

INSTRUMENTALISM, REALISM AND  
THE OBJECT OF INQUIRY IN THEORETICAL LINGUISTICS

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## ABSTRACT

This dissertation concerns the status of theoretical constructs in linguistics. Part One addresses the question of whether these ought be interpreted instrumentally or realistically. The discussion of this question is centred around the instrumentalist/realist debate in the philosophy of science. In chapter 1, I argue for a version of realism and try to defend this metatheoretical position against instrumentalist objections.

In chapter 2, I show how this realist position applies to theory construction in linguistics; I also discuss problems encountered within the instrumentalist tradition in linguistics.

Part Two concerns what has been called the ontological question in the philosophy of linguistics, that is it addresses the question of the ontological status of the object of inquiry. I show that this question is closely linked to the sorts of problem discussed in Part One.

In chapter 3, I discuss versions of the view that the object of inquiry is psychological in nature, and thus able to be viewed as an object of psychological theory. In chapter 4, I deal with philosophies of linguistics which claim that linguistic objects are social realities. Having argued that neither of these approaches is satisfactory, I examine, in chapter 5, proposals that the object of inquiry is neither social nor psychological. I show why Platonism, as a version of this approach, is unsatisfactory. I then present a

metatheoretical position (Interactionism), which is based on the later work of Popper, particularly his notion of objective knowledge. The dissertation thus presents a coherent philosophy of linguistics which has realism and interactionism as its two principle components.

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PART ONE

REALISM AND INSTRUMENTALISM

## Introduction

My concern in part one is with the status of theoretical constructs in grammatical inquiry, particularly the question of what kind(s) of relationship they bear to the domain which they are designed to account for. The discussion centres on the instrumentalist/realist debate in the philosophy of science\* and is an attempt to see whether the issues raised in this debate shed any light on the problems associated with evaluating the status of theoretical constructs in grammatical inquiry. I argue that they do. Furthermore, I argue for a version of realism in the philosophy of theoretical linguistics and against instrumentalist interpretations of theoretical linguistic constructs.

Chapter 1 argues for a particular version of realism, and chapter 2 tackles the objections raised by instrumentalists to such a realism. Each chapter proceeds by discussing the issues in the philosophy of science and then relating these to the interpretation of constructs in theoretical linguistics

I think that a brief (and necessarily oversimplified) outline of realist and instrumentalist positions in the philosophy of science is in order at this point. A realist view of theoretical constructs in science may be stated thus: such constructs may refer to, or be descriptive of, extra-theoretical realities. In this respect, the realist may wish to distinguish the following: (1) theory construction, (2) observable phenomena or sense data and (3) a 'hidden' reality 'behind' these sense impressions. The task of the realist is then to devise,

with the aid of (2), theoretical constructs in (1) which are descriptive of the reality in (3). The realist wants to claim for the propositions expressed as sentences containing his theoretical terms, that they are true or false descriptions of reality. For a statement of various realist philosophies of science, cf Popper (1963), Lakatos (1970) and Putnam (1975). I discuss each of these below.

The instrumentalist view might be stated thus: it is a mistake to take theoretical constructs as being descriptive of some 'hidden reality' over and above the observable data. Rather, they are best seen as instruments for systematising, imposing order on, and predicting, our sense impressions (or 'observable phenomena'). Propositions expressed as sentences which contain theoretical terms are neither true nor false, since our theoretical terms are not observational terms, but are tools, and nothing more than tools, for the ordering and predicting of observations. Theoretical terms cannot be used to refer to 'real' entities; the entities which we may say they are used to refer to are fictitious objects which we devise in order to predict the phenomena.

This rather simplistic summary of the instrumentalist and realist positions should suffice to set the context for the discussion in part one. The subject matter of part two is the question of the ontological status of the object of inquiry in theoretical linguistics. I hope to show in part one that this question is closely related to the methodological concerns discussed in the first two chapters, and that the two parts



taken together constitute the basis of a coherent philosophy of theoretical linguistics.

## CHAPTER 1

## THE REALIST/INSTRUMENTALIST DEBATE

## 1.1 Realism

There are many different versions of realism in the philosophy of science; in order to distinguish between them and to define the version which I want to adopt, it is necessary to describe the different philosophical strands that usually function as components of realism. To do this, I will take as my starting point a version of realism (Harré's, as exemplified in his 1970, 1972) which contrasts with the one I will adopt. Take, for example, the following statements on the status of theoretical constructs (and their corresponding terms) in scientific inquiry:

(1) Some theoretical terms can be used to make reference (verbal) to hypothetical entities.

(2) Some hypothetical entities are candidates for existence (i.e. could be real things, qualities and processes in the world).

(3) Some candidates for existence, for reality, are demonstrable, i.e. can be indicated by some sort of gesture of pointing in the appropriate conditions.

(Harré 1972:91)

(Realism).... is the view that the statements of the theory are true or false, and that many of the entities referred to in a theory do exist. They are as much in the real world as are human beings, houses, stones, stars, and so on.

(op.cit.:90)

A number of aspects of these statements are of importance for the discussion that follows, such as Harre's conception of

demonstration and its status in his realism, the idea that we may verify, once and for all, the existence of some postulated entity or process, and the ontological problems arising from the use of the term 'real world'.

Concerning the first of these, Harré claims that demonstration provides the final incontrovertible proof of the existence of the entity demonstrated. Thus: '...if we can demonstrate a thing to which we have previously made reference, on this or other occasions, then we have proved that thing exists.'(op.cit.:90-91). Similarly: 'To be able to indicate something is the final, incontrovertible proof of its existence.'(90)

This adherence to a notion of proof by demonstration is a kind of verificationism: it is a claim that we may verify our theories, show them to be true descriptions of the world, in this case by means of ostension. It is a view of the nature of scientific inquiry which, at least in the form in which Harré states it, I do not want to adopt. Consider the act of reference which accompanies the act of indication in Harré's ostensive proof: this reference, to succeed, would require that the object referred to be observable. But there are problems with the notion that an entity is observable, and therefore capable of being pointed to. Popper (1959 and elsewhere) has often pointed out that all observation is theory-impregnated. Take the following (apparently simple) example: while it may seem rather evident that entities like fish are observable, it is clear that what will count as a fish (and therefore what will

count as a successful act of pointing out a fish) depends on our definition of the term 'fish', and this depends on the framework in which the term is defined. This framework is, of course, a set of theoretical proposals concerning such entities and is located within a more general set of theoretical assumptions about the natural world, which is in turn tied in with our theories about the physical world in general.

It is evident that our scientific theories develop and evolve, and as they do, so do the meanings of the terms we use to express them. If one considers a point in time at which our theories of the natural world classify what we now call a whale as a fish, and compare this with the present, where whales are not considered to be fish, it is clear that what has changed is our definition of 'fish', and this change comes about because of a change in our set of theoretical assumptions. Clearly, if 'fish' is defined (rather crudely) in terms of certain features of habitat (living entirely in the sea rather than on land), behaviour (being a creature which swims underwater) and form (having a tail of a certain sort) then whales can plausibly count as being fish. We can say that, with an act of ostension taking place while such a theory is accepted (and thus such a definition of 'fish'), that whales are observable and are observably (demonstrably) fish: one can point to them, as in Harre's test situation.

