality share in common"[10]. But this, according to Kekes, shows that alternatives are not possible: "only if magic and science and religion are genuinely different forms of life could one accept their having genuinely different norms of rationality"[11]. But then, being discrete, they would be incommunicable; while if communicable, then not discrete, and therefore commensurable, for their notions of rationality would not be really different.

This form of argument, roughly, that recognisability entails commensurability, may be said on my account to hold for alternative forms of life; but it does not hold for alternative ways of acting, which is the form in which Winch's most relativist positions need to be expressed. It does not hold because the link between communicability and commensurability, turning as it does on the notion that a common domain of interpretation must serve as a common epistemic base, breaks down when the account of the formation of ways of acting through interpretation is applied.

To understand what counts as, say, a religious experience is certainly not necessarily to accept that that experience, under any identifying description, must

count as evidence for the truth of a religious claim. Rather, it is to see that the notion of religious experience involves a particular identification of some part of the domain; and this may in a sense be easier to see, if not to accept, if one has already accepted a scientific game in which that part of the domain is to be identified as some or other sort of delusion. Indeed, to see such a case as involving interpretation, it is almost necessary to see it as a possible, but mistaken, alternative. To come to it without alternatives would be to see it as a matter of fact. This, perhaps, is the point of our fears of religious indoctrination - it is better that people should meet mistaken views only as alternatives - and our lack of fear of scientific indoctrination - science does not have alternatives. But the status of matter of fact, or perhaps of benchmark, is not defensible as issuing from characteristics which the domain had independently. The figure of light I see does not support science by being a delusion, or religion by being an angel. By the time it is identified in a way of acting, the question which way of acting it supports by its existence is already settled.

Now, one might make this sort of point by saying that the norms of rationality employed in religion and in science differ, so that different forms of rationality are involved; but there is no real advantage in

talking in this way, and plenty of opportunity for confusion. In different ways of acting, we have different complex ways of identifying and reidentifying people and things and events; we have, that is to say, different possible kinds of fact. Because our uses of 'rational' are not independent of what in the context of use we take to be matters of fact, our use of 'rational' will vary characteristically with ways of acting. But this need not be called a variation in the sense of 'rational', or in forms of rationality, unless there is some point to be made through that locution.

Since ways of acting are communicable, and since our general notion of rationality has to do with the intelligibility and communicability of judgments, we might wish to say that the notion of rationality was common to different ways of acting; since some ways of acting are incommensurable, and since our general notion of rationality has something to do with the soundness of judgments, we might want to say that there are different notions of rationality. Only if we think that communicability and commensurability are linked in the way that Kekes suggests will it seem to matter which way we decide.

SECTION 23 Objectivity is not peculiar to science, although a certain abstracted notion of science can serve as a paradigm of objectivity.

In a paper called "Objectivity and the Scientific Tradition", read to the ANZAAS conference of 1972, R.S. Walters argued that objectivity is specially associated with the scientific tradition, the public institutional procedures of science being necessary, but not sufficient conditions of objectivity. In the course of his discussion he contrasted science with art, on the ground that the latter is productive of "... something complete in itself, subject to interpretation and critical appraisal", and the former of "... a contribution to ongoing work that is contributed to by others and often completed by others", which "... has to be understood in relation to some body of theory and to the judgment of others similarly concerned with that body of theory". It is thus, he said, that we can "... significantly talk of objectivity in science concerning the products of scientific investigation and not in art concerning works of art". He also supported the view that the conceptual structure of science more closely resembles the actual structure of the world than such competitors as "magical, mythical, religious, animistic, anthropomorphic, anthropocentric conceptual frameworks", generally on the ground

that its public terminology is neutral, which he explained as meaning that "... unless terms are explicitly 'about' persons of some kind, the sense can be given without essential reference to a person or set of persons (class or interest-group) and testing procedures can be specified that do not essentially require consideration of specific persons".

This seems a fair sample of the common view that science is objective in at least two senses: that it allows us to make statements that correspond to reality in a way that statements of, say, magic do not; and that the scientific image of reality is a public or inter-subjective or cooperative construct, in a way that artistic images of reality are not.

Thus Scheffler suggests that:

> To propound one's beliefs in a scientific spirit ... is, in effect, to conceive one's self of the here and now as linked through potential converse with a community of others, whose differences of location or opinion yet allow a common discourse and access to a shared world. It is accordingly to lay oneself open to criticism from any quarter and to acquire an impersonal regard for the judgments of others; for what matters is not who they are, but whether they properly voice the import of controlling standards.[1]

Similarly, Popper sees in science a step "... comparable to the emergence of a descriptive and argumentative language, or to the invention of writing. It is a step at which our explanatory myths become open to conscious

and consistent criticism and at which we are challenged to invent new myths".[2]

All of which may be acceptable, as part of the business of saying what science - or objectivity - is; but there is more to it than this. Associated with this characterization of science is a sort of normative scientism, involving the claim that science is a rational way of going on, and leads to more rational ways of going on generally. Scheffler, in continuing his introductory comments on the objectivity of science, makes the point quite explicitly.

> Current science is continuous with other areas of life, and shares with them the distinctive features of the rational quest. However, in institutionalizing this quest so as to subject an ever wider domain of claims to refined and systematic test, science has given us a new appreciation of reason itself. Since reason is, moreover, a moral as well as an intellectual notion, we have thereby been given also a new and enlarged vision of the moral standpoint - of responsibility in belief, embodied not only in a firm commitment to impartial principles by which one's own assertions are to be measured, but in a further commitment to making those principles ever more comprehensive and rigorous. Thus, though science has certainly provided us with new and crucially important knowledge of man's surroundings and capacities, such enlightenment far from exhausts its human significance. A major aspect of such significance has been the moral import of science: its dynamic articulation of the impulse to responsible belief, and its suggestion of the hope of an increased rationality and responsibility in all realms of conduct and thought.[3]

More succinctly, Popper remarks that "... it is only science which replaces the elimination of error in the

violent struggle for life by non-violent rational criticism, and which allows us to replace killing (world 1) and intimidation (world 2) by the impersonal arguments of world 3".[4]

There is an oddity in this suggestion, most clearly to be seen in Popper's briefer remark. The apparent point of his comment is that we learn to achieve by impersonal argument what we could before only achieve by killing or intimidation; but this is absurd. If I covet my neighbour's wife, or his wealth, or his ox or his ass, I might, in a suitable society and given the appropriate capacities, gain possession of them by killing him. I might find that I did not need to kill him, since he could be intimidated, and thus accept the replacement of one means by another for some reason of my own. But the circumstances under which I would find impersonal argument a satisfactory replacement means would be peculiar indeed. The point is that the interests in which I might kill or intimidate are not impersonal interests, or something readily describable in neutral terminology. It is not in the least clear what bearing an impersonal argument could have on such a matter, or why anyone should think that it could have a bearing. It is tempting to say that Walters and Scheffler and Popper, by their stress on the neutrality and impersonality of science, have themselves ruled it

out that it could serve us in the way Scheffler and Popper suggest, by placing it firmly on the opposite side of the is-ought gap from the reasons for our actions, and even from the language in which our actions must be identified. This point is of concern in the next chapter, and I shall not pursue it now; but it should be noted that it is at least *prima facie* peculiar to suppose that science can be made more a model for human action by more clearly distancing it from the personal.

Popper's apparent suggestion that this alleged effect of science on human behaviour is the same sort of thing as its effect on human life in allowing us to modify behaviour on the basis of the prediction rather than the suffering of consequences is simply silly: the ability to determine the consequences of killing someone other than by experiencing them may as readily increase our determination to kill him. It depends on what the consequences are, not on how we find out about them.

It is particularly surprising that this silly mistake should be made, in view of the fact that it has been claimed that accounts of the logic of science, or rational reconstructions of scientific method, fail to be accurate accounts of the behaviour of scientists, even when they are practising science. Such accounts as Kuhn's serve as an example here; and while it is true

that neither Popper nor Scheffler accepts Kuhn's story, one might suppose that the existence of challenges to the "objective rationality" story as an account of a model for the practice of science itself, would lead to some caution in supposing that this might be stretched to provide a model for "all realms of conduct and thought". One wants to say that, if we cannot cross the is-ought gap, the suggestion is nonsense: the objective rationality and impersonal arguments of science can no more provide us with practical decisions than can a book of log. tables serve as plans for a bridge. If, on the other hand, we can cross the is-ought gap, it seems that we must either personalize our model of science, or impersonalize our notion of human action. The first alternative would make the account of science sketched here incoherent; the second would introduce serious problems in understanding the notion of human action, not the least of which would be the problem of understanding the nature and point of the preference which both Scheffler and Popper clearly display for series of events which do not include killings over those which do.

There is, I think, a confusion in the view I have sketched, which arises from the fact that the model of human conduct and thought, of which science is some part, is not rich enough to express everything

that Scheffler and Popper want to say: their concern with models of scientific enquiry and the use which may be made of them, has led them to somewhat overlook the aspects of life which do not readily fit the models. Popper is handicapped by his vision of world 3 as knowledge without a subject: and Scheffler's faith in extensionality is a similar handicap. Their attempts to say how science relates to the rest of human life are limited by the fact that the only language in which they have been interested is some sort of "language of science"; and this is not the language in which such a relation could be expressed.

I now wish to ask what sort of account of science might be given in terms of the account of forms of life I have attempted to develop, and what sort of translation of Scheffler's and Popper's comments might this account make possible?

Perhaps the most obvious suggestion in answer to the first of these questions, and the one with the clearest precedents, is that 'science'names a way of acting. As such, science would involve the use of mixed descriptions, and involve both descriptive and normative aspects. The normative aspects we would take to be those invoked in the quotations above: the canons of scientific enquiry, and the disciplines and

controls involved in the quest for scientific truth, so that the point of science as a way of acting would be the finding out of the truth about the world, in a sense of 'truth' which these disciplines and controls establish. The descriptive aspects would be those which Popper has attempted to invoke in the notion of world 3: the world of objective knowledge as self-existent, without a knowing subject. If science is taken to be a way of acting in the sense previously discussed, it is to be noted that these aspects are separable only as abstractions - we cannot suppose either to be separable in reality.

But this is just what Scheffler and Popper ask us to do. Scheffler depends on the notion of a neutrally descriptive extensional language so far removed from normative contexts that we can suppose it to be the basis of our communication with beings even when their basis of individuation is significantly different from ours. Popper, typically more explicit, argues for the "(more or less) independent existence of the third world"[5] and for ".. a sense in which world 3 is autonomous"[6]. He tells us that world 3, or a part of it, in the form of a book, ".. need not have been written by anybody: a series of books of logarithms, for example, may be produced and printed by a computer"[7]. Nor need it be read by anybody, though we may suppose that it might have been ultimately "deciphered" by ".. some civilized

successors of ours (no matter whether these are terrestrial animals which have become civilized, or some visitors from outer space". In fact, he says, ".. I do admit that in order to belong to the third world of objective knowledge, a book should - in principle or virtually - be capable of being grasped ( or deciphered or understood, or 'known') by somebody. But I do not admit more".[8]

This is, however, a considerable admission. The books ".. possibility or potentiality of being understood, its dispositional character of being understood or interpreted, or misunderstood or misinterpreted, which makes a thing a book",[9] requires that we suppose of it that it has the property of being mappable onto some part of world 1; its being "decipherable" requires us to suppose of it that it has the property of being mappable onto some <u>particular</u> part of world 1. There is no special trouble about the first: we already know of one mapping, and there might be others. The second is more tricky, for we have to suppose that any mapping-on alternative to that particular one of which we are aware must be impossible; and it is not clear why this should be so. Popper assumes that there is some straightforward difference between an interpretation and a misinterpretation. We would normally draw this distinction on the basis of a notion of intended interpretation; but this does not seem to be allowed by

Popper. He says that world 3 is a human product; but he thinks it has contents which have never been produced or understood by humans, and may never be so.

The fact that we can map the book onto the world in one way, and conceive of its being mapped on in others, whether we call these alternative mappings misinterpretations or not, gives rise to another problem, We can perhaps find other chunks of world 1 which we can map onto further chunks: the stages of a river onto the stages of organisms, the sands of the seashore onto the stars of the sky. Certainly we can map the universe onto itself. And do we thus shift chunks of, or the whole of the universe, from world 1 to world 3?

Nor is the attempt to regard a scientific attitude of responsibility in belief, or impartiality, or objective rationality, as really separable from its descriptive context, and transferable to any and all realms of conduct and thought, successful. Insofar as we need scientific information, of course, we might as well get it right, and must therefore know what counts as getting it right; but there are cases, like mathematics, where the need is minimal; and cases, like moral and aesthetic judgment, where the information is needed, but is only indirectly to the point.

Norms of scientific enquiry are definitive of a notion of finding out the truth about the world (of the notion of finding out the scientific truth about the world); but for activities with a different point, say, gaining the greatest immediate personal advantage from the world, they are not just unsatisfactory as norms - they are irrelevant. They have a secondary role in establishing means, but they do not identify the end. Of course, Scheffler and Popper do not exactly say that selfishness and violence would not occur if people took a scientific attitude to life generally; but they pretty clearly think that there is something in the notion of objective rationality which would proscribe them. This, as I have argued above, is wrong; and the point to be made here is that the wrongness lies not in supposing that scientific norms have a particular function in ways of acting in general, but in supposing that they have any function in ways of acting in general, and it arises from treating the norms as independently meaningful and thus transferable, rather than as abstractions from a particular way of acting.