However, considering the case where we can now point to a whale and successfully identify it as a mammal, it is clear that a whale is demonstrably not a fish. What have changed are our

theoretical assumptions and thus our theoretical terms (such as 'mammal', 'reptile', 'amphibian', etc), and because of these, the very acts of demonstration and observation themselves. It is rather clear from this that the act of observation is rather heavily dependent on theoretical states of affairs: our theoretical assumptions enable us to engage in the act of observation, and our observations are thus heavily theory-dependent.

One of the points that Popper frequently makes in this respect is that, in setting up a scientific experiment, our theoretical assumptions will determine what will count as an observation; clearly, just any sense impression which occurs during an experiment will not necessarily be relevant. An itch in the experimentalist's elbow during an experiment on loss of heat energy in a refrigeration system might well be determined to be irrelevant to the experimental results, for instance, or the colour of the room in which the experiment will take place. And nor are such things self-evidently irrelevant: we must *decide*, on the basis of our theoretical orientation, what is to be counted as being part of the result and what is not. If we are mistaken in this, we may not achieve the success we aimed for. An itch in the experimentalist's elbow, or the colour of the room, just might turn out to be relevant to the result, and experiments have often taken place in which it turns out that some factor considered unimportant and not part of the evidence emerges as crucial for the experiment and the theory in question. In these cases, we have to revise our theoretical

proposals and our interpretation of the evidence.

Popper also suggests that our perceptual system works in this way, with certain sense impressions being foregrounded and others relegated to the background in accordance with our hypotheses (some of them very deeply embedded in our perceptual framework) as to what it is we are perceiving. Obviously, the deeper a notion is embedded in our way of viewing the world around us, the more such notions appear 'directly observable'. The notion 'physical object', for instance, is thus deeply rooted in our way of viewing the world to such an extent that we take it as self-evident that physical objects exist. Notice that this is so even when changes in our scientific theories undermine the very notion 'physical object'.

Eddington (1927) makes a great deal of this in arguing for an idealist philosophy of science, and while I do not wish to adopt such a view, I accept his point that, with a notion as quotidian as 'table' we begin to see two quite distinct 'tables' emerging as realities as *our* theories develop: one which is solid and exemplifies our standard notion of everyday, medium-sized macrophysical object (of the sort whose existence in 'the real world' Harré is so certain of), and another which consists largely of space inhabited by energies and forces. I will suggest below that we are justified in treating both of these 'tables' as inhabitants of the real world, and that we need not resort to idealism to accommodate two such distinct interpretations of what sort of thing a table is.

Consider the bearing these considerations have on Harre's

idea that we provide the final incontrovertible proof of the existence of something by successfully demonstrating it, having referred to it. If the act of demonstration does depend, in the way I have been suggesting, on our theoretical apparatus, and if that apparatus is at all subject to revision, then so are our acts of demonstration. Now, since it is evident that our theories are thus subject to revision, it is clear that our acts of demonstration are too. And if we consider whether our theories are capable of being said to be incontrovertibly true, we can decide whether the act of demonstration provides us with the incontrovertible evidence of the existence of an entity that Harré says it does.

It seems that, in our simple case of theories of the natural world, this is not so. What appeared to be demonstrably a fish at one stage in the development of our theories turned out to be demonstrably not a fish at later stage. In fact, it would be perverse to suggest that we should not build a rather large amount of fallibilism into our view of scientific theories, given that they so frequently change. And, with this in mind, it would be absurd to assume that any given theory is not subject to revision, often radical revision to the point where the theory is abandoned altogether. This sort of fallibilism is a central part of Popper's philosophy of science, and with it goes the idea that falsifiability, rather than verifiability, is the hallmark of scientific theories: rather than claiming that science provides us with theories which are incontrovertibly proven to be true, Popper emphasises the fact

that none of our theories are immune to the possibility of future falsification, even if they have held up against all testing up till now.

I am inclined to adopt Popper's view concerning fallibilism and falsification, and indeed his proposal that falsifiability be taken to function as a demarcation criterion for distinguishing between scientific and non-scientific theories. A theory which turns out to be true come what may, and for which there is, in principle, no way of devising a means of falsification, is not a scientific theory for Popper, and is in fact lacking in content because it is not susceptible to disconfirmation. That is, there must be states of affairs which are ruled out as impossible by a theory, such that if we were able to show that such a state of affairs held, we could show the theory to be falsified. If one imagines a theory for which no such state of affairs were describable, then it is clear that it excludes nothing, and will be true no matter what. The content of such a theory is minimal: it excludes nothing, and is therefore uninformative\*.

This sort of falsificationism is quite distinct from Harre's verificationism and is, I think, a closer characterisation of how scientific theories develop, and the provisional status which they enjoy. One may argue, of course, that a theory which is

\* Note that Popper's demarcation criterion is not a criterion of meaningfulness. Unlike the logical positivists (cf 2.2), Popper has never proposed that non-scientific theories, theories which are unfalsifiable, are meaningless. For more on this and on the relationship between scientific and metaphysical statements, cf chapter 2.



falsifiable, but which we have consistently failed to falsify should be considered a true theory. Popper refers to this failure to falsify a theory as the corroboration of the theory; this does not, however, amount to saying that the theory is true. Rather, one takes it to be our present best guess at what the object of inquiry is like, but a conjecture which is still open to refutation.

In this sort of conjectural realism, one allows (contra Harré) that theories may be said to be false, but not that they may be said to be true. However, there is a notion of *convergence upon the truth* in Popper's philosophy of science. That is, Popper allows that our theories may possess greater or lesser degrees of what he calls verisimilitude: through the process of falsification, we develop theories which come closer and closer to a true description of their object of inquiry, even though we can never claim that they are incontrovertibly true descriptions. Thus, the notion of truth is still present in Popper's philosophy, but in a form which is considerably weaker than Harré's. The conception of falsification which Popper uses is a very strong one, however, and is a very central part of his realism. We shall see below that Popper's falsificationism needs to be weakened in the face of what we know about the historical development of scientific theories. This will leave us with a version of realism which is even more fallibilistic than Popper's.

I should deal with a couple of possible objections to Popper's proposals at this point. One of them is that Popper,

in so emphasising the theory-dependence of observation, might be said to be denying the possibility that there is a theory-external world to be described by our theories. Popper's position is that there is indeed a theory-external world, and this is mirrored in his adoption of a correspondence theory of truth (cf Popper 1959:274). In fact, this is a central part of his realism (for discussion of the problems in maintaining it, cf 2.1). It has to be said that the claim that there is a theory-external world is a metaphysical one. But then, so is the the claim to the contrary. Such metaphysical positions are an unavoidable part of the process of constructing and evaluating scientific theories. Just how one should decide between such competing metaphysical assumptions is a philosophical matter; I try to show, particularly in 2.1, that the assumption that there is a theory-external world is coherent in that it allows us to understand why it is that our theories should have varying degrees of success or failure. I postpone discussion of this for the moment, however.