In the case of the descriptive aspect of science considered as a way of acting, the need to regard it as an abstraction is one which might have occurred to Popper, for in involves an argument very similar to that which has been at the heart of his anti-inductivist

account of science. He has told us, in effect, that our ability to come by significant bodies of observation in the face of a massive bombardment of potential data requires that we begin with an hypothesis. But the field of potential hypotheses is as extensive as the field of potential data, unless we suppose that certain hypotheses are already rendered significant in the context of the point of a particular way of acting. Hence the above suggestion that we might identify a scientific way of acting as one the point of which was to find out the truth about the world is already to some degree involved with abstraction. The truth about the world is as complex as the world itself. Particularly among low-order hypotheses, there is a large, perhaps an indefinitely large, available range; and we must suppose that the choice of hypotheses for investigation is generally arbitrary, if the point of the way of acting is simply that suggested.

Now this, of course, is a patently false suggestion. The hypotheses with which we shake significant order out of the universe of observables are highly selected. If we will not allow them to be inductively forced upon us by the run of experience, we must allow them to be selected as significant for the point of activities in which we are engaged. On the face of it, then, science does not qualify as a way of acting, for

on the account given it could provide no basis on which hypotheses were selected for investigation, since any hypothesis may succeed or fail in stating the truth about the world.

However, this is only troublesome if we consider science, as a distinct way of acting, to be concerned in the first place with the production of low-order generalizations rather than with higher-order law-like statements. If we regard low-order generalizations as characteristic products of various ways of acting, the points of which provide the basis of selection from data which the generalizations express, we can regard the domain of preselected generalizations as the raw material of a scientific way of acting. That the account of science as a way of acting did not offer a principle of selection among possible hypotheses would not appear a problem in this case; the necessary focus of attention would already be effected in prior ways of acting.

Thus there are built-in limits to what we can do with the notion of science as a way of acting. If we wish to stress those characteristics which so impress Scheffler and Popper, while preserving its autonomy, or at least maximising its separability, we need to regard it as concerned with the explanation of preselected low-order generalizations, and hence as a basically

subsequent or derivative way of acting, with no capacity for being turned back upon and modifying the normative aspects of prior ways. If, on the other hand, we want to stress the continuity of science with other areas of life, and see it as including the selection of low-order generalizations, then we cannot suppose either that scientific activity is in fact characterized by adherence to the sort of norm of neutrality and impersonality which cannot achieve selection of data, or that it should be. One might play the game either way, being idealistic about a limited notion of science, or realistic about the place of scientific modes of rationality in human activities generally; but the notion that the canons of scientific enquiry offer a pattern for human rationality in general rests on confusion.

The general features of ubiquity, and the offering of a basis for a notion of objectivity, which on these accounts attach to science, are on the account I have offered here more clearly attached to the notion of descriptive interpretation. Descriptive interpretation is a feature of every way of acting; and to the extent that it is part of science, it is a part in which science is continuous with other areas of life. The language of the domain in which a formal game is descriptively interpreted can stand as an observation language: it has the necessary characteristics of an established and

presumably well-understood use relative to the new interpreted game; and of independence from the formal game under interpretation. It is not, of course, independent of the resultant interpreted game; but the case is not that the "observation language" is loaded by a theory expressed in the interpreted game; but that the interpreted game might be called the expression of a theory in a language derived from the observation language. The place of the formal game is analogous to that of a theory or hypothesis, in that it is subject to revision under interpretation; however, a formal game is not a theory or hypothesis, since these are meaningful statements or statement-sets which are true or false of something, while a formal game is not.

This approach is in effect the first of the two suggested above, invoking the notion of a descriptive interpretation characteristic of all ways of acting, including, if there be one, a scientific way of acting, to explain the context in which low-order generalizations are produced, and the inclination to suppose that science has something to do with all realms of conduct and thought. By the argument that no way of acting could be purely normative, descriptive interpretation is always relevant; to the extent that we can abstract the descriptive aspects of ways of acting, we might with benefit consider it is a scientific product, and open to

improvement.

This might not give to science all the moral import Scheffler suggests, nor would it necessarily lead us to replace killing and intimidation with rational argument; but if it improved communication in some aspects of ways of acting, this might be of some value, and perhaps as much as Scheffler or Popper could really hope for. What is more to the point is that it would be the most they could intelligibly suggest.

What above I called normative scientism cannot now be seen as extending the scope of a scientific way of acting. No way of acting with a point other than finding out the scientific truth about the world can turn into science. Instead, normative scientism must be seen as a two-part recommendation: first, to abstract from any way of acting its descriptively interpreted components; and second, to check the adequacy of the modification of the formal game in interpretation. Put in this way, it seems clear that normative scientism contributes to the creation of the is-ought gap, which, at least in the hands of Scheffler and Popper, it subsequently attempts to bridge.

A way of acting is just such a context that certain assertions do therein entail certain

prescriptions. 'The king is in check' entails 'Either move the king or ...'; 'The prediction was not experimentally supported' entails 'Either modify the hypothesis or ...': 'This is a non-smoking compartment' entails 'Either put out that cigarette or ...'. It is only when the descriptive components are abstracted, and treated as though they constituted an activity the only point of which was to determine the truth, that these relationships are broken; not because the abstracted bit is in some absolute sense unrelated to any norms other than those of scientific enquiry, but simply because what we have been doing, in the process of abstraction, is adopting the rule that we will consider it to be so.

On this account, then, the notion of objectivity is one that attaches primarily to a kind of interpretation rather than to a way of acting. That is to say, 'objectivity' is not, at least in the first place, the sort of honorific term that those authors I have mentioned in this section take it to be. Rather it is a term which owes its existence to a necessary characteristic of any way of acting: that some of the language of that way of acting is taken as descriptive.

In any way of acting, some terms of an interpreted game are so related to the terms of the domain in which that game was interpreted that the formal or

intensional relations of those terms may be varied according as certain statements expressible in the language of the domain are true or false. It is for terms to be so related to a taken-for-granted domain that constitutes their being terms of objective application, or components of objective statements. That objective statements are also possible in the language of the domain is to be similarly explained. Science as a way of acting, being in effect the apotheosis of descriptive interpretation, presents itself naturally as a paradigm of objectivity; but while some lessons of a scientific way of acting may be transferable, in the interest of more satisfactory descriptive interpretation in other areas, to be a paradigm of objectivity in this sense is not to be the model of human rationality. It is, at most, to be a reminder of an important part of rationality.

## CHAPTER 9 The is-ought gap arises from the same source as the distinction between observation and theory.

### SECTION 24 Searle's attempt to bridge the is-ought gap with institutional facts suffers from an inadequate notion of institutional contexts.

In developing my main thesis, I have drawn heavily upon two sources: Winch's account of forms of life, and Searle's account of constitutive rules. While I have claimed to modify or develop their accounts in some respects, I have offered a thesis which I think is close to the spirit of their claims, so that it might be expected that any significant claim to which they both subscribed would find some support in my account, even though in a variant form.

One important nexus of opinion is that which leads Winch to say that "... the social relations between men and the ideas which men's actions embody are really the same thing considered from different points of view"[1] and "... our language and our social relations are just two different sides of the same coin. To give an account of the meaning of a word is to describe how it is used; and to describe how it is used is to describe the social intercourse into which it enters"[2].

Similarly, Searle says that he is "... challenging a certain model of describing linguistic facts". He says that, according to the model he opposes

> once you have described the facts in any situation, the question of any 'evaluations' is left absolutely open. What I am here arguing is that, in the case of certain institutional facts, the evaluations involving obligations, commitments and responsibilities are no longer left completely open because the statement of the institutional facts involves these notions.[3]

He also says that "... there is no literal meaning of "promise" in which all it means is uttering certain words. Rather "promise" denotes speech acts characteristically performed in the utterance of certain words. But "promise" is not lexically ambiguous as between uttering words and undertaking obligations".[4]

Winch's social relations and Searle's institutional facts are in this context the same thing; and they agree that, where language is a part of the expression of these relations or institutions, the meaning of the language thus used is not detachable from the function it has in the institution. They are agreed in distinguishing between natural and social/institutional facts, the former being for Winch represented by the electrical storms which existed, as a soial relation could not, long before there were human beings to form concepts of them,[5] and for Searle by the natural physical facts which, rather than conventions, support the

activity of fishing.[6] They are also agreed that the description of a social practice or an institutional fact is a factual description, rather than an evaluation. Of course, Winch is concerned to argue from this that descriptions of social relations or institutions must go wrong if they are not couched in the terms which have sense in the institution itself, while Searle's purpose is to argue that we must preserve the integrity of the language of the institution; but this divergence of interest does not seem to reflect any difference of opinion.

Most significantly for my present purpose, we can note Winch saying that "... an act of obedience itself contains, as an essential element, a recognition of what went before as an order"[7], from which we can infer, with the help of his other comments, that the idea of obedience includes the idea of an order, and that 'obedience' is conceptually and logically related to 'order', and no doubt to other terms such as 'authority' as well. Now, except for the point that utterances of 'obey' do not figure in acts of obedience as Searle says there may characteristically be utterances of 'promise' figuring in acts of promising, what Winch says is exactly what Searle wants to say of the social relation or institutional fact of promising. To promise is to undertake an obligation; the idea of a promise contains,

as an essential element, the idea of recognition of obligation; and 'promise' is conceptually related to 'obligation', not in one of a number of alternative senses of 'promise', but once and for all.

The flowering of this view, in Searle's case, is his putative derivation of ought from is. This provides a convenient point of focus, since it brings what I think are fundamental aspects of the kind of view I have been concerned to defend together in a relatively concise piece of argument, and it has been extensively discussed. I shall in this section consider certain aspects of the discussion in the terms in which it was conducted, drawing most upon some relatively recent criticisms by J.L. Mackie. I think that it can be shown in this way that the most common points of criticism, and the questions which arise in relation to them, are just what would be expected on my account. Having made this point in general terms, I shall in the next section consider how the points at issue might be identified and considered in the terms I have offered.

Searle's derivation moves from the sentence 'Jones uttered the words "I hereby promise to pay you, Smith, five dollars"' to the sentence 'Jones ought to pay Smith five dollars' in four steps with the support of several auxiliary premisses, of which, as standing at the points where the case has been most attacked, the

most significant are the premiss Searle numbers 1a, viz. under certain conditions C anyone who utters the words (sentence) "I hereby promise to pay you, Smith, five dollars" promises to pay Smith five dollars; and a *ceteris paribus* clause intended to protect the argument from the effect of unusual conditions.

Searle notes that "... criticisms of the derivation tend to fall into two categories - those which attacked the *ceteris paribus* clause and those which attacked the alleged logical connection between promising, obligation and 'ought'".[8] The same tendency is noted by W.D. Hudson, who explains the point of the second criticism as being that Searle "... is not clear on the difference between reporting the use of a word such as 'promise' and actually using it".[9]

Searle says that he has side-stepped the first criticism by "... excluding from consideration within the proof the various kinds of consideration that the *ceteris paribus* clause is designed to deal with",[10] and being prepared at need simply to add 'other things being equal' to his conclusion. Since the considerations in question have to do with moral dilemmas, the settlement of which is not really part of the issue Searle discusses, I shall regard this as a sufficient defence in the present context of discussion. However, I shall later suggest that Searle was right in the first

place to be concerned with the kind of question to which he advanced the *ceteris paribus* clause as an answer, not because he should have shown how moral dilemmas were to be resolved, but because matters important to his argument turn upon things other than the mere possibility of making a promise. He says that he is concerned with a relation in language and not with a moral issue, and indeed is not sure that promising is a moral issue; but to see the point of the rule on which the relation is founded we need to know something of its context. This point is made by Phillips and Mounce in discussing our ability to recognise a moral rule;[11] but it has general implications.

The second category of criticism Searle regards as significant, since it directs attention to his premiss 1a, "... which gets us from the brute to the institutional level, the level that contains obligations".[12] The crucial question turns out, as it does for Winch, to concern the differences there may be between a detached or anthropological account of the doings of Jones in the institution, and an account issued from within, or in acceptance of, or in commitment to, the institution. The criticism is that a report, in a detached, anthropological way, can report on what people say without any commitment to evaluative statements at all. Searle's answer is that it is true that one can avoid commitment

by indirect quotation, but that this would count equally against any argument, and so does not count particularly against his.

Pursuing this argument, Mackie says that Searle is wrong: that

> words like 'promise' ... have a peculiar logical feature not shared by most parts of language. The performance of a certain speech act in appropriate circumstances is, in virtue of one part of the meaning of the words, sufficient to validate the statement that 'Jones promised ...' ... But also, in virtue of another part of their meaning, such a statement entails 'Jones ought to ...'. ... The very meanings of these words thus embody synthetic claims, in fact justifications of transitions from 'is' to 'ought'. For this reason the adopting of such fragments of language is not a neutral matter, as is the use of most parts of language ... to use the word 'promise' ... with its full within-the-institution meaning is already to endorse the institution in a substantial way.[13]

No doubt Searle has asked for this. His suggestion that he is not particularly concerned with moral 'oughts', but with a perfectly general question about a copula and an auxiliary, and that commitment in the use of language is much the same across language, both pull in one direction; his suggestion that institutional facts are different from both brute facts and evaluations pulls the opposite way. The ground is prepared for Mackie to offer the moral 'ought' as the basis of significant distinctions, to deny the peculiarity of institutional facts, and deliver the argument quoted.

Nevertheless, Mackie's argument has problems.

Searle has considered the view that 'promise' has two senses, meaning in one the utterance of certain words, and in the other the undertaking of an obligation. He argued, I think clearly correctly, that the first was not a sense of 'promise' at all. It then reappeared as the detached or anthropological occurrence of 'promise' in a factual or descriptive statement, where it was mentioned rather than used, so that the statement failed to entail the claims it would have entailed had 'promise' been used. It was against this view that Searle argued that the logical links of any expression can be broken if it is only mentioned.

This seems clearly correct also. 'That is a black horse' entails 'That is black'; but 'That is what Fred calls a black horse' does not - indeed, on Mackie's account, it is not even clear that it would entail 'Fred calls that black'. This perfectly general point cannot be regarded as an argument against Searle's thesis in particular. Mackie attempts to deal with this by reintroducing the distinction between senses as a division between parts of the sense of 'promise', so that adopting bits of language with this built-in evaluative component is a special sort of business. Searle might well say: you state my argument as if it

were an objection against me.

Mackie's point about the peculiarity of 'promise' does not reinforce the anthropological point, as he wishes it to, but pulls against it. The complexities of use are not to the point if the basic claim is that the expression is not being used at all. Mackie says that "the performance of a certain speech act" validates 'Jones promised' by one part of the meaning of 'promise'; another part is necessary to entail 'Jones ought to'. But what speech act? Presumably, a promise; but then, why does not this alone entail 'Jones ought'? Mackie's language is evasive here. What he should say is that uttering certain words is part of the meaning of 'promise', not that performing a certain speech act is. But then he has to maintain that uttering these words is a _detachable_ part of the meaning, so that one can use 'promise' in this sense without incurring the entailment which arises from another part. But this is not so: to utter the words without incurring the obligation is not to promise at all; to perform the speech act is to incur the obligation. Perhaps Mackie does not notice the confusion because he takes it that the word is only mentioned anyway.

Still, to the extent that Mackie's main point is that Searle has not really contradicted Hume, he may

be said to have made it. If there are social institutions or practices in which obligations are attributed and incurred, then no doubt we can say so, and note how claims of obligation are established by the rules of the institution. But in noting that one must be inside the institution to incur the obligation, one stresses the difference between accepting the obligation and accepting the institution; and one notes that the institutional reasons which may require of a member that the obligation be accepted cannot be reasons for accepting the institution. Roughly, from the fact that there _is_ a certain institution, it does not follow that there _ought_ to be. And this, I think, is what has worried some of Searle's critics. He has seemed, perhaps, to argue from the fact that people are bound by promises to the view that they ought to be bound by promises, and to block this it has appeared to be necessary to defend a detached anthropological viewpoint which allows us to observe institutions while retaining our independence of them.

Yet it does seem perverse to attribute such a view to Searle; he might well see his critics as looking over their shoulders at moral and even political questions, and consequently attempt to detach his account from a particular concern with moral issues. He has stressed, I think to the point of error, the importance of connecting obligation to "... the notion of accepting, acknowledging, recognizing, undertaking, etc., obligations in

such a way as to render the notion of an obligation essentially a contractual notion".[14]

On examination, it seems less and less clear just what sort of gap it is that, say, Mackie is trying to keep open when he distinguishes between taking the statements in Searle's derivation as describing the institution from the outside, and taking them as spoken within the institution. Searle has argued that the gap between use and mention will not serve, and Mackie's reply is confused and ineffective. Mackie claims that there is a logical gap: that the conclusion of the derivation, taken as internal and having prescriptive force, is derivable only by a special logic, while taken as external it follows by ordinary logic, but has no prescriptive or evaluative force. But this is merely assertion: he reiterates that there is a gap where Searle does not see one, but this account of what sort of gap it is is not helpful. Of course, the statement attributing obligation to Jones depends on statements which are taken as asserting the existence of an institution and Jones's invocation of that institution - it is not held that Jones ought to pay Smith five dollars regardless of such facts as these - but what, precisely, is the difference between these internal and external senses supposed to be, now that it is clear that it is not simply the difference between use and mention?

Mackie suggests that Searle does not establish his conclusion that Jones ought to pay Smith five dollars as either a statement of objective value, or as a statement the acceptance of which as a conclusion would commit one accepting it to himself endorse or subscribe to the relevant prescription.[15] If this is true, and if Searle could be thought to have supposed that he did establish his conclusion in such "senses", we might have a clearer picture of the gap in question.

I can see no reason to think that Searle supposed that his conclusion expressed an "objective value", in the sense in which Mackie himself denies the existence of objective values: a sense in which values are part of the ultimate fabric of the world, to be discovered rather than invented by people. He wants to say that Jones's obligation is part of the constitution of an institution which Jones has invoked; and if someone like Mackie wanted to say that this was the only sense in which a value could be objective, there is no reason to suppose that Searle would object. Yet, oddly enough, this is not what Mackie says. If the conclusion asserted an objective value, he says, it would "... say that Jones's paying of five dollars to Smith is now intrinsically required - not just required by the institution of promising, but, given the facts that there is such an institution and that Jones has tangled with it,

required simply by the nature of things".[16]

Again, this is evasive language. One would like to see more clearly the force of 'intrinsically'; and why do we say that Jones has "tangled with" the institution, as though he had somehow been trapped? However, putting this aside, we are again confronted by the assertion that there is a gap: in this case between something's being required by the institution, and its being required "simply by the nature of things". But since the nature of things in this case is in fact that Jones operates within an institution which requires this of him, the nature of the gap is still far from clear.

Of course, we might say: granted that the institution of promising requires Jones to pay, I, who reject that institution, think that he oughtn't to. But why do I think he oughtn't to? Am I supposed to be saying that Jones's conduct ought to be governed by the rules of some institution of which I am a member but he is not? This seems the only line for Mackie. If he is to say that Jones is not <u>really</u> obliged to pay, even though the institution in which Jones operates requires it of him, he must be supposed to say either that the institution fails to reflect the true objective value in the situation, or to match some other and preferable institution, or that institutions are not the sort of thing that should be seen to be binding on its members.

The first would contradict his own account of values; the third makes nonsense of the idea of an institution; the second would at least allow him to suggest that the speaker/reporter, except when engaged in quotation, must be supposed to speak within his own linguistic institutions rather than those which govern Jones, and thus could not endorse the relevant prescription unless the relevant institution were part of his form of life also.

Although Mackie does to some extent follow this line, he does so in a rather feeble manner. He says: "I can surely refrain from endorsing the promising institution; I can decline to speak within it. No doubt this would be eccentric, unconventional, it might well make people distrust or dislike me, but it is not logically ruled out".[17] That is, he does not suppose that the observer might have alternative institutional commitments or social relations, but just that he might stand aside. But what does this standing aside come to? He might never give a promise, but would he also have to somehow prevent others from giving promises to him? Would he also have to encourage others to break their promises, and is it this that would make him distrusted and disliked? How much of our speech is within the institution, and how easy is it to suppose that one can just stand aside? All these are pressing questions, yet to some extent beside the point, for Mackie's main line

seems to be a version of the third alternative above: he suggests that Searle's conclusion is not only not a commitment for Mackie, but it is not even one for Jones.

His argument is almost incredibly bad. He considers as crucial the claim that "... by making the promise at one time [Jones] has committed himself to the promising institution in such a way that it will not merely be a change of mind but wrong for him to refuse to endorse it when the time comes for payment".[18] This he suggests is in effect the claim thatJones has promised to keep his promise, and is viciously circular in that "..we have to assume that Jones ought to fulfil his commitment to the promising institution before we can establish, in this way, his obligation to keep his promise to Smith".[19] So if, when the time for payment comes, he refuses to continue endorsing the institution or to accept the conclusion of Searle's argument, "... he will have changed his mind; there is no logical inconsistency here".[20]

But the view proposed was not that he would be logically inconsistent, but that he would be wrong; and the supposition that he has changed his mind about endorsing the institution, whatever this might come to, has no bearing on that. Nor need we ever accuse Jones of logical inconsistency - this is not quite the way the constitutive rule game works. Depending on circumstances, we might say that he did not promise, or we might

simply say that he made a promise, and broke it.

Part of the trouble is that Mackie has not taken into account even the first thing about the notion of constitutive-rule-constituted institutions which Searle is using. The best evidence of this occurs in a passage in the piece of argument now under discussion. One of the three points which Mackie thinks are run together to give us the idea that Jones ought to keep his promises is

> .. the hypothetical imperative: if you want to play chess you had better obey the rules of the game - not merely analytically, because if you don't obey the rules it won't be chess, but because if you want to play even anything like chess you need someone to play with, no-one will play with you unless you abide by some rules, and it will be easier to follow the established rules than to invent and consistently observe new ones. Similarly, if Jones wants to retain, for any length of time, the benefits of the institution of promising, he had better stick to its rules.[21]

What seems clear here is that the notion that the identity of an action or a practice has anything to do with the rules by which it is constituted weighs very lightly on Mackie. We want to play chess, or, if you like, something like chess - let's not quibble. The point is that we won't find anyone to play whatever it is with us if we don't abide by some rules, and we need playmates. And since we have to have some rules, they might as well be chess rules - it would be so

troublesome to have to invent new ones. Part of what he is saying here seems to be that cheating, whether in chess or promising, requires at least some appearance of honesty; and this, of course, is quite consistent with the sort of ethical theory he goes on to advance. But it entirely misses Searle's point, which has to do with the "merely analytic" point that it is only when your behaviour is seen as complying with the relevant rules that it makes sense to call it chess or promising; and it makes no sense, once the behaviour is identified in those terms, to pretend that the relation between the behaviour thus identified and the rules which make that identification possible is simply contingent, or variable *ad lib*.

No one would deny that Jones can break his promise, and might have a variety of reasons for doing so; but this does not show the nature of the gap that Mackie wants us to see. We might also agree that he could withdraw from the institution without acrimony, even before he had honoured his promise - which seems to be one of the points that Mackie wants to make - without seeing this as evidence for a gap. Although an institution of promising could not be thought to provide codes of behaviour for non-promisers, it could be thought to provide ways of identifying some situations to which it did not apply. If Jones is supposed to step out from

under the institutional umbrella for some good reason, then this may be as clear from inside the institution as from outside.

In one way, the point that Mackie wants to make is clear enough, but it is quite readily accepted by Searle. The point is that the rules constituting an institution are effective within the institution and not without it, and there is a difference between the areas of activity which lie within and without an institution. There are, I think, two factors which confuse this relatively clear point, and Searle is as much responsible for the confusion as Mackie. One factor is an element of muddle and conflation in the notions of accepting, endorsing, approving, acting within etc. an institution; the other, which I have mentioned in earlier chapters, is the radical unclearness of the notion of an institution which is being used.

There is a clear difference between notions such as participating in or acting or speaking within an institution, and the notions of endorsing or approving of such institutions. What seems to me in this context important is that, although I will generally have some choice concerning those institutions which I approve or endorse, I may have little choice about participation. Both Mackie and Searle speak, most of the time, as though

the social relations or institutions of which they speak were a sort of club, membership in which was at the option of each individual. But not only is it perfectly clear that one may be inducted in some cases willy-nilly; it is not always clear what is supposed to be involved in recognising an institution or exercising an option.

Searle does not entirely miss the point: he does say "One does not first decide to make statements and then make a separate evaluative decision that they would be better if they were not self-contradictory. So, we are still left with commitments being essentially involved in facts"[22]. But then, one does not decide to make statements either. Whatever "making statements" comes to, it is not the sort of thing that it makes sense to suppose one could opt into, or opt out of. One could stop saying things to others, of course; but nothing short of suicide would count as opting out of saying things to one's self, and one could opt in only if one were in already.

And at a simpler level, of which promising is perhaps a good example, the notion of acceptance and endorsement can be inapplicable. I suggested above that the notion of standing aside from the institution of promising is odd. One can refrain from making

promises, but this is hardly enough. If promises are made to one, or to others on matters in which one is concerned, and if one plans one's actions accordingly, then one is not standing aside from the institution at all - one may not be speaking within it, but one is certainly hearing within it. But then, were one to determine to ignore all acts of promising, and to plan one's actions not only as though no one ever made promises, but as though no one even knew what a promise would be, one would simply be stupid, for one would be planning on the basis of false information, in the full knowledge that it was false.

Moreover, there is not just one form of speech act which counts as promising. All sorts of things might so count, and one held to have promised might not have the last word on whether what he did ought so to count. Searle suggests that it is a necessary condition of an utterance counting as a promise that the speaker intends the utterance to place him under an obligation, and quotes Mr. Pickwick's breach of promise case as an example of the intention being absent; but this is perhaps a philosopher's view rather than a lawyer's. With less knowledge of and faith in Mr. Pickwick, and no privileged access to his inner thoughts, the case might seem different. Certainly, it would not be clear that Mr. Pickwick was entitled to the last word.