Another objection to Popper's convergent realism is this: that there is surely some kind of continuity between the earlier and later meanings of terms in our theories. That is, to return to the 'fish' example, there are surely certain core semantic notions which link our earlier and later conceptions of what is to count as a fish. What has happened is that we have simply refined our fundamental definition of 'fish': the fundamental meaning remains the same from original to successor theory. An even stronger version of this objection would claim that it is

the essential definition that remains unchanged, and that to define an entity is to isolate its essential properties.

This second objection is an interesting one. It certainly seems as if much of the original definition of fish (which included whales) is unchanged in our current definition. This is an entirely post-hoc assumption, however. There is no way of knowing in advance which aspects of our theories, and thus our definitions, will change, and it is only from the perspective of the later theory and definition that we establish what we take the essential or central components of the definition to be. Thus essentialist realism (or essentialism), the view that we arrive at a definition of the essence of an entity via our theories, does not reflect the provisional nature of theory construction and refutation. With the adoption of Popper's version of realism, we move away from the idea that we can incontrovertibly establish what the properties of the external world are.

This relates to the fact that single terms do not change their meaning independently of the entire conceptual system within which they are located: as the entire network of theoretical assumptions changes, so do the single terms embedded within them, and since we cannot tell just how our theories will change, so we cannot tell which aspects of a definition will count as the central ones and which not. This point bears very closely on the question of falsification: it is arguable (and I am convinced of the argument) that single hypotheses alone are not susceptible of disconfirmation (cf 1.2 for discussion of this

argument, often referred to as the Duhem-Quine Thesis), since a given hypothesis, and the terms contained in it, gain their meaning from an entire network of theoretical assumptions\*. This means that, in attempting to falsify a single hypothesis, we attempt to falsify the set of assumptions underlying it. In my example, it means that our definitions of 'fish' and 'mammal', and much of our theoretical apparatus concerning the natural world, are subject to revision, and that incontrovertible proof of existence is therefore not available to us.

There are other aspects of Harré's claims about incontrovertible proof of existence via demonstration which are worrying, and these involve considerations which are even more radical (in terms of the sort of realism they allow us) than Popper's arguments. It is arguable, for instance, that aside from the problems concerning observability and fallibilism which I have been discussing, Harré's notion of pointing to the object of reference misrepresents the act of reference. Putnam (1960) makes an interesting comment on this matter, namely that any statement of even the most observational sort will necessarily contain terms which are informal in nature, and not quite explicitly defined. This suggests something like referential 'open-endedness' for our theoretical terms, and also suggests that ostension is not the sine qua non for the act

\* We will see below (2.1) that this is in fact taken as an argument against the sort of realism I am proposing to adopt. However, I will argue that it is a point in favour of, rather than against, realism.

of reference that Harré supposes it to be.

In this regard, Boyd (1979) makes a comment regarding the nature of reference in science which I think is important. It concerns what he refers to as 'theory-constitutive' metaphors and the 'implicit' reference that they allow us:

'There exists an important class of metaphors which play an important role in the development and articulation of theories in science.....They are used to introduce theoretical terminology where none previously existed...their success depends on their 'open-endedness', i.e. they do not convey quite specific respects of similarity and analogy....Theory-constitutive metaphors, when they refer, refer implicitly, in the sense that they do not correspond to explicit definitions of their referents, but instead indicate a research direction toward them. The same thing is apparently true of theoretical terms in science generally.'

(363)

Boyd claims that this use of metaphor is a device for 'accomodating our linguistic categories to the causal structure of the world so that they 'cut the world at its joints'.'(358). Thus the reference of a general term is 'its role in making possible socially coordinated epistemic access' to a particular sort of thing or natural phenomenon. Ostension, under this sort of view, is still embedded in a realist philosophy of science, but is defined in a sufficiently sophisticated way to allow for the complexity of theory change and the resultant changing meaning of our theoretical terms.

Under a sufficiently sophisticated realism, we can allow that there is no such thing as final, incontrovertible proof without abandoning the central realist notion that there is an extra-theoretical world which our theories are designed to describe.

Consider how far we have now come from Harre's version of realism: ostension and the act of reference are very much theory-dependent, and if we allow that our theories are subject to radical revision, then we allow that the acts of ostension and reference are too. Further, if we allow for referential open-endedness, we are even further from being able to claim that we can provide final, incontrovertible proof of the existence of an entity. And this has fairly major consequences for Harre's notion 'the real world', since what counts as the real world and its inhabitants is seen to be theory-dependent, open, and evolving. But if we take it that it is the properties of a theory-external world that induce the results of our tests then we can maintain the realist idea that we are attempting to describe the properties of a world outside of our theories.

In my brief summary of the realist position in the introduction, I said that realism was concerned with describing a reality which induces our sense impressions, and that the realist takes scientific hypotheses to be true or false in relation to this reality. Indeed, this is what Harre claims. We have now begun to see what the various components of the realist position are, and what the issues are on which realists and non-realists construct their philosophies of science. I have mentioned the question of ostension and of reference with regard to theoretical terms, and the problems connected with observability in relation to theory. And I have tried to discuss the question of the possibility of a theory-external world in relation to Harre's claims about the 'real world'.

There is undoubtedly much room for manoeuvre on the part of the realist when it comes to reacting to these various issues. I have indicated that one can still maintain a realist position while abandoning Harré's claim that we may prove the existence of an entity incontrovertibly. In response to Harré's verificationist realism, I stressed the Popperian notion of falsificationist realism. Thus, rather than saying that hypotheses are taken to be either true or false by the realist, I accept Popper's position which incorporates a considerably weakened role for the notion of the truth of a theory. However, I now want to consider weakening the role played by the notion of the falsity of a theory.

Lakatos (1978) has suggested that, given an inconsistent set of scientific statements (that is a set in which there is some sort of clash or contradiction between the members of the set) one must select from among them the following: (i) a theory under test and then (ii) an accepted 'basic statement'\*, leaving the rest of the set of statements to serve as background knowledge against which a test will occur (these are the

\* 'Basic statements' is an expression used by Popper (1959) to cover what might be referred to as observational statements in a philosophy of science which proposes a distinction between observation and theory language. It reflects Popper's views on the theory-impregnated nature of observation in that he does not assume that statements of the form 'The oil is floating on the water' are theory-free. Popper allows that there is a large conventional element in such statements, and that they do not represent the sort of hard core observational knowledge which instrumentalists such as the Logical Positivists assumed they did.

interconnected theoretical assumptions which I have said are present as an integral part of the context in which a single hypothesis is tested).