Nor is it clear why both Searle and Mackie seem to think that the promise is somehow conditional upon the promiser's sincerity. No doubt the possibility of an institution of promising has something to do with assumptions of sincerity, as the possibility of an institution of statement making has something to do with assumptions about truth-telling; but I can promise insincerely just as I can lie. Searle does say that his "sincerity condition" distinguishes between sincere and insincere promises; but he includes it in a list of necessary conditions of promising at all, and he and Mackie mention sincerity as though it were somehow part of the required voluntary commitment to the institution. Thus Mackie says ".. if Jones makes a sincere promise, he is at least at that time endorsing the institution"[23]; and the implication is pretty clearly that if Jones were not sincere, then he would not at that time be endorsing - i.e., voluntarily and approvingly committing himself to - the institution; and, so far as Mackie's argument allows one to tell, would thereby evade the obligation which follows endorsement.

This is patent nonsense. Jones may cynically invoke an institution the obligations of which he has no intention of meeting, for some benefit he hopes to gain, and Smith may know him well enough to suspect this, and may place no credence whatever in what Jones says; yet

for all that Jones has made a promise. His speaking within the institution - using a form of language in the knowledge of its institutional implications - is all that is required in order that the full force of Searle's derivation of 'ought' should apply to him. This is part of the difficulty of supposing that Jones, or someone reporting Jones's activities, can stand aside from the institution. Of course they can disapprove of it, or hope to benefit from it without cost; but if they use its language, then they identify acts as that language identifies them, and this may include identifying them as acts by which obligations are incurred. Whether they approve of this, or endorse it, is not to the point.

Of course it would be easier, at least in the case of Jones, to see how he could stand aside from the institution in question if it were clearer just where the boundaries of the institution lay. At an earlier point in my discussion, I referred to a difference between a rule-constituted way of acting and a speech act, offering as an example of the different scope of these notions the example of the operation of the law as a rule-constituted way of acting, within which a number of different speech acts could be identified.[24] The point I there made, in present terms, is that a speech act like promising may arise in a variety of social

relations or institutions: giving evidence, standing for office, getting married, and so on.

Now, it is not in the least clear, when Mackie supposes that he or Jones might refuse to endorse, or change his mind about endorsing, the institution of promising, what sort of social relations he is thereby detaching himself from. Indeed, it is not even clear that he sees this sort of question as arising at all. Not only does he not say anything about what might replace the rejected institution; but he sees the action as being merely eccentric or unconventional, and giving rise to distrust and dislike; he does not seem to consider that more than eccentricity may be involved, or that the conventions he breaks may be very extensive. It is reasonable to notice that similar speech acts occur in a variety of contexts, and to take note of these similarities; but when we fall into speaking of this family of speech acts as though it were, in some primary sense, a social institution or relation, trouble follows. The notion of promising is both too limited and too general for this to work. It is too limited because it is the notion of an act which does not have a point in itself but requires a contextual explanation, so that the suggestion that it might be given up is one the point and consequences of which are obscure. It is too general because it is an activity for which we

might manage to find a place in almost any kind of social relation - it is in this respect similar to statement making - so that we can set no clear limit to the consequences of giving it up.

I think that this is the real source of Searle's concern with a *ceteris paribus* clause. He initially inserted it to cope with contingencies which might affect the obligation, and with the possibility of moral conflict. He continued to discuss these possibilities even when he dropped the clause, which is odd when one recalls that he was offering his thesis as one with no particular relation to moral matters. But of course, the equality of other things must be enormously important if it is the other things, rather than the promising, which constitute the institution within which promises are offered. Searle recognises the point, but only in a limited way. At one point, he says that considerations outside the act of promising bear on what obligations one has or what one ought to do, but not on the logical relations he is trying to spell out.[25] I think that the trend of discussion above shows that the role of considerations outside the act of promising is greater than he allows it to be, and generates many stubborn problems. The questions, like the voluntary or other nature of the institution, and its extent, which Searle tries to deal with in the context of a concern with speech acts, show

the need for a more substantial context, in which something more worthy of being seen as a social institution figures as the basis of an answer.

Many of the problems have to do with indeterminacy. I have mentioned indeterminacy of the notion of institution; and much of Mackie's argument has turned on the claim that many of the steps in Searle's derivation of 'ought' from 'is' are indeterminate between statements internal to, and statements externally descriptive of, an institution. While I have found much to criticise in Mackie, there is certainly some sense to be made of the notion that the conclusion of Searle's derivation is indeterminate between a statement descriptive of an institutional fact, and therefore true or false, and a prescription which is not true or false. But this is not the end of the matter. The point of the derivation turns on the function of constitutive rules. Of these, Searle has said:

> If our paradigms of rules are imperative regulative rules, such non-imperative constitutive rules are likely to strike us as extremely curious and hardly even as rules at all. Notice that they are almost tautological in character, for what the 'rule' seems to offer is part of a definition .... That such statements can be construed as analytic is a clue to the fact that the rule in question is a constitutive one.[26]

But these constitutive rules, which are in some sense indeterminate between rules and tautologies or

definitions, are also the things that turn up as statements of institutional fact. Thus, statement 1a. in the derivation is "... the one that gets us from the brute to the institutional level .... 1a. states a fact about the meaning of a descriptive word, 'promise'".[27] But clearly, a fact about the meaning of 'promise' is not just a definition or a partial definition - at least, not always. It can also be construed as a fact about a social institution or relation or practice; so on top of an indeterminacy between description and prescription, we have an indeterminacy between an empirical fact and one of a family of loosely related categories: rule, definition, tautology. And we are now reasonably reminded of a suggestion of indeterminacy between fact and definition which I have earlier noted in Scheffler, and related to Searle.

Scheffler suggested that the indeterminacy he noted was settled when we settled the point of our utterance; Mackie says something the same, since he suggests that deciding whether our point, in terms of the institution, is description or participation will resolve the indeterminacy; I have made a similar suggestion in saying that some aspects of the indeterminacy of Searle's statements are related to the identification of a satisfactory institutional context, and therefore, of the point of the social activity.

But I think that there is a more important source of indeterminacy than any of these, which can be expressed in terms of the thesis I have advanced. I shall use Searle's example of promising, and apply, in a somewhat sketchy but I think sufficient way, my earlier distinction between constitutive and interpretive rules to some of the statements he discusses. I shall hope to suggest that an alternative and interesting account of his arguments and difficulties can be offered in this way.

SECTION 25 The existence of justificatory gaps is a consequence of interpretation, and is not limited to moral or practical contexts.

Let us first suppose that Searle offers us certain formal rules, of which the simplest versions of the simplest cases are:

1. A only if B
2. C only if D
3. A only if C

Now that we understand of 'A' that it will hold only when 'B' and 'D' hold, we may be invited to interpret these formal rules by taking it that obligations count as C's, that things that ought to be done count as D's, that a certain sort of self-referential prediction count as B's, and finally, that we might use 'promise' in the place of 'A'. We now know that something is a promise if it is a certain kind of self-referential prediction, and only if it is an obligation of such a sort that I ought to satisfy it. By this some kinds of prediction are ruled out, as are obligations - possibly traditional or institutional ones - which I recognise but do not accept as binding, and anything which manages to satisfy only one of the descriptions 'self-referential prediction' and 'obligation'.

Certain formal rules are thus interpreted in an

existing domain of discourse, taking advantage - though, in the case of 'obligation', selective advantage - of the structure of that domain, and using it to introduce promises as a new category of action: a way of incurring an obligation by predicting. Of course, this is not an adequate account, and I have ignored many of the points Searle treated as significant; but it suffices for the point I wish to make.

On this account, a constitutive rule at work as the basis of an institutional fact is not one sort of thing, or even a set of things - rules - of the same sort. The formal rule is neither descriptive nor prescriptive, since it can have such a character only under interpretation. It may well appear "almost tautological in character", or such as "can be contrued as analytic", as Searle says constitutive rules are.[1] It provides a formal relation between symbols which have no meaning other than that which the rule itself confers upon them, so that there can be no sort of question about whether the rule gets the relation right, or whether there is any point in following it. Thus 1., 2. and 3. are not indeterminate, but purely formal.

However, there are two other sets of rules to take into account. First, there are the rules for interpreting 1., 2. and 3., which are sketched above in the

passage beginning "we may be invited to interpret", and ending "in the place of 'A'", where the rules are loosely cast in the '.. counts as ..' form. Second, there are the rules, which might be numbered 1'., 2'. and 3'., which are the interpreted rules which result when when the formal rules are interpreted according to the interpretive rules. The interpretive rules are prescriptive in relation to the formal rules, as specifying the process by which interpreted rules are to be produced; they are descriptive in relation to the interpreted rules, either of the interpretation which yielded them, or of the analysis which will reveal their formal content.

The interpreted rules may be in part descriptive of the domain, and in whole definitional of expressions referring to newly constituted relations or institutional facts. Some versions of the interpreted rules may also be used, where the definition is received, either to describe the institution which one supposes to exist in supposing the definition to be received, or to prescribe for those whom one supposes to participate in the institution in supposing the definition to be received. These last two uses are, of course, what Mackie wished to distinguish as senses which statements might have within the institution. I am suggesting, as Searle also suggested, that the gap between these uses and those mentioned before them is not the sort of logical or moral

one that Mackie proposed, which required that either the sentences be supposed to add or drop logical consequences according as they were used or reported, or according as the utterer approved of their sentiments or not; but is rather the gap between sense and nonsense, since to reject the definition is not to keep the expression with different entailments, but to make the expression senseless - or, should the symbol happen to be one with some other established use, to cling to an inappropriate sense.

On this view, the basis of indeterminacy lies in the process of interpretation: the process by which certain more or less definitional, quasi-analytic, near-tautological - i.e., formal - structures are given a pointful use in relation to a prior domain of meaningful discourse. Since it does not follow, from the nature of any formal structure or of a part of any domain, that the latter must provide an interpretation of the former, interpretation rules are prescriptive and stipulative: for the 'X counts as Y' formula we might read 'You are hereby required to take X's as Y's'. That this command is obeyed, if it is, is an empirical fact; but obedience creates an institutional fact: the fact that X counts as Y. Someone arguing as Mackie argues would dispute this, and suggest that the only fact created by obedience is that <u>someone</u> counts X as Y. One can see that the prescription is obeyed, and still choose whether

or not to obey one's self. Thus, Mackie says:

> When I speak of someone who 'endorses the institution, I do not mean one who merely approves of it or thinks it is beneficial, but rather one who thinks or speaks from within the institution .... it is not the mere verbal trick of an explicit performative which creates the obligation, nor even the existence, as a sociological fact, of the relevant practice. Someone who recognises both of these still has the choice either to stand outside the institution or step inside it. (My emphasis.) [2]

What Searle might call the institutional fact that X counts as Y is precisely what Mackie here says is optional; in effect, he says that there is no institutional fact, but only a sociological fact, until one steps inside.

The first point here is that for Mackie, institutional facts, like values, are subjective. That is, we should not say "It is an institutional fact that F", but always "It is an institutional fact for P that F", where P is some person or other. Again, he tries to hold open a non-existent gap. The sociological fact is not separable from the institutional fact. No doubt, if no one counted X as Y, there would be no sociological fact, though there might still be a sort of institutional fact - a fact of a possible institution. But if anyone counts X as Y, then there is an actual institution, and the sociological fact of the existence of that institution. It does not follow from this that everyone must approve of the institution; but Mackie says that

this is not his point anyway: his point is that they don't have to speak inside it. And of course this is true. One can describe the behaviour of a group in the language of a domain which does not include the interpreted rule based on 'X counts as Y'. The behaviour so described may appear pointless; but perhaps some people do behave pointlessly.

What one cannot do is recognise the existence of an institution, identify it in terms of the language structures which constitute the possibility of such institutions, and still hold open the question whether the feature by which the institution was picked out is really there or not. To use the language in stating the sociological fact is to state, indirectly, the institutional fact. One can escape by quotation, but the question this leaves open is the question whether the quoted expressions are meaningful. Hence one escapes the normal entailments of the quoted statements, but not only does this, as Searle pointed out, apply quite generally; it also has the effect of raising the question whether one in fact managed to identify anything, except, perhaps, a putative sentence. Thus the sociological fact is drained away along with the institutional fact.

This is to say that an interpretive rule gives sense to a new way of speaking through the relation in which it places it to a prior domain of discourse, and

that one cannot use ways of speaking so introduced as though the acceptance of the relevant interpretive rules remained optional; but this is not quite all that Searle's point about institutional facts requires. There is the further point that, under normative interpretation, the new way of speaking does not make a claim about the domain which is falsifiable in the terms of the domain. That is, if we take the introduction of 'promise' sketched above to be by normative interpretation of the three formal rules, the statement that a promise has been made will not be the statement that a certain coincidence of prediction and obligation has taken place, and is thus properly called a promise. Such a statement could be falsified, in a way, by one who had never heard of the new way of speaking, but was in a position to notice that the terms 'prediction' and 'obligation', both terms of the domain, were not correctly applied. Rather, it would be the statement that a certain event in the domain, properly called a prediction, constituted the undertaking of an obligation by being a promise, when it occurred under required circumstances. This claim is not falsified by the non-occurrence of an appropriate event; and, when the event occurs, there is no further discovery about it which can be made in the terms of the domain, and used to show that there is no obligation.

To show that there was no promise one would have to show that the institution was not invoked; but one invokes it in the use of 'promise'. This, I think, is the point of Winch's notion that a social relation and the idea men have of it are identical, so that there is no question of having the idea of a social relation, and wondering whether there is such a relation.