Lakatos suggests a version of falsificationism which allows for a weakened role for falsification as follows: we allow that any part of the entire body of knowledge be amended in the light of a clash or inconsistency, such that the apparently falsified theory may be allowed to stand, even in the face of the conflicting basic statement. What this means is that we do not take falsification to be final and incontrovertible any more than we took verification to be.

That is, since our theories are subject to radical revision, we allow that (a) what we now see as a falsification might be revised in the light of future theory development and (b) what counts as a clash between 'observation' (basic statements) and theory can be accommodated by means of adjustments elsewhere in the theoretical system.

There are several elements in this version of a falsificationist realism that might appear to undermine the very basis of realism. The non-realist might respond for instance, by claiming that in allowing for the heavily theory-dependent nature of both falsification and verification, we are close to abandoning the idea that scientific theories are true or false descriptions of reality. And in abandoning this, we abandon the very core of realism, since it is precisely this claim that the instrumentalist objects to.

The most appropriate response to this instrumentalist



reaction (and there is a wide range of such reactions to realism: cf 2.1) is to insist that the most important factor in deciding whether a basic statement should be lead us to abandon a theory is the question of whether adjustments elsewhere in the system would result in a greater or lesser degree of empirical yield. That is, if the adjustment to the system is 'progressive', to use Lakatos' term, and allows us to predict novel facts, then such an adjustment is preferable to an abandonment of the theory. Unfortunately, this response, which strikes me as being a rational and justifiable one, is not sufficient to pacify the instrumenatlist, since he need only claim that greater degrees of empirical yield are all that the instrumenatlist requires of a theory, and that the notion of a reality which our theoretical constructs correspond to is superfluous to an understanding of the growth of scientific knowledge.

As I point out in 2.1, the realist can respond to this that if we do not assume the existence of a theory-external world, there is no explanation for the greater success of some theories over others. In adopting the instrumentalist's position, we simply accept the variation in the empirical yield of theories without knowing, or asking, why there should be such variation. I also suggest there that this leads to heuristic complacency.

## 1.2 The instrumentalist case against realism

The previous section ends with a version of realism (which could be called conjectural realism) based largely (though with refinements) on Popper's proposals concerning falsification and fallibilism, corroboration and verisimilitude. Here, I present some of the principal instrumentalist arguments against a realistic interpretation of scientific theories, and try to indicate how the realist might respond to them.

Most of the instrumentalist's objections to realist interpretations of theories centre on the relationship between 'observation' and theory. It is felt by instrumentalists of most persuasions that science is best characterised as an activity that values observability ('the observable facts') highly, and this is probably a view that would gain widespread 'commonsense' support. On the face of it, this certainly seems to be a fair attempt at saying what science is all about. The dissatisfaction that many instrumentalists feel with theoretical constructs concerns the distinction between these and the observable facts, whereby it is getting the facts right that counts, and theoretical constructs are no more than our means of achieving this. Any attempt to elevate theoretical constructs such that we claim that they refer to extra-theoretical entities over and above the data is regarded as overstepping the mark, and in particular, any metaphysical content in scientific theories is regarded with suspicion.

These sorts of notion underlie, in the late nineteenth century, the work of Mach, in early twentieth century philosophy of science, that of Duhem and Poincarre and, in the thirties, logical positivism. Nor have such views on the nature of scientific theories ceased since the demise of logical positivism; the anti-realist lobby is as strong today as ever (cf van Fraassen's 'constructive empiricism' in his 1980 and the Dummettian anti-realist lobby in the theory of language reported in Dummett 1976, 1978, Luntley 1982, and elsewhere).

As one might expect, this emphasis on observation rather than theory is often accompanied by an attempt to provide a strict delimitation between observation language and theoretical language (this is especially so of logical positivism). However, since I have argued in the previous chapter that such a delimitation is difficult to make, precisely because it is methodologically ill-conceived, I will concentrate here on what I take to be the strongest aspects of the 'instrumentalist challenge to realism, rather than on the observation/theory division, which is probably one of the weakest points in the range of instrumentalist proposals. I do return to the observation/theory dichotomy in my discussion of logical positivism, in relation to instrumentalism in linguistics (in 2.2).

In order to get at the heart of this debate, it is necessary to unpack the notion 'instrumentalism', and examine the cluster of issues that have divided realists and

instrumentalists. Once this is done, it becomes less important whether a given philosopher is regarded as belonging to one camp rather than another. Rather, what does matter is where different philosophers stand on the various issues. (Thus such questions as whether Duhem was 'really' a realist and whether Dummett is an instrumentalist are seen to be less important than where Duhem and Dummett stand on the core issues). In order to make some inroads into the relevant issues, I consider them under two main headings: 'transcendence of data by theory' and 'overthrow of theories'; under each heading, several distinct but related issues are presented and discussed in relation to realism.

(i) Transcendence of data by theory.

One point made about the relationship between theories and 'the facts' is as follows: for any given empirical domain, there will be more than one possible theory consonant with the facts. This should suggest to us that we ought to be rather wary about the claims we make for the constructs in a given theory (such as interpreting them as descriptions of an underlying reality behind the phenomena). Consider the following statements by Duhem:

'When these hypotheses have enabled us to decompose the complex movements of the planets into simpler ones, we should not think that we have now come upon the real movements that lie behind the apparent ones. The real movements are the apparent ones. The end achieved is more modest: we have simply made the celestial phenomena accessible to calculation.'

(1908/1969: 20)

'Since the astronomer's hypotheses are not realities but merely fictions, the whole purpose of which is to save

appearances, we should not be surprised that different astronomers attempt to achieve this purpose by means of different hypotheses.'

(op. cit. : 22)

'...the hypotheses of physics are mere mathematical contrivances for the purpose of saving the phenomena.'

(op. cit. :112)

That is, the fact that theories transcend the data, or put another way, theories are underdetermined by the data, is taken as a reason for making fairly modest claims about the status of theoretical constructs. Quine is frequently cited in this respect too; according to Quine(1953), our theoretical constructs (including those referring to physical objects) are to taken to be 'myths':

'Viewed from within the phenomenalist conceptual scheme, the ontologies of physical objects and mathematical objects are myths. The quality of myth, however, is relative; relative, in this case, to the epistemological point of view. This point of view is one among various, corresponding to one among our various interests and purposes.'

(1953: 19)

There are several related views connected with this. One is the pragmatist position that our theories are best evaluated in relation to our purposes rather than in relation to their correspondence , or degree of correspondence, to an extra-theoretical reality. This pragmatic strand is common among instrumentalists, and can be easily detected in the above quotations from Duhem and Quine. It is explicitly spelled out in the following statement by Quine:

'Our standard for appraising basic changes of conceptual scheme must be, not of a realistic standard of correspondence to reality, but a pragmatic standard.'