There is therefore a sense in which social relations or institutions can be arbitrarily created by anyone who chooses to invent a piece of language: that is, to give normative interpretation to a formal structure. In doing so, he creates a possible institutional fact. However, his doing so does not guarantee that anyone else will use this language, and until at least someone does, there will be no sociological fact of which anyone need take account. However, when someone does, they will accept not just a new expression, but a new social relation or practice or institution. R.M. Hare makes precisely this point, in discussing the way in which a group might come to accept that a speech act could count as the putting of one's self under an obligation, and sees it as related to the status, as tautologous or synthetic, of the rules or principles involved.[3] This agreement I think is important, even though Hare's further account of the status of the rules in question diverges sharply fom mine.

The Winchian notion of identity between social relations and ideas of them does, I think, provide reinforcement for Searle's notion of institutional facts; and the use of the notion of interpretation which I have offered provides further support, and a basis on which some of Searle's claims can be argued. But none of this, in the end, yields forms of argument which manage to dispose of the is-ought gap so clearly as he seems to hope, for it is in the nature of the support that it establishes a gap at just the point the is-ought gap is held to occupy.

Note first that Searle does not get rid of a notion of a brute fact, a fact being brute in that it does not have institutional implications. He suggests that auxiliary premiss 1a. in his derivation of 'ought' from 'is' gets us from the brute to the institutional level; but consider what this implies. If "Jones uttered the words 'I hereby promise to pay you, Smith, five dollars'" states a brute fact, then, by the arguments already offered, 'promise' cannot be supposed to have its ordinary sense, for in that sense it identifies the presence of an institutional fact. Since there is no basis for attributing a non-ordinary sense, we must suppose that the quoted utterance by Jones is not a meaningful sentence: the brute fact is that he uttered certain sounds. Premiss 1a. cannot then be a simple fact about what people do - it must double as a

rule of meaning, and indeed might have to be understood as a condensed citation of the formal and interpretive rules of the promising game. If it is so understood, most of the criticism directed against it would have to be said to be well directed. The only way of taking the strain off 1a. is to let the institutional fact back into the initial premiss, and then brute facts recede to the other side of the gap.

Substantially the same point may be made, more circuitously , as follows. I noted above that a common point of objection to Searle's story was that even if he showed how certain claims were supported within an institution, and even if we accepted his claim that to make sense of the claims in question involved recognition of the institution, none of this provided any support for the institution itself, or for the proposition that the claims in question made sense. This is very like the point that I was concerned to argue in Part I. I there suggested that in the defence of particular claims we committed ourselves to interactive bodies of belief and principle, to which I later attached the term 'ways of acting'; and that the defence of a way of acting was a problem of a very different sort.

Now, the thesis that I have advanced is in effect that we cannot defend a way of acting, because we cannot defend its constitutive interpreted games.

The relation of an interpreted game to its prior domain makes it meaningful; but the domain can give meaning to many, and to incompatible and dissonant, games. Hence this meaning relation, established through interpretive rules, cannot be invoked to defend one way of acting against another, or to show that involvement in the domain commits one to any particular subsequent interpreted game. One can argue backwards: given acceptance of some interpreted game, one can argue for the taken-for-granted necessity of the domain which is the basis of its intelligibility; but this is a different matter, of analysis rather than of justification.

On this account, then, there is a certain sort of gap, in the simplest case between any interpreted game and its domain. One might call it the justification gap, and describe it by saying that no set of statements in the language of the domain can entail a statement in the language of the subsequent interpreted game, except there be added as further premisses the constitutive and interpretive rules of the subsequent game. One might also say that facts stated in the language of the domain would count as brute facts relative to the subsequent game.

As a different way of pointing to the same gap, one might say that no set of statements in the language

of a domain could entail a statement to the effect that a certain subsequent game should be adopted. This approaches quite close to one of Mackie's ways of putting his argument; and it seems clear that it is closely related to the sort of point, about particular institutions, that he wished to make. However, the gap thus shown is not precisely an is-ought gap, or a gap between fact and value, for scientific theories are equally undemanded by their domain, which can as well provide meaning for religious or magical theories. Insofar as a claim for the peculiarly unsupported nature of values could be made in the terms I offer, it would depend upon the notion that scientific or factual games are introduced by descriptive interpretation (a comment which of course is tautologous); and thus make many claims issued in their terms subject to revision in the light of claims expressible in the terms of the domain, in a way that normatively interpreted games do not.

But this does not make the required point about the adoption of scientific games; that is, as we have noted in other cases, to see how an institution works is not to see why the institution is adopted. That bodies of scientific theory are not supported by the domain is a position which has been argued in various ways: in the anti-indictivist theories of Popper, and the more radical theories of Kuhn. Both present at least the initial adoption of a new structure as involving a sort

of imposition of order on a domain. I think it is clear that the thesis I offer would support such arguments at least to the degree it has supported those of Searle and Winch.

This kind of support does not get rid of the is-ought gap, or the analogous observation-theory gap; but it shows it in a new light. Such gaps are bridged by interpretive rules, so that a game-theoretic description cannot be inferred from a prior-domain description by purely logical rules of inference. But on the other hand, to discover such a gap is to confer implicit recognition on the rules in question, for to discover a gap is to give the game-theoretic description a sense relative to the domain. There is a gap between any game-theoretic level of language and its relatively brute domain. This is not a special problem for values, not only for the main reasons given, but also because the purely factual domain supposed can be achieved only by abstraction. Indeed, it is not a problem at all, if we resist the temptation to see ways of acting as part of an epistemic hierarchy.

CHAPTER 10 A concern with questions of form over questions of content supports the notion of alternative forms of life.

SECTION 26 It would be desirable to offer a more substantial account of alternative forms of life.

In *Ways of Worldmaking*, Goodman finds it ironic that "... our passion for *one* world is satisfied, at different times and for different purposes, in *many* different ways"; and says that the "one and the same neutral and underlying world" of which all right versions are versions, is "... a world without kinds or order or motion or rest or pattern - a world not worth fighting for or against".[1] In reply to Scheffler, who objects to the idea that we make worlds, and contends that we cannot have made the stars,[2] he says:

> Does he ask how we can have made anything older than we are? Plainly, by making a space and time that contains those stars. By means of science, that world (indeed many another) was made with great difficulty and is, like the several worlds of phenomena that also contain stars, a more or less right or real world. We can make the sun stand still, not in the manner of Joshua but in the manner of Bruno. We make a star as we make a constellation, by putting its parts together and marking off its boundaries.[3]

Now, much of this - perhaps, in the end, all of it - can be covered in my account by alternative or

incompatible or dissonant ways of acting, and their constituent games, within a form of life. But one can still. I think, read into such comment a leaning towards what I left as a formal possibility only: that we might have made a world quite otherwise, or that worlds may have been made quite otherwise, although we are not in a position to recognise them as they are. In Section 22, I made such alternatives incommunicable, and therefore insignificant, apart from the possibility of a certain sort of modelling. I shall briefly restate this position.

In discussing the notion of an interpreted game as basic or foundational in a form of life, I said that we could, in this case, abstract the formal game from the material component; and that while the formal game thus abstracted need not be regarded as distinguished from other formal games by any characteristics peculiar to its status as the formal component of a basic game, the material component, which I called the given, could not be regarded as having the prior articulation which, in the case of subsequent processes of interpretation, provided a domain of terms, natural relative to the formal game under interpretation, and taken for granted in the resultant interpreted game. Hence, although the material component of any interpreted game is taken for granted in the sense previously

discussed, the given, as the material component of a basic interpreted game, is distinct in that there is in this case no sensible question to be asked as to what is taken for granted: it has no points worth fighting for or against.

Thus I suggested that the possibility of alternative forms of life, that is, of alternative basic interpreted games, was in effect the possibility that the given might be articulated by alternative formal games. Since the given can only be discussed within a form of life, and since the significance of the language in which such a discussion could be conducted rests on the formal structure basic in that form of life, one could not hope to describe an alternative in an intelligible way, or produce intelligible samples from an alternative; though one might attempt to produce a model for such an alternative. Such models as I suggested were those offered by a reversal of priorities between existing interpreted games: the suggestion, for instance, that the formal structures of physics or of phenomenalist analyses might be seen as prior rather than subsequent to the formal structures of commonsense realism. To the extent that such analyses as those offered by Strawson are correct accounts of the form of life within which I now write - to the extent that I share with the man in the street "... the familiar

serviceable world he has jerry-built from fragments of scientific and artistic tradition and his own struggle for survival"[3] - such suggestions are contrary to fact, and will at some point involve severe logical strain, or even a kind of contradiction. They would thus offer imperfect models of forms of life in which the suggested priority orders were logically sound.

This account of basic interpreted games and alternative bases can be defended as showing what are the limits, in the context of the thesis I offer, of intelligible comment on the matter; and as being at least no more grossly obscure than such attempts as Strawson's to give an account of a basic conceptual structure, or such attempts as Winch's to give an account of alternative forms of life. However, even if these defences are accepted, some degree of dissatisfaction remains. The present account was offered as being more detailed and more useful than such alternatives; it might therefore be expected to offer less obscure suggestions on such matters, rather than suggestions which are no worse. Moreover, the clarification of such notions of relativism as are involved in the suggestion that there might be alternative forms of life was the major purpose for which the thesis was advanced. While it has to some extent achieved this by showing that some situations which have been presented as

involving alternative forms of life, like the tension between science and religion or magic, can be accomodated as involving alternative ways of acting within a form of life, such achievement only sharpens the question what non-Pickwickian sense can be made of the supposition of alternative forms of life. Simply to retrace some well-trodden paths of philosophical dispute, even by a new map which allows the route to be identified in a different way, seems a less interesting answer than one might have hoped for.

My attempts to do more than suggest the mere formal possibility of alternative forms of life can only be based on what are, at least at first sight, intelligible parts of some candidate; and those parts, to serve this purpose, must be regarded either as models produced by a distortion of their proper place in the form, or as mistakes. Still, one might wish for better models, or more informative mistakes, than have so far been suggested. If an alternative form of life requires only an alternative formal structure to that of, say, the public objects proposed by Strawson in its basic interpreted game, it ought to be a simple enough matter to propose such an alternative base, and indicate something of a possible form of life on that base. Something, whether phenomenalism, or scientific realism based on the theories of quantum mechanics, or magic, ought to

appear more capable of providing an understanding of a possible form of life alternative to the more or less Strawsonian one I implicitly accept, than seems actually to be the case. If these are not the right sort of models or mistakes, one might wish to know why, and whether something better is possible.

Magic, I think, counts as a mistake rather than a model. To the extent that stories which accept magic make sense, they do so by postulating phenomena which are recognizable in public object terms, but inexplicable on any available scientific theory. Thus presented, magic is not an alternative form of life, but a system of explanation whose acceptability is contingent on the actual course of events. If handsome princes were regularly changed into frogs, our explanatory theories would be different, no doubt; but this does not count as the identification of an alternative way of acting, much less an alternative form of life.

If magic provides an alternative way of identifying events which does not constitute a problem for scientific explanations, it may count as an alternative way of acting, but as one which shares its domain with scientific explanation. If, to make its effect more general, one treats it as part of a quasi-Berkeleyan articulation of the world on the basis of spirits and their perceptions, then one shifts to a more or less

phenomenalist model, and the question whether magic is a peculiarly appropriate development in such a model becomes subsequent to questions about the satisfactoriness of the model itself.

The difficulty with the phenomenalist model, as with that offered by scientific realism, is that it requires a better account than has to my knowledge been offered of how a form of life might be constructed on such a base. I am not now considering the common objection to such theories that the statements which might be formed within them are not translatable into, or in some sense equivalent to, certain statements of ordinary language. To require such relations of translatability or equivalence to hold between statements in alternative forms of life would clearly be a mistake on my account. But there are certain formal structures which we might expect to find in anything to which 'form of life' could intelligible be applied; those which, in our own case, might be represented in the existence of language, and in social or moral relations between individuals. Insofar as arguments have been used to show that phenomenalism cannot account for mutual recognition between individuals, much less for social or moral relations between them, and that scientific realism, with its concomitant notions of determinism, does not even allow significant meaning to such notions,

these arguments tend to suggest that neither theory can be seen as the basis of a possible form of life, no matter how liberal one's views on equivalence or translatability might be.

One might, of course, hope to refute such arguments. It is not to my present purpose to make the attempt; it is to my present purpose to consider what would be the consequence of success. Too much success would be disastrous. If counter-arguments were so successful that they met the translatability and equivalence requirement, they would count as accounts of the form of life in which they were issued, and thus eliminate the model. We might then try to take Strawson's thesis as a model; but since we would, *ex hypothesi*, fully understand everything that could be said in those terms, and presumably see through the mistake which led Strawson to take public objects as basic, this would not be successful. To preserve the models, we should have to succeed only in refuting the suggestion that nothing formally equivalent to language use or moral or social relations could be introduced in the models, while claims of non-equivalence or non-translatability remained sound.

But the models thus preserved have as their bases structures we already recognise in some sense:

the structure of the physical universe, or the sense-data structure of experience. To the extent that we manage to take either seriously, it seems we must take it not as an alternative, but as a competitor. In part, this is a consequence of the account given here. To suppose that the model is free of logical strain, which would be part of the notion of taking it seriously, would be to suppose that the Strawsonian context in which it served as a model was logically strained. This is to be expected: to take the model seriously, to suppose it accepted as a form of life, must be to suppose alternatives to be rejected as unintelligible, at least in the sense of being fundamentally logically flawed. But this is only part of the story. The competitor appearance of the models is, I think, also a consequence of the way the models are made intelligible as models.