(1953: 79)

The two notions, theories as myths, and evaluation of theories in purely pragmatic terms, are frequently linked, thus:

'the myth of physical objects is epistemologically superior to most in that it has proved more efficacious than other myths as a device for working a manageable structure into the flux of experience.'

(Quine, op cit : 44)

Because theories are thus underdetermined by data, it is therefore possible, for any given theory, to construct an alternative, logically distinct theory which entails the same body of data. This is often referred to as the Underdetermination Thesis, and is closely linked with the Duhem-Quine Thesis, which can be stated thus: any theory can be protected from falsification, when faced with contradicting evidence, by means of adjustments to some part (or parts) of the theory, or to the background knowledge within which it is located .

One of Quine's points in this respect is that single hypotheses themselves are not susceptible to disconfirmation, but only theories as wholes; thus a single consequence of a theory, when in conflict with experience, can be saved by adjustments to the theory. Quine (1953 and elsewhere) also distinguishes between experiences which are on the 'periphery' of our theoretical scheme of things and those closer to the centre, so that ordinary everyday macrophysical objects such as 'table' are apparently observable, owing to their being instances of 'physical object', a notion so central to our conceptual scheme as not to appear theoretical at all. Note that

this way of dealing with the difference between observation language and theory language is distinct from the logical positivist attempt (as in Carnap's Logical Syntax) to distinguish sharply between observation and theory. The principal problem with it is that, while it does allow that even terms like 'table' are theoretical, i.e. are part of our conceptual scheme, it creates a very vague scale of theoreticity, where it isn't clear how we go about determining the degree to which a term is theoretical. It is similar in this respect to Lakatos' distinction between the 'hard core' and the 'protective belt' of theories.

How should the realist respond to these points, and to what extent do they seriously undermine realism? Regarding the interpretation of theories as myths, it need not concern the fallibilist realist that this is so. Under Popper's version of realism, many of our scientific theories do in fact start out as myths, but the important distinction between non-scientific myths and scientific theories is that the latter are testable (i.e. falsifiable); Popper is quite happy to allow that our scientific theories are embedded in 'metaphysical research programmes' which are a kind of general picture of the world, much as myths are\*.

\* The point that our theories are thus embedded is stressed by those, such as Boyd and Hesse, who view the background picture as a kind of metaphor; Boyd goes further in taking theories themselves to be a special kind of metaphor which allows us 'epistemic access' of the Quinean sort (cf his 1979), but adds to this a rather strong realist claim that such metaphors then 'cut the world at its joints', a position which reflects his belief that realism is 'an empirical hypothesis'.

Of course, Quine allows that our scientific 'myths' are epistemically superior in this way, but does not stress that in thus being superior, they allow us something new, namely access to the world, knowledge of it, and progress in developing that knowledge.

The crucial point here is that at which the realist decides that it is more likely that there are physical objects in the world than it is that there are Homeric gods, but for Quine, this is going to far: all we are warranted in claiming is that the former is an epistemically superior myth, and therefore to be believed in more readily than the latter. It is heuristic fruitfulness, of the sort whereby a given construct really does allow us progress, which is the major factor in favour of realism; of course, the instrumentalist takes just this fertility to be one of his major criteria for assessing theories. However, the realist can respond with the following: it is hardly likely to be mere chance that one particular construct/set of constructs is more fertile than another. For the instrumentalist, there is nothing to be said about WHY one myth is superior to another, whereas the realist gives an account of the success of particular constructs: they are fair approximations at how the world is organised.

Regarding the pragmatic means of assessing theories, this is at best trivially valid, at worst, inaccurate. Quine's claim that we should appraise changes in theoretical framework from a pragmatic RATHER THAN from a standard of correspondence to reality becomes empty if our pragmatic goal, our purpose, is to



approximate, through our theories, the structure of the world, that is, if it is a realist purpose (and often it is). Duhem's response to this was that, even if our successful theories ARE constructed because of a realist attempt to describe the world, we should not confuse the motivation for a theory with its degree of success. In Duhem's words, we must distinguish between 'the chimerical hopes that have incited admirable discoveries' and the notion that such discoveries 'embody the chimeras that gave birth to them'(1906: 32). However, this again undervalues HEURISTIC factors: if a realist approach gives rise to progress, this suggests that it is the right approach, just as a given cluster of theoretical constructs which gives rise to success should be viewed as being on the right lines. Duhem's attempt to weaken the heuristic value of a realist approach is open to the following response: what can we do to validate a particular metatheoretical approach if not point to its success in research and discovery?

It should also be noted that pragmatism of this sort easily leads to the worst relativistic excesses of the Feyerabendian sort whereby there is nothing to choose between one way of describing the world and another, and where differences in conceptual scheme are simply reflections of different cultural outlooks. Kuhn (1970) is rather guilty of this too\* ; the

\* To be fair to Kuhn, he has modified the extremely relativistic stance that seemed so salient in his Structure of Scientific Revolutions and has proposed (Kuhn 1976) a 'non-paradigmatic' rationality in an attempt to avoid the claim that there simply is no rational justification for scientific theories.

principal objection to it is Popper's, namely that it denies the objectivity of science by viewing it as little more than a manifestation of social, personal and political outlook. But the difference between our current view of the world and that which incorporates Homeric gods is not just a matter of different cultures choosing different conceptual schemes: our framework has allowed us knowledge, and more importantly, progress, in a way which the panoply of Homeric gods could never have done; we are justified in claiming that we know more about our world via our scientific theories than was, or could be, known via the postulating of Homeric deities.

If 'theories as myths' and pragmatism are not worrying threats for the realist, and I do not think they are, the Underdetermination Thesis is often taken to be so. It has led realists such as Boyd (1973) to deny the possibility of underdetermination, and Worrall (1982), in accepting its possibility, to retreat to a very minimal realist position. It has induced Newton-Smith to adopt a position which he says is not realist in any currently understood sense, since, in his view, the possibility of underdetermination seriously undermines the realist position (cf his 1978).

In discussing the underdetermination thesis, it is as well to follow Newton-Smith (1978) and distinguish between a weak and a strong interpretation. The strong interpretation (Quine's version) is that all theories necessarily are underdetermined by the data, such that for any theory, there will always be an alternative, logically distinct theory, which entails the same

body of data. The weak interpretation states that theories can be, but are not necessarily always so underdetermined. In addition to this, one can make more progress with the underdetermination thesis if one distinguishes, along the lines suggested by Worrall(1978) between the general sense in which theory is underdetermined by data, and those cases where there does seem to be an insurmountable difficulty in choosing between alternative theoretical accounts of the same set of phenomena. The general sense in which theories are underdetermined by data is, as Worrall points out, a trivial consequence of the transcendent nature of our theories. I take it to be a feature of our theories which supports realism, since it reverses the inductivist notion that observation is methodologically privileged with respect to theory.