For something to be recognisable as a possible base, it must be presented as an interpreted game - as a formal game under interpretation. But it then brings with it to the new context the crucial features of its interpretation in the old one. To suppose the theoretical structures of physics to be basic is to suppose _the physical universe_ to be given as structured; to suppose the theoretical structure of phenomenalism to be basic is to suppose _the elements of sense experience_

to be given, structured as sense data. If we attempt to play fair, by abstracting the formal structure from all the connotations and associations of the underlined expressions, we lose the force and purpose of the model; if we do not, we are left with accounts of the given which compete with aspects of our construct.

Insofar as this is a necessary consequence of the sort of modelling process I have proposed, which of course requires that the model be built of recognisable elements, it must be accepted. It would, however, be clearly advantageous if a model could be offered which met the requirement of recognisable elements for a basic interpreted game without importing such a heavy load of connotation, and which permitted the abstraction of formal structures without totally dissolving any notion we might have of the material or given component. Strictly speaking, this is not possible; but I think that some things which cannot be said can at least be hummed, and I shall attempt to satisfy these requirements in some degree. The model I offer is based on an essay in what Strawson would call revisionary metaphysics which I made elsewhere.[4] That essay was an enterprise quite separate from this one; it neither supports nor is supported by my present concern with relativism, or with the notion of forms of life. I shall not attempt to present the view offered in that essay fully, since

a minimal account will suffice for present purposes; neither shall I attempt to anticipate or counter possible criticisms. It would, of course, be no basis for a model if the metaphysical thesis were totally indefensible; but I do not think that it is totally so, and I shall take it that it is not. Neither shall I attempt to decide the extent to which the metaphysical thesis could deal with objections typically brought against theses of its type. I shall be concerned only to suggest what would be the effect, upon its usefulness in illuminating the notion of alternative forms of life, of varying degrees of success.

## SECTION 27 A basis for modelling alternative forms of life.

The metaphysical thesis on which the model will be based may be regarded as a variant of phenomenalism. Its basic interpreted game yields a concept of individuals. Individuals may be regarded, in certain limited respects, as like Strawson's enduring three-dimensional objects. Those properties which Strawson's objects had by virtue of their endurance in a three-dimensional reference frame exhaust the properties of individuals: an individual has shape, size and location at a time, and no other properties. The formal component of the interpreted game comprises a logic and a geometry and a metric, sufficient for the expression of co-ordinate sets determining a size, shape and location at a time, and formulæ relating such co-ordinate sets through time. Each such formula constitutes an individual. Any co-ordinate set derivable from such a formula is a predicate of the individual the formula constitutes, serving as an identifying description of the individual. This is, in effect, an account of individuals whose identity consists in spatio-temporal continuity. Although I have described the thesis as a variant of phenomenalism, such an account is applicable to public objects in the ordinary sense as well as to private objects of phenomenalism.

The material component of the basic interpreted game is discontinuity. Again, this notion is equally applicable to a world of public objects or a private woeld of experience; but consistent with the introductory comment, I shall speak in terms of the latter.

I take it to be uncontroversial that experience is qualitatively discontinuous. Further, I take there to be significant reason to suppose that the discontinuous character of experience is a necessary condition of its being meaningfully articulated. Although experience is discontinuous, it does not follow from this that there is anything, referred to by 'discontinuity', which is an element of experience in the sense in which, say, colour regions are held to be elements of experience, or sense-data, in phenomenalism. To be aware of a discontinuity is not to be aware of an experience of a certain kind; it is rather to be aware that something is the case. One might cite such examples as the awareness of discontinuities in peripheral vision, or the awareness that something has happened when the clock stops, which does not involve awareness of what it is that is discontinuous. Discontinuities are not a part of experience, over and above that, whatever it may be, that is discontinuous. They are a characteristic, rather than a part, of experience.

It is now to be supposed that the domain constituted by individuals is susceptible of further interpretations, yielding concepts of physical objects, conscious beings, a self and others, and generally all the conceptual apparatus of at least commonsense realism. It would go without saying that this supposition is open to challenge; and that at least those kinds of challenge which I have mentioned above, concerning first the possibility of introducing and interpreting the formal structures required to yield the concepts I mention, and second the possibility of producing statements equivalent to those of ordinary language, are among those that might be made. Neither the possible challenges, nor any possible replies, can be sensibly considered on the basis of this sketch, but this is not to the present point. I wish only to consider the consequences, for this story considered as a model of alternative forms of life, of success or failure in meeting such challenges.

The interpreted game proposed is one in which the notion of alternatives has considerable play. The notion of alternative metrics is entirely unproblematic: it can be taken for granted that a metric is essentially arbitrary. Logic and geometry are not clearly arbitrary in the same sense; nevertheless, the notion of alternatives is readily available: we are able to consider non-Euclidean geometries and deviant logics

as internally coherent systems, and also as systems with possible applications.[1] Thus it is possible, not only to suggest that formal games might be substituted at the basic level, but also to offer some suggestion as to the kind of substitution that might be made, and to consider, at least formally, the effect this might have on subsequent structures.

These are, of course, alternatives affecting the formal component of the interpreted game; but in this case I think it is also possible to form a notion of how such alternatives might affect the given or material component, without in the process losing our ability to recognise the basis of the model. I stressed above that discontinuity was a general characteristic, rather than a substantive component, of experience. That the substance of the universe, or the substantive content of experience, is in some sense indifferent to the forms in which it is articulated, which seems to be the aspect of Goodman's notions of worldmaking which troubled Scheffler, I regard as a difficult proposition to understand, much less to believe. This may be because we are at these points confronting matters of brute fact, though it seems to me a sufficient explanation to suggest that notions of the physical universe, or of the substantive content of experience, are relatively highly placed in the

hierarchy of interpreted games, so that they involve presuppositions which such propositions contradict.

But the notion that experience is characteristically discontinuous does not seem to lead to the same sort of difficulty. It is commonplace to think that our experience is indefinitely discontinuous; that it contains discontinuities that we do not recognise; that the discontinuities that we recognise at any particular time are to some extent determined by our purpose or interest at that time; that we can train ourselves to discover discontinuities in areas of experience that had seemed continuous, and that we can lose the ability to detect some discontinuities; that there are discontinuities the existence of which we can only infer; and that, as in the examples above, our awareness of discontinuity does not entail a corresponding awareness of the substantive features of experience which are discontinuous. Thus it is, I think, easier, if only psychologically, to understand, and perhaps to believe, the proposition that the discontinuous character of experience is indifferent to the forms in which it might be articulated.

Now, I have so far discussed this story as a piece of revisionary metaphysics rather than as a model, and it is important that the comments above were part of

that discussion. Total success in defending the story would not have the disastrous effect which I suggested would attend success in defending traditional phenomenalism or extreme scientific realism. If success were so complete that the metaphysics became descriptive rather than revisionary - if, that is to say, we came to understand our form of life as having the interpreted game I describe at its base - the above comments would still be available as an account of possible alternatives. Of course, we would still be unable to understand the alternatives, in the sense of knowing what it would be like to participate in them, any more than we can now understand what it would be like to live in a universe with, say, four spatial dimensions; but there would be a great deal that we could understand about an indefinitely large category of possible alternatives.

I think it is important that the success of such a story as this should not tell against its use, for the underlying concern with form, and indeed with the formal structuring of matters of form, shows in the literature, not only among those with some kind of relativist position, but even in such a case as that of Scheffler. His suggestion that we could learn to construct someone else's wholly different individuals out of elements admitted by us,[2] and Quine's comments on indeterminacy of translation affecting 'gavagai' and

'rabbit',[3] might readily be expressed in terms of the basic interpreted game the story proposes, as might Goodman's reference to making a constellation by putting its parts together and marking off its boundaries.

What I primarily wish to show through the discussion in this chapter is that, while my thesis must establish the possibility of talking about alternative forms of life in some terms if the expression 'form of life' is to have any particular significance, the degree of success one might have in talking about an alternative form of life in something other than "something I know not what" terms depends not upon the content of my main thesis, but upon the richness of the domain to which it is applied. This piece of metaphysical speculation is not especially original - it has a source, clearly, in Locke's distinction between primary and secondary qualities, to mention just one - and its lack of originality I think is significant.

The development of something like my main thesis would be, so far as I can see, possible in any form of life; but perhaps an occasion for its development could be found only in a form of life in which relativistic notions could find a grip. On my account, the most radical relativistic notions involve speculative

priority reversal, and are metaphysical not in the sense that they somehow transcend the form of life in which they occur, which seems a senseless supposition; but in the sense that they systematically ignore those logical/conceptual features of the form of life which arise from the interpretation rather than the identity of formal games. Thus the phenomenalist might be seen as concerning himself with the formal structure of subjective experience, ignoring the "referential priority" which Strawson, no doubt correctly, attributes to public objects within the form of life, and indeed even as proposing to interpret the formal structures of public objects in the domain of private experience.

If such speculative priority reversals are the basis of intimations of relativity, then it seems that no more radical reversal of ordinary understanding is possible than is involved in making pure form prior to substance. Even Quine, with his formal interests and his inclination to see a kind of relativism in individuation, paints the world first as a prelinguistic quality space of mass features: substantive content across which, in some places, the lines of divided reference are subsequently to be drawn.[4] The alternative view, in which the recognition of referents is prior to awareness of their substantive properties, or in which the basic level of a form of life is a

concern with form, gives substance to the most thoroughly relativistic notions we might entertain. It is a view which at least science fiction seems to acknowledge, when in so many cases it portrays initial attempts at communication with aliens as expressions of mathematical form.

# REFERENCES

## Introduction.

1. Lukes, S. and Runciman, W., "Relativism: Cognitive and Moral", in Proceedings of the Aristotelian Society, Supplementary Vol. 98, 1972, p. 165.

2. Goodman, N. Ways of Worldmaking (Hassocks: Harvester Press, 1978), p. ix.

3. Lukes and Runciman. Loc. cit..

4. Ibid. p. 168.

5. Quine, W.V.O., Ontological Relativity and Other Essays (New York: Columbia University Press, 1969), p. 28.

6. Magee, B., Modern British Philosophy (St. Albans: Paladin, 1973), p. 90.

7. Goodman. Op. cit., p. 119.

8. Goodman, N., "On Starmaking" Synthese. Vol. 45, 1980, p. 215.

## Section 1.

1. Lukes and Runciman, Op. cit., p. 168.

2. Ibid., p. 174.

3. Goodman, Ways of Worldmaking, p. 4.

4. Bartley, W.W., The Retreat to Commitment, (New York: Alfred A. Knopf, 1962)

## Section 2.

1. Davidson, D., "On the Very Idea of a Conceptual Scheme", Proceedings and Addresses of the American Philosophical Association, Vol. XLVII 1973-4, pp. 5-20.

2. Ibid., p. 5.

3. Ibid., p. 6.

4. Ibid., p. 18.

5. Ibid., p. 13.

6. Ibid., p. 18.

7. McGinn, C., "Charity, Interpretation and Belief", Journal of Philosophy, Vol. LXXIV No. 9, 1977, pp. 521-35.

8. Ibid., p. 526.

9. Davidson, Op. cit., p. 20.

10. Ibid..

11. Ibid., p. 11.

12. Ibid., p. 6.

13. Ibid., p. 7.

14. Quine, Ontological Relativity and Other Essays, p. 46.

15. Newton-Smith, W.H., The Rationality of Science, (Boston: Routledge and Kegan Paul, 1981)

16. Ibid., p. 35.

17. Ibid..

18. Ibid., pp. 35-36.

19. Winch, P., The Idea of a Social Science, (London: Routledge and Kegan Paul, 1958), p. 100.

20. Newton-Smith, Op. cit., p. 36.

21. Davidson, Op. cit., p. 6.

22. Ibid., p. 7.

23. Trigg, R., Reason and Commitment, (Cambridge: University Press, 1973), pp. 15-16.

24. Ibid., p. 23.

25. Corbett, P., Ideologies, (London: Hutchinson and Co., 1965), pp. 159ff..

26. Bartley, Op. cit., p. 139.

27. Ibid., p. 146.

28. Ibid., p. 139.

29. Watkins, J.W.N., "Comprehensively Critical Rationalism", Philosophy, Vol. XLIV, 1969, pp. 57-62.

30. Ibid., p. 60.

31. Ibid., pp. 60-61.

32. Bartley, Op. cit., p. 175.

Section 3.

1. Jarvie, I.G. and Agassi, J., "The Problem of the Rationality of Magic", in Rationality, ed. by B.R. Wilson, (Oxford: Blackwell, 1970), p. 177.

2. Brandt, R.S. (Ed.), Value and Obligation, (New York: Harcourt Brace and World, 1961), p. 435.

3. Horton, R., "African Traditional Thought and Western Science", in Rationality, ed. by Wilson, pp. 156-57.

4. Ibid., p. 157.

5. Ibid..

6. Ibid..

7. Esland, S., "Teaching and Learning as the Organization of Knowledge", in Knowledge and Control, ed. by M.F.D. Young, (London: Cassell and Collier Macmillan, 1971), pp. 70-116.