It is also interesting to note that in describing two theories as equally warranted by the data, the instrumentalist must consider that mere 'alignment' with the data is insufficient to allow us to conclude that two theories are empirically equivalent: it is clear that considerations such as simplicity and unity or coherence internal to a theory will also guide us in judging one to be better warranted by the data than another (as both Duhem and Quine would allow). For the realist, it is the latter which is the best candidate as our present best guess.

Having said this, we have considerably reduced the range of cases where there could be said to be two logically distinct but empirically equivalent theories. However, there is a more

worrying case, where there does seem to be a ready translation from one theory to a logically distinct alternative one, which does seem to be worrying for the realist: in this circumstance, it would appear that either account of the world is possible, and that the realist must either illogically accept one to the exclusion of the other or abandon his realism.

There are two responses the realist can make to these points: one is that it is not normally the case that with two extant theories of the same phenomena, there does exist a straightforward translation algorithm, and that the realist is not therefore normally faced with such a situation; another is that in the cases where there really is such a direct translation, we simply do not have the means for deciding between the two rival theories, and that this situation in turn should be taken to indicate that we need to discover more, via development of existing theories, which would make ready translatability impossible, and a choice possible. This suggests, again, that the realist position, as Feyerabend (1964) points out, is heuristically fertile in a way that the instrumentalist position is not: when faced with such a ready translatability, where there appears to be nothing to choose between two competing theories, the instrumentalist would simply accept the situation (this would appear to be Duhem's response, if one considers his comments above). The realist, however, finds it unacceptable, and this spurs him on to richer development of the theories in question, and the possibility of discovery resulting from this.

The Underdetermination Thesis, then, is not as worrying to the realist as it may seem, but the Duhem-Quine Thesis is a major worry, for several reasons, the principal one being this: if a major component of instrumentalism is the claim that theories are neither true nor false, and if, as falsificationist realists, we have abandoned any simple verificationism, whereby theories can be said to be undubitably true, then it is essential that falsification is possible, otherwise, we end up accepting that our theories can neither be verified nor falsified. To accept this is to abandon realism altogether and allow that our theories are indeed neither true nor false.

The realist must deal with one of the points on which Duhem and Quine express the same view, namely the possibility of testing single hypotheses. Both point out that a given prediction (upon which one might devise a crucial experiment) is usually based on several assumptions which are either internal to the theory or are part of the assumed background knowledge within which the theory is located (part of the 'metaphysical research programme' in Popper's terms). It is from a combination of these factors that consequences of the theory are deduced, and therefore any clash between the consequences so deduced and experimental results does not constitute a direct falsification of a single hypothesis, but a problem for the theory and its background knowledge taken as a whole. Thus disconfirmation can be avoided by suitable adjustments to either the theory and/or its background knowledge.

In order to assess the degree of difficulty created for the

realist, it is important to distinguish between various versions of the Duhem-Quine thesis. A very strong interpretation, that which Quine appears to be making (though see the reservations expressed in his reply to Grunbaum, in Harding 1976) is that auxiliary devices are always available when such a clash between a theory and experimental results occurs. As Grunbaum notes, the onus is on the proponents of such an interpretation to provide a demonstration that for any set of data deduced from a specific hypothesis (the 'target' hypothesis), there will be a set of nontrivial auxiliary hypotheses which, together with the 'target' hypothesis, will guarantee the same set of results. It is also important to stress the importance of the nontriviality requirement here: if all Quine is saying is that there will be some set of auxiliary hypotheses, no matter how ad hoc and inelegant, then this is rather a trivial remark and certainly not a matter for concern for the realist. Grunbaum points out that, because it cannot be guaranteed that such a saving set of hypotheses will exist in any given case, this strong version of the Duhem-Quine thesis is a logical non-sequitur: from the occasional inconclusiveness of crucial experiments, Duhem assumes inconclusiveness to be the rule.

It seems clear that this strong version of the thesis is not especially interesting. Consider, however, the weaker claim that we may in fact find that an appropriate set of saving hypotheses is available in some cases, and that falsification becomes impossible under those circumstances, and following from this, that we cannot ever be certain that such a set of devices

is not available in any given case. This seems a valid enough point. However, I do not think that it entirely undermines the sort of falsificationist realism adopted by Popper, though it does force some sort of retreat from the strongest versions of falsificationism (the sort Lakatos 1970 refers to as 'naive', as distinct from his weaker 'sophisticated' version). Popper allows that single hypotheses are tied both to the internal structure of the theory and to the background data in the way indicated, and he also accepts that there is a strong conventionalist element in our background knowledge, which is assumed, but not accepted as confirmed, or even probable. All of this is part of Popper's fallibilism, in which we cannot be sure of the foundations upon which our theoretical frameworks are built, but we can nonetheless get falsifiable theories out of this construction and thus make progress; furthermore, we may be able to pinpoint the part of a theoretical network which is responsible for the collision with experimental results.

The Duhem-Quine thesis does not, of course, amount to a claim that entire theories are unfalsifiable, rather it asserts that only whole theories, and not specific hypotheses are falsifiable. Thus, falsification, even if only of entire theories, is still possible. The falsificationist realist also has open to him the sort of avenue explored by Lakatos (1970), whereby, even if we accept that 'instant' falsification of the sort assumed by the naive falsificationist is rarely possible, falsification is indeed possible in terms of 'progressive' as opposed to 'degenerating' problem-shifts, where the odds against

a particular theory mount as attempts to save it become not only excessively ad hoc and inelegant, but decline into lower and lower degrees of empirical yield. Once we get to this stage, of course, we are dealing with a very much modified version of realism, but it is one which is nonetheless workable.

Realism, then, is able to survive the set of instrumentalist challenges which I have cited under the general heading 'transcendence of data by theory'; however, these do not exhaust the range of possible serious objections to realism, the remainder of which I have grouped together under the heading 'overthrow of theories'. Furthermore, both the underdetermination thesis and the Duhem-Quine thesis re-emerge as being equally problematical when one comes to consider the development of theories and the manner in which one theoretical framework gives way to another.

(ii) Overthrow of theories

Major discontinuities in scientific theories seem to lend credence to the instrumentalist claim that theories come and go, but that what matters is that there is a steady build up of empirical results which constitutes real progress. Attached to this is the notion that the real descriptive part of a superseded theory lives on when old theories are replaced. The instrumentalist can claim that the realist is faced with insurmountable difficulties in trying to show that there is either (a) semantic continuity from one theory to its successor



or (b) any sort of convergence or approximation towards the truth, in the sense of Popper's verisimilitude. If the instrumentalist is right about this, then it does look as though we have either discrete changes from one theoretical framework to another, and only a build up in the range of phenomena dealt with constituting progress, or old theories changing like shifting networks, almost independently of the data. Either way, our theories begin to look like dispensible instruments for achieving greater degrees of empirical success.

Both the Duhem-Quine Thesis and the Underdetermination Thesis are relevant here too: the idea that we can accomodate recalcitrant experiences by means of adjustments to the system supports the general picture of our theories as shifting networks which are only tenuously connected to the phenomena we want them to account for; this picture of theory development strengthens the notion that they be treated as dispensible instruments. And the Underdetermination Thesis would lead us to expect radical discontinuities as the norm: they would follow from the fact that it is always possible to construct a logically distinct alternative theory for any given range of phenomena.

I have argued that realism can be saved from the most worrying aspects of these two theses, but how is the realist to deal with the arguments about semantic continuity and convergence? Regarding the notion that the semantic core of a theory is transmitted to successor theories, I argued in the

previous chapter that Harre's verificationism was untenable because we could only be certain that some part of the meaning of theoretical terms would be thus transmitted, but not which part, or how much. This argument against verificationist realism rather rebounds on the falsificationist realist, however. If we allow for such a heavy dose of fallibilism, we begin to concede to the instrumentalist that our theoretical constructs are divorced from the data in a rather disturbing way. Our theories begin to look like shifting networks of constructs whose internal changes, the changes in the meaning of its terms, are triggered by the data, but in a very indirect way, and in a manner such that we cannot point to any stable part of the scheme.

One response to this is to claim that there is indeed some kind of semantic continuity in the form of the semantic core of a theory (cf Lakatos 1970 for this sort of suggestion) which, it is claimed, will remain stable from one theory to its successor. The principal objection to this is that we cannot identify what the core is on anything other than a post-hoc basis; it is only once we actually have the successor theory that we can outline the aspects of the meaning of its terms which are shared with its predecessor. It would be impossible to predict in advance what the shared elements might be. Thus for example, what seems to the Einsteinians to be the core of Newtonian mechanics is unlikely to have appeared thus to the Newtonians.

This seems to lend credence to Feyerabend's (1975) notion of the 'incommensurability' of theories: semantic continuity

from a given theory to its superseding theory is impossible, such that a term like 'electron' in one theory of the structure of the atom has neither the same sense nor the same reference as the same term in a later theory. Putnam's (1975, 1982 and elsewhere) response to this is to appeal to what he calls the Principle of Charity (or Principle of the Benefit of the Doubt): scientific terms are not synonymous with descriptions; a term such as Bohr's 'electron' may thus have a different description from our current term of the same name, but the two are 'approximately' about the same thing. Thus, both Bohr's term and ours refer.

However, one must then beware of claiming that any theoretical construct, once postulated, refers. We must allow that some terms are abandoned because their function in theories causes complexity and failure to account elegantly for the phenomena, and this is as good a reason as any for saying that they do not refer. How then does the realist account for terms like 'phlogiston' which, he claims, have never referred? Putnam's proposal is that, for the Principle of the Benefit of the Doubt to operate, other factors must be in place, such as our capacity to account for the same range of phenomena dealt with under the previous theory. Since such factors were not in place when phlogiston theory was superseded, and the account given of the phenomena by 'phlogiston' did not live on as a limiting case in the subsequent theory, we can conclude that the term did not then and does not now refer. In this account, heuristic and methodological criteria for decisions about the status of

our constructs are stressed, and this, I think, is the right approach.

These considerations bear directly on the second of the two problems in question, namely the difficulty the realist has in maintaining that science is convergent upon the truth, that we achieve progress by means of greater and greater degrees of approximation to the truth via the process of falsification (Popper's notion of verisimilitude and its concomitant notion 'corroboration'). If we do face this difficulty in maintaining that there is some sort of semantic continuity between successive theories, how can we sustain the idea that our theories gradually allow us to build up an increasingly faithful picture of how the world is? The most extreme response that I know of to this is Worrall's (1982) in which he abandons verisimilitude altogether and concedes that our superseded theories are not approximately true, but plain false. This leaves him with an absolutely minimal version of realism: we cannot know that our theories are true, nor can we claim that they approximate a correct account of the structure of the world via the process of falsification.

I do not think that we need go as far as Worrall does in responding to the problem of convergence. We can make a fair amount of headway in retaining convergent realism if we adopt something like Boyd's (1973) version of approximation to the truth, which runs along the following lines. Boyd makes the (rather extreme) claim that realism should be taken to be an EMPIRICAL HYPO THESIS, and furthermore, one which is true, and

which has explanatory force in accounting for why scientists (who he says are realists by virtue of their practice) behave as they do and why science succeeds. Thus, the idea that science approximates an objective truth is seen as an empirical hypothesis supported by the facts of scientific practice\*. I have suggested that there is indeed something explanatory in the realist position in that it does allow us to give an account of why our theories succeed, though, unlike Boyd, I take this to be a factor in favour of realism as a methodological stance rather than as an empirical hypothesis. However, the point remains that if we can maintain a version of a theory of correspondence between our theories and the world, and if we suggest that it is some kind of approximation to the structure of the world that allows our theories to succeed, then the notion of verisimilitude is salvageable, and with it semantic continuity.

The means of retaining continuity and convergence are, like the realist arguments against underdetermination, methodological, and heuristic in particular. And there is one further such argument against instrumentalism, relating to idealisation. It has been taken by some (eg Duhem) to be an argument for instrumentalism that our theories possess a precision that is not mirrored in the phenomena they account

\* It has been pointed out to me (by J.R. Hurford) that if metascientific propositions are empirical hypotheses, and therefore falsifiable, then if we falsify the hypothesis that scientific realism is an empirical hypothesis, we have falsified a hypothesis which, by virtue of its being false, is not falsifiable.

for, and that we must therefore see them as idealised fictions which are not to be confused with the real observable data. This strikes me as being, along with the proposed distinction between observation and theory language, one of the weakest instrumentalist claims; it is easily turned to the realist's advantage: not only are our theories thus idealised, but they must be. It is not clear that we could gain any knowledge of our world otherwise, and if idealisation allows us progress in the way it does, this can be taken to suggest rather strongly that our idealised theoretical constructs are indeed a fair guess at the structure of the world which induces the phenomena. Again, there is no other way of accounting for the success of our theories other than by adopting this realist position, the principal warrant for which is heuristic fertility.

Itkonen's (1983:129) objection to this, that there is no non-circular definition of success, that the success of an activity depends on its purpose, and thus the criterion for success changes with changing purposes, is not something the realist need worry about. It reflects just the sort of relativism that one would expect in a philosophy of science, such as those proposed by Kuhn and Feyerabend, which overemphasises the social aspect of scientific activity. Contra Itkonen and Feyerabend, I take it as rather evident that Western science is more successful than African witchcraft; one need only establish what the witchcraft practitioner himself wants to count as success (let's say in the realms of physical healing or weather prediction) to see that Western science is usually

more successful ON THEIR TERMS as well as on ours (why should Itkonen assume that these are so different anyway? It seems fairly obvious what any culture will want to count as success in the way of dealing with, say healing and weather prediction). Note the pragmatist element in Itkonen's view, one which I criticised as trivially self-evident in my discussion of the pragmatic strand in Quine's position. It is clear that if we take a position such as Itkonen's seriously, then the entirety of the framework of rational inquiry descends into a rather meaningless relativism, which is precisely what Feyerabend(1975) proposes, of course. All that social relativism amounts to is the trivially true claim that ANY framework will work better for us than none at all; that does nothing to impugn the fact that some frameworks are clearly better than others, and that we set ourselves the task of selecting from among the available ones and improving those we have.