8. Gellner, E., "The New Idealism - Cause and Meaning in the Social Sciences", in Problems in the Philosophy of Science, ed. by I. Lakatos and A. Musgrave, (Amsterdam: North-Holland, 1968), p. 379.

9. Flew, A.G.N., Sociology, Equality and Education, (London: Macmillan, 1976), Section 2.

10. Gellner, Op. cit., p. 377.

11. Ibid., p. 379.

Section 4.

1. Winch, Op. cit., pp. 8-9.

2. Ibid., p. 41.

3. Ibid., p. 43.

4. Ibid., p. 121.

5. Ibid., p. 125.

6. Ibid..

7. Winch, P., "Language, Belief and Relativism", in *Contemporary British Philosophy* Series IV, ed. by H.D. Lewis, (London: Allen and Unwin, 1976), pp. 322-37.

8. Winch, P., "Comment" in *Explanation in the Behavioural Sciences*, ed. by R. Borger and F. Cioffi, (Cambridge: University Press, 1970), p. 254.

9. Ibid., pp. 254-55.

10. Ibid., pp. 256-57.

11. Winch, *The Idea of a Social Science*, p. 40.

12. Ibid., p. 24.

13. Kekes, J., "Rationality and Coherence", *Philosophical Studies*, No. 26, 1974, pp. 51-61.

Section 5.

1. Kekes, Op. cit., p. 54.

2. Ibid., p. 55.

3. Ibid., p. 60.

4. Ibid., p. 55.

5. Strawson, P.F., *The Bounds of Sense*, (London: Methuen, 1966), e.g., p. 44.

6. Searle, J., "What is a Speech Act?", in *Philosophy in America*, ed. by M. Black, (London: Allen and Unwin, 1965), pp. 221-39; and *Speech Acts*, (Cambridge: University Press, 1969), pp. 33ff..

7. Winch, P., "Understanding a Primitive Society", in *Rationality*, ed. by Wilson, pp. 105-106.

8. Kekes, Op. cit., p. 57.

9. Ibid., p. 58.

10. Ibid..

11. Ibid..

## Section 6.

1. Wittgenstein, L., *Philosophical Investigations*, (Oxford: Blackwell, 1963), S. 108, p. $46^{e}$.

2. Ibid., S. 143, p. $56^{e}$.

3. Ibid., S. 202, p. $81^{e}$.

4. Ibid., SS. 130-31, pp. $50\text{-}51^{e}$.

## Section 7.

1. Quinton, A., *The Nature of Things*, (London: Routledge and Kegan Paul, 1973), p. 109.

2. Ibid., pp. 109-10.

3. Ibid., pp. 111-12.

4. Rescher, N., *The Coherence Theory of Truth*, (Oxford: Clarendon Press, 1973), p. 324.

5. Ibid..

6. Quinton, Op. cit., pp. 113-14.

## Section 8.

1. Scheffler, I., *Science and Subjectivity*, (Indianapolis: Bobbs-Merrill, 1967), pp. 40-41.

2. Ibid., pp. 37-38.

3. Ibid., p. 58.

4. Ibid., p. 60.

5. Ibid., p. 42.

6. Ibid., p. 58.

7. Ibid., p. 42.

8. Ibid., p. 43.

9. Papineau, D., *Theory and Meaning*, (Oxford: Clarendon Press, 1979), p. 58.

10. Scheffler, Op. cit., p. 58.

Section 9.

1. Searle, *Speech Acts*, p. 39.

2. Ibid., p. 33.

3. Ibid., p. 35.

4. Ibid., p. 34.

5. Ibid., p. 35.

6. Ibid..

7. Ibid., p. 36.

8. Ibid., p. 35.

9. Ibid., p. 36.

10. Ibid., p. 34.

11. Ibid..

12. Ibid., p. 36.

Section 10.

1. Ransdell, J., "Constitutive Rules and Speech-Act Analysis", *Journal of Philosophy*, Vol. LXVIII, No. 13, 1971, pp. 385-400.

2. Ibid., p. 387.

3. Ibid., p. 388.

4. Ibid., p. 389.

5. Ibid., p. 388.

6. Ibid., p. 391.

7. Ibid., p. 390.

8. Ibid., pp. 390-91.

9. Strawson, P.F., Individuals, (London: Methuen, 1959), p. 110.

10. Ransdell, Op. cit., p. 391.

11. Ibid..

12. Searle, Speech Acts, p. 39.

13. Ransdell, Op. cit., p. 388.

14. Ibid., p. 392.

15. Ibid., p. 390.

Section 12.

1. Ransdell, Op. cit., p. 388.

2. Ibid., p. 387.

3. Searle, Speech Acts, p. 34.

4. Ransdell, Op. cit., p. 390.

5. Ibid., p. 391.

6. Ibid., p. 390.

7. Ibid., p. 388.

8. Deutscher, M., "Mental and Physical Properties", in The Identity Theory of Mind, ed. by C.F. Presley, (Brisbane: Queensland University Press, 1971), pp. 65-83.

9. Nagel, E., The Structure of Science, (London: Routledge and Kegan Paul, 1961), p. 90.

10. See p. 245 below.

11. Hempel, C.G., "On the Standard Conception of Scientific Theories", in Minnesota Studies in the Philosophy of Science Vol. IV, ed. by

M. Radner and S. Winokur, (Minneapolis: University of Minnesota Press, 1970), pp. 142-63.

12. Ibid., p. 161.

13. Feyerabend, P.K., "Against Method", in _Minnesota Studies in the Philosophy of Science_ Vol. IV, ed. by Radner and Winokur, p. 85.

14. Ibid..

15. Ibid., p. 86.

16. Ibid., p. 88.

17. Stegmuller, W., _The Structuralist View of Theories_, (Berlin: Springer-Verlag, 1979), p. 10.

18. Strawson, P.F., _Introduction to Logical Theory_, (London: Methuen, 1952), p. 62.

19. Ibid..

20. Ibid., pp. 62-63.

21. Ransdell, Op. cit., pp. 389-90.

22. Introduced on p. 185 above.

23. Introduced on p. 197 above.

Section 13.

1. Ransdell, Op. cit., pp. 392-93.

2. Frege, G., "Frege against the Formalists" in _Translations from the Philosophical Writings of Gottlob Frege_, ed. by P. Geach and M. Black, (Oxford: Blackwell, 1970), pp. 190-91.

3. Wiggins, D., _Identity and Spatio-Temporal Continuity_, (Oxford: Blackwell, 1967), p. 9.

4. Ibid., p. 18.

5. See, e.g., Bach, K., "What's in a Name?", _Australasian Journal of Philosophy_, Vol. 59 No. 4, 1981, pp. 371-85.

6. Wiggins, Op. cit., p. 3.

7. Ibid., p. 24.

Section 14.

1. Frege, "Frege against the Formalists", pp. 182-233.

2. Ibid., pp. 185-86.

3. Ibid., p. 186.

4. Ibid., pp. 190-91.

5. Strawson, Individuals, p. 110.

6. Searle, Speech Acts, pp. 39-40.

Section 15.

1. Goodman, Ways of Worldmaking, p. 138.

Section 16.

1. Toulmin, S., The Uses of Argument, (Cambridge: University Press, 1964), pp. 97-103.

2. Strawson, Introduction to Logical Theory, pp. 174-75.

3. Winch, "Understanding a Primitive Society", p. 106.

Section 17.

1. Warnock, M., Schools of Thought, (London: Faber and Faber, 1977), provides discussion of this contrast.

Section 18.

1. Carnap, R., Meaning and Necessity, (Chicago: University of Chicago Press, 1956), pp. 206ff..

Section 19.

1. Strawson, Individuals, p. 111.

2. Ibid., pp. 40-45.

3. Ibid., pp. 39-39.

4. Ibid., p. 86.

5. Ibid., p. 85.

6. Ibid., p. 101.

7. Quine, Ontological Relativity and other Essays, p. 46.

Section 20.

1. Quinton, Op. cit..

2. Ibid., pp. 240-41.

3. Ibid., pp. 242-43.

4. Ryle, G., Dilemmas, (Cambridge: University Press, 1956), pp. 104ff..

5. Quinton, Op. cit., p. 246.

6. Strawson, Individuals, p. 59.

7. Ibid., p. 17.

8. Ibid., p. 105.

9. Ibid., p. 41.

10. Ibid., p. 104.

Section 21.

1. Waismann, F., "Verifiability", in Logic and Language First Series, ed. by A.G.N. Flew, (Oxford: Blackwell, 1963), pp. 117-44.

2. cf. my comments on Esland in Section 3.

Section 22.

1. Ayer, A.J., The Foundations of Empirical Knowledge, (London: Macmillan, 1940).

2. Wittgenstein, L., Tractatus Logico-Philosophicus, (London: Routledge and Kegan Paul, 1961), e.g., 5.6, 5.63.

3. Kekes, Op. cit., pp. 58-59.

4. Lukes, S., "Some Problems about Rationality", in *Rationality*, ed. by B.R. Wilson, pp. 219-10.

5. Ibid., p. 210.

6. Jarvie and Agassi, "The Problem of the Rationality of Magic", p. 173.

7. Ibid..

8. Lukes, "Some Problems about Rationality", p. 210.

9. Winch, "Understanding a Primitive Society", p. 99.

10. Kekes, Op. cit., p. 60.

11. Ibid..

Section 23.

1. Scheffler, Op. cit., pp. 1-2.

2. Popper, K., *Objective Knowledge*, (Oxford: Clarendon Press, 1972), p. 84.

3. Scheffler, Op. cit., pp. 3-4.

4. Popper, Op. cit., p. 84.

5. Ibid., p. 107.

6. Ibid., p. 74.

7. Ibid., p. 115.

8. Ibid., p. 116.

9. Ibid..

Section 24.

1. Winch, *The Idea of a Social Science*, p. 121.

2. Ibid., p. 131.

3. Searle, *Speech Acts*, p. 190.

4. Ibid., p. 131.

5. Winch, *The Idea of a Social Science*, p. 125.

6. Searle, *Speech Acts*, p. 37.

7. Winch, *The Idea of a Social Science*, p.125.

8. Searle, *Speech Acts*, p. 188.

9. Hudson, W.D., "The Is-Ought Controversy", in *The Is-Ought Question*, ed. by W.D. Hudson, (London: Macmillan, 1969), p. 168.

10. Searle, *Speech Acts*, p. 188.

11. Phillips, D.Z. and Mounce, H.O., "On Morality's Having a Point", in *The Is-Ought Question*, ed. by Hudson, pp. 228-39.

12. Searle, *Speech Acts*, p. 190.

13. Mackie, J.L., *Ethics*, (London: Pelican, 1977), pp. 71-72.

14. Searle, *Speech Acts*, p. 189.

15. Mackie, Op. cit., p. 69.

16. Ibid..

17. Ibid..

18. Ibid., p. 70.

19. Ibid..

20. Ibid..

21. Ibid..

22. Searle, *Speech Acts*, p. 191.

23. Mackie, Op. cit., p. 70.

24. pp. 211-12 above.

25. Searle, *Speech Acts*, p. 180.

26. Ibid., p. 34.

27. Ibid., p. 190.

Section 25.

1. Searle, Speech Acts, p. 34.

2. Mackie, J.L., Hume's Moral Theory, (London: Routledge and Kegan Paul, 1980), p. 159n..

3. Hare, R.M., "The Promising Game", in The Is-Ought Question, ed. by Hudson, pp. 144-56.

Section 26.

1. Goodman, Ways of Worldmaking, p. 20.

2. Scheffler, I., "The Wonderful Worlds of Goodman", Synthese, Vol. 45, 1980, pp. 201-209.

3. Goodman, "On Starmaking", p. 213.

4. Goodman, Ways of Worldmaking, p. 20.

5. Simpson, K., Individuation and the Foundations of Knowledge, submitted an an Honours thesis for the degree of Bachelor of Arts, (Monash, 1968).

Section 27.

1. e.g., Haack, S., Deviant Logic, (London: Cambridge University Press, 1974), Chapter 8.

2. Scheffler, Science and Subjectivity, p. 41.

3. Quine, W.V.O., Word and Object, (Cambridge: Technology Press of the Massachusett Institute of Technology, 1960), pp. 29ff..

4. Ibid., pp. 80-85.

# BIBLIOGRAPHY

## PAPERS


Bach, K. "What's in a Name?" Australasian Journal of Philosophy. Vol. 59 No. 4. 1981. 371-85.

Davidson, D. "On the Very Idea of a Conceptual Scheme". Proceedings and Addresses of the American Philosophical Association. Vol. XLVII 1973-4. 5-20.

Deutscher, M. "Mental and Physical Properties". The Identity Theory of Mind. Edited by C.F. Presley. Brisbane: Queensland University Press, 1971. 65-83.

Esland, G.M. "Teaching and Learning as the Organization of Knowledge". Knowledge and Control. Edited by M.F.D. Young. London: Cassell and Collier Macmillan, 1971. 70-116.

Feyerabend, P.K. "Against Method". Minnesota Studies in the Philosophy of Science. Vol. IV. Edited by M. Radner and S. Winokur. Minneapolis: University of Minnesota Press, 1970. 17-130.

Gellner, E. "The New Idealism: Cause and Meaning in the Social Sciences". Problems in the Philosophy of Science. Edited by I. Lakatos and A. Musgrave. Amsterdam: North-Holland, 1968. 377-406.