This central theme of stressing heuristic fertility and the desire to know why some frameworks are more successful than others runs through all of the realist's responses to instrumentalism, and is particularly relevant when it comes to assessing instrumentalist trends in the interpretation of linguistic theories, which I consider in the following chapter.

## CHAPTER 2

## A REALIST PHILOSOPHY OF LINGUISTICS

## 2.1 The realistic interpretation of linguistic theories

Having considered the general issues connected with the particular version of realism I want to adopt, I turn now to the question of how it applies to theory construction in linguistics. My aim will be to stick to the methodology of theoretical linguistics, though I will inevitably refer to those characteristics of the methodology of related disciplines (such as psycho- and socio-linguistics) which I consider to be methodologically distinct from theoretical linguistics.

It is interesting to consider Chomsky's remarks concerning the question of the validity of a realist position on the interpretation of theories in linguistics. That he would describe himself as a realist is self-evident, but what is interesting is the question of what this amounts to for Chomsky. He frequently cites theory construction in physics as the model upon which theories in linguistics are tested and developed, and assumes that realism is the norm in the philosophy of physics. Thus, arguing against the adoption of an instrumentalist philosophy of linguistics, he says:

'...to say that linguistics is the study of introspective judgements would be like saying that physics is the study of meter readings, photographs and so on, but nobody says that. Actually people did say that during the heyday of operationalism, but that did not have a pernicious effect on physics, because even the people who said it did not really believe it at any relevant level, and they did their work



anyhow. At any rate, it did not make any sense, and was rapidly discarded.'

(Chomsky 1982: 33)

As my discussion of the instrumentalist tradition in the philosophy of science (1.2) shows, this misrepresents both the content and the history of instrumentalism. Not only was such a philosophy of science not 'rapidly discarded', it is still alive and well (cf van Fraassen 1980 for a recent formulation of the principal instrumentalist arguments). Thus, Chomsky's idea that physics enjoys a universally accepted realist interpretation is quite mistaken. So too is his claim that it 'does not make any sense': as I demonstrated in 2.1, the instrumentalist's arguments against realism are rather powerful and force the realist into major revisions of some central realist claims.

Non-realist philosophies of science are only crudely summed up in the way that Chomsky describes them, but so too are their realist counterparts. Chomsky assumes, for example, that the restriction of the data of theoretical linguistics to intuitive (in his terminology, 'introspective'\*) grammaticality judgements is a consequence of the adoption of a non-realist philosophy of linguistics:

'It seems absurd to restrict linguistics to the study of introspective judgements, as is very commonly done...many textbooks that concentrate on linguistic argumentation for

\* Chomsky's use of the term 'introspective' as a synonym for 'intuitive' is unfortunate; in 5.1, I accept a definition of intuition which marks it off clearly from introspective phenomena such as remembering, believing, etc. I show there why it is important to do this.

example are more or less guided by that view. They offer special sets of techniques for dealing with particular data and thus reduce the field to problem solving, defining the field in these terms. That is perhaps the natural definition if you abandon any realist conception of the field.'

(op cit: 33 - 34)

Chomsky is, I think, conflating several different issues here. When one speaks of 'the study of intuitive judgements', one must distinguish between at least two distinct approaches. In the first, one would allow that our object of inquiry is something over and above any set of grammaticality judgements, but in this approach, we accept that such judgements are the principal data on which we test our theories. This is a realist view, and is in fact the one I adopt. Under this approach, we do not claim that theoretical linguistics has grammaticality judgements as its object of study, but that these are its DATA, its evidential basis. This is completely in accordance with what realism is all about.

In the second approach, in contradistinction to the first, we deny that there is some object of study over and above the data, and then restrict the data to grammaticality judgements: this is a non-realist position, but is quite distinct from the first approach. Both of these are distinct from the position which Chomsky says he wants to allow for (although there is little evidence that in practice he actually does) whereby we allow that evidence other than intuitive grammaticality judgements is directly available for the testing of hypotheses in theoretical linguistics.

Chomsky is therefore mistaken in assuming that there are only two methodological options to choose from here, namely an instrumentalist 'purely problem-solving' approach, and a version of realism which admits of more than grammaticality judgements as evidence. And the option which Chomsky fails to admit of (the first of the three I describe) is fully consistent with the most central realist claims.

In adopting this option, I will therefore assume that it is reasonable to take intuitive grammaticality judgements as the evidential basis for theoretical linguistics, in the way that generative grammarians have done for some time, and that it is also feasible to assume a linguistic reality underlying these which we are attempting to characterise. This basis for a realist linguistics is a rather simple and in my view, unsurprising proposal, and yet it is not without its opponents. If we add to it the notion that evidence from neighbouring disciplines do not enter *directly* into the testing of linguistic hypotheses we have a version of autonomism which is even less palatable to many. However, it is one that I want to defend. Note that I have not characterised this linguistic reality as psychological, and in fact I will argue (in part two) that to do so is mistaken. With this added ontological ingredient, the version of autonomism that I propose becomes even more extreme.

To see the extent to which such radical autonomism is opposed, one only has to consider the views of those, such as Derwing and Botha (cf chapters 5 and 3 respectively), who would object to the idea of accepting intuitive grammaticality

judgements as evidence, and those, such as Chomsky (apparently) and Bresnan who object to allowing ONLY these as the means of directly testing linguistic theories. In Chomsky's case, we find the rather odd situation in which he states that other sorts of evidence are relevant to testing but never in practice uses, or recognises, such evidence. In comparison with Chomsky, Bresnan (1978 and elsewhere) has the merit of allowing that evidence from neighbouring disciplines (principally, psychology) should enter the testing of hypotheses in theoretical linguistics, and then in practice trying to develop linguistic theories which, in her view, fit the psychological evidence.

However, it is interesting to observe that in attempting to develop such a 'realistic' linguistic theory, Bresnan does pretty well what one would predict she would do if the methodological basis of my version of autonomism (not to mention Itkonen's: cf chap 4) is right. Consider the basic contention of her lexical functional grammar (LFG) and its relation to the form and testing of the grammar. She wishes to say that psychological evidence points to a (mental) lexicon which is much more than a repository of linguistically arbitrary information, and therefore constructs a grammar in which a highly structured lexicon plays a major part. But the motivation for a grammar with a more highly structured lexicon need not be psychologically orientated, as is shown by the existence of such work as Mohanan (1986). Furthermore, it is not clear that Bresnan uses psychological evidence *directly