Goodman, N. "On Starmaking". Synthese. Vol. 45. 1980. 211-15.

Hare, R.M. "The Promising Game". The Is-Ought Question. Edited by W.D. Hudson. London: Macmillan, 1969. 144-56.

Hempel, C.G. "On the Standard Conception of Scientific Theories". Minnesota Studies in the Philosophy of Science. Vol. IV. Edited by M. Radner and S. Winokur. Minneapolis: University of Minnesota Press, 1970. 142-63.

________. "Comments on Goodman's Ways of Worldmaking". Synthese. Vol. 45. 1980. 193-99.

Hollis, M. "Witchcraft and Winchcraft". Philosophy of the Social Sciences. Vol. 2. 1972. 89-103.

Hollis, M. Review of J. Kekes's _Justification of Rationality_. _Review of Metaphysics_. Vol. XXXII no. 3. 1979. 115.

Horton, R. "African Traditional Thought and Western Science". _Rationality_. Edited by B.R. Wilson. Oxford: Blackwell, 1970. 131-71.

Hunter, J.F.M. Forms of Life in Wittgenstein's _Philosophical Investigations_". _American Philosophical Quarterly_. Vol. 5 No. 4. 1968. 233-43.

Jarvie, I.C. "Understanding and Explanation in Sociology and Social Anthropology". _Explanation in the Behavioural Sciences_. Edited by R. Borger and F. Cioffi. Cambridge: University Press, 1970. 231-48.

________. Reply to Winch. _Explanation in the Behavioural Sciences_. Edited by R. Borger F. Cioffi. Cambridge: University Press. 1970. 260-69.

________. "The Problem of the Rationality of Magic". _Rationality_. Edited by B.R. Wilson. Oxford: Blackwell, 1970. 172-93.

________. "Cultural Relativism Again". _Philosophy of the Social Sciences_. Vol. 5. 1975. 343-53.

Kekes, J. "Fallibilism and Rationality". _American Philosophical Quarterly_. Vol. 9 No. 4. 1972. 301-309.

________. "Rationality and Coherence". _Philosophical Studies_. No. 26. 1974. 51-61.

________. "The Rationality of the Social Sciences". _Philosophy of the Social Sciences_. Vol. 9. 1979. 105-13.

Kuhn, T.S. "Reflections on my Critics". _Criticism and the Growth of Knowledge_. Editied by I. Lakatos and A. Musgrave. Cambridge: University Press, 1970. 278-31.

Louch, A.R. "The Very Idea of a Social Science". _Inquiry_. Vol. 6 No. 4. 1963. 273-86.

________. "On Misunderstanding Mr. Winch". _Inquiry_. Vol. 8. 1965. 212-16.

Lukes, S. "Some Problems About Rationality". Rationality. Edited by B.R. Wilson. Oxford: Blackwell, 1970. 194-213.

_______. and Runciman, W.G. "Relativism: Cognitive and Moral". Proceedings of the Aristotelian Society. Supplementary Vol. 98. 1972. 165-89.

McGinn, C. "Charity, Interpretation and Belief". Journal of Philosophy. Vol. LXXIV No. 9. 1977. 521-35.

Mounce, H.O. "Understanding a Primitive Society". Philosophy. Vol. 48. 1973. 347-62.

Neilson, K. "Rationality and Universality". Monist. Vol. 59. 1975. 441-55.

Phillips, D.Z. and Mounce, H.O. "On Morality's Having a Point". The Is-Ought Question. Edited by. W.D. Hudson. London: Macmillan, 1969. 228-39.

Popper, K. "Normal Science and its Dangers". Criticism and the Growth of Knowledge. Edited by I. Lakatos and A. Musgrave. Cambridge: University Press, 1970. 51-58.

Putnam, H. "Reflections on Goodman's Ways of Worldmaking". Journal of Philosophy. Vol. LXXVI No. 11. 1979. 603-18.

Ransdell, J. "Constitutive Rules and Speech-Act Analysis". Journal of Philosophy. Vol. LXVIII No. 13. 1971. 385-400.

Richmond, S. On the Possibility of Rationality: Some Comments on Roger Trigg's Rationality and Commitment." Philosophy of the Social Sciences. Vol. 6. 1976. 155-63.

Rorty, R. "The World Well Lost". Journal of Philosophy. Vol. LXIX. 1972. 649-65.

Shapere, D. "Meaning and Scientific Change" Mind and Cosmos. Edited by R.G. Colodny. University of Pittsburgh Press, 1966. 41-85.

Searle, J.R. "What is a Speech Act?" Philosophy in America. Edited by M. Black. London: Allen and Unwin, 1965. 221-35.

Waismann, F. "Verifiability". _Logic and Language_. First Series. Oxford: Blackwell, 1963. 117-44.

Watkins, J.W.N. "Comprehensively Critical Rationalism". _Philosophy_. Vol. XLIV. 1969. 57-62.

Winch, P. "Understanding a Primitive Society" _Rationality_. Edited by B.R. Wilson. Oxford: Blackwell, 1970. 78-111.

________. "Nature and Convention" _Proceedings of the Aristotelian Society_. Vol. 20. 1960. 50-72.

________. "Mr. Louch's Idea of a Social Science". _Inquiry_. Vol. 7 No. 2. 1964. 202-208.

________. Comment on I.C. Jarvie. _Explanation in the Behavioural Sciences_. Edited by R. Borger and F. Cioffi. Cambridge: University Press, 1970. 249-59.

________. "Language, Belief and Relativism". _Contemporary British Philosophy_ Fourth Series. Edited by. H.D. Lewis. London: Allen and Unwin, 1976. 322-37.

## COLLECTIONS.

Borger, R. and Cioffi, F., ed.. _Explanation in the Behavioural Sciences_. Cambridge: University Press, 1970.

Brandt, R.S., ed.. _Value and Obligation_. New York: Harcourt, Brace and World, 1961.

Colodny, R., ed.. _Mind and Cosmos_. University of Pittsburgh Press, 1966.

Daniels, N., ed.. _Reading Rawls_. Oxford: Blackwell, 1975.

Davidson, D. and Hintikka, J., eds.. _Words and Objections: Essays on the Work of W.V. Quine_. Dordrecht, Holland: D. Reidel, 1969.

Feigl, H. and Scriven, M., eds.. _Minnesota Studies in the Philosophy of Science_ Vol. I. Minneapolis: University of Minnesota Press, 1956.

Flew, A.G.N., ed.. Logic and Language. First Series. Oxford: Blackwell, 1963.

Frege, G. Translations from the Philosophical Writings of Gottlob Frege. Edited by P. Geach and M. Black. Oxford: Blackwell, 1970.

Hollis, M. and Lukes, S., eds.. Rationality and Relativism. Oxford: Blackwell, 1982.

Hudson, W.D., ed.. The Is-Ought Question. London: Macmillan, 1969.

Lakatos, I. and Musgrave, A., eds.. Problems in the Philosophy of Science. Amsterdam: North-Holland, 1968.

________. Criticism and the Growth of Knowledge. Cambridge: University Press, 1970.

Lewis, H.D., ed.. Contemporary British Philosophy Series IV. London: Allen and Unwin, 1976.

Linsky, L., ed.. Reference and Modality. London: Oxford University Press, 1971.

Presley, C.F., ed.. The Identity Theory of Mind. Brisbane: Queensland University Press, 1971.

Radner, M. and Winokur, S., eds.. Minnesota Studies in the Philosophy of Science. Vol. IV. Minneapolis: University of Minnesota Press, 1970.

Ryan, A., ed.. The Philosophy of Social Explanation. London: Oxford University Press, 1973.

Searle, J.R., ed.. The Philosophy of Language. London: Oxford University Press, 1971.

Suppe, F., ed.. The Structure of Scientific Theories. Urbana: University of Illinois Press, 1977.

Wilson, B.R., ed.. Rationality. Oxford: Blackwell, 1970.

Young, M.F.D., ed.. Knowledge and Control. London: Cassell and Collier Macmillan, 1971.

BOOKS.

Alston, W.P. *Philosophy of Language*. Englewood Cliffs, New Jersey: Prentice-Hall, 1964.

Austin, J.L. *Sense and Sensibilia*. Reconstructed from manuscript notes by G.J. Warnock. Oxford: Clarendon Press, 1962.

________. *How to do Things with Words*. Oxford: Clarendon Press, 1962.

Ayer, A.J. *The Foundations of Empirical Knowledge*. London: Macmillan, 1940.

Bartley, W.W. *The Retreat to Commitment*. New York: Alfred A. Knopf, 1962.

Bennett, J.F. *Rationality*. London: Routledge and Kegan Paul, 1964.

Berger, P.L. and Luckmann, T. *The Social Construction of Reality*. London: Penguin, 1967.

Carnap, R. *Meaning and Necessity*. Chicago: University of Chicago Press, 1956.

Corbett, P. *Ideologies*. London: Hutchinson, 1965.

Flew, A.G.N. *Sociology, Equality and Education*. London: Macmillan, 1976.

Geach, P.T. *Reference and Generality*. Ithaca, New York: Cornell University Press, 1962.

Gellner, E. *Words and Things*. Harmondsworth, Middlesex: Pelican, 1968.

Goodman, N. *The Structure of Appearance*. Third edition. Dordrecht, Holland: D. Reidel, 1977.

________. *The Languages of Art*. London: Oxford University Press, 1969.

________. *Ways of Worldmaking*. Hassocks: Harvester Press, 1978.

Haack, S. *Deviant Logic*. Cambridge: University Press, 1974.

Hanson, N. _Patterns of Discovery_. Cambridge: University Press, 1958.

Hare, R.M. _The Language of Morals_. Oxford: Clarendon Press, 1961.

Holland, R.F. _Against Empiricism_. Oxford: Blackwell, 1980.

Kekes, J. _A Justification of Rationality_. Albany: State University of New York Press, 1976.

Kovesi, J. _Moral Notions_. London: Routledge and Kegan Paul, 1967.

Kuhn, T.S. _The Structure of Scientific Revolutions_. Chicago: University of Chicago Press, 1970.

Mackie, J.L. _Ethics_. Harmondsworth, Middlesex: Pelican, 1977.

________. _Hume's Moral Theory_. London: Routledge and Kegan Paul, 1980.

Magee, B. _Modern British Philosophy_. St. Albans: Paladin, 1973.

Nagel, E. _The Structure of Science_. London: Routledge and Kegan Paul, 1961.

Newton-Smith, W.H. _The Rationality of Science_. Boston: Routledge and Kegan Paul, 1981.

Papineau, D. _Theory and Meaning_. Oxford: Clarendon Press, 1979.

Phillips, D.Z. and Mounce, H.O. _Moral Practices_. New York: Schocken Books, 1970.

Plamenatz, J. _Ideology_. London: Macmillan, 1971.

Popper, K. _The Open Society and its Enemies_. London: Routledge and Kegan Paul, 1945.

________. _Objective Knowledge_. Oxford: Clarendon Press, 1972.

Quine, W.V.O. _Word and Object_. Cambridge: Technology Press of the Massachusetts Institute of Technology, 1960.

________, _From a Logical Point of View_. New York: Harper and Row, 1968.

Quine, W.V.O. Ontological Relativity and Other Essays. New York: Columbia University Press, 1969.

Quinton, A. The Nature of Things. London: Routledge and Kegan Paul, 1973.

Rescher, N. The Coherence Theory of Truth. Oxford: Clarendon Press, 1973.

Rudner, R.S. The Philosophy of Social Science. Englewood Cliffs, New Jersey: Prentice-Hall, 1966.

Russell, B. An Inquiry into Meaning and Truth. London: Allen and Unwin, 1940.

Ryle, G. Dilemmas. Cambridge: University Press, 1964.

Shaffer, J.A. Philosophy of Mind. Englewood Cliffs, New Jersey: Prentice-Hall, 1968.

Salmon, N.U. Reference and Essence. Oxford: Blackwell, 1982.

Scheffler, I. Science and Subjectivity. Indianapolis: Bobbs-Merrill, 1967.

________. The Anatomy of Inquiry. New York: Alfred A. Knopf, 1967.

Searle, J.R. Speech Acts. Cambridge: University Press, 1969.

Stegmuller, W. The Structuralist View of Theories. Berlin: Springer-Verlag, 1979.

Strawson, P.F. Introduction to Logical Theory. London: Methuen, 1952.

________. Individuals. London: Methuen, 1966.

________. The Bounds of Sense. London: Methuen, 1966.

________. Logico-Linguistic Papers. London: Methuen, 1971.

Stretton, H. The Political Sciences. London: Routledge and Kegan Paul, 1969.

Toulmin, S.E. _The Uses of Argument_. Cambridge: University Press, 1964.

________. _Reason in Ethics_. Cambridge: University Press, 1968.

Trigg, R. _Reason and Commitment_. Cambridge: University Press, 1973.

Warnock, M. _Schools of Thought_. London: Faber and Faber, 1977.

Wiggins, D. _Identity and Spatio-Temporal Continuity_. Oxford: Blackwell, 1967.

Winch, P. _The Idea of a Social Science_. London: Routledge and Kegan Paul, 1958.

Wittgenstein, L. _Tractatus Logico-Philosophicus_. London: Routledge and Kegan Paul, 1961.

________. _Philosophical Investigations_. Oxford: Blackwell, 1963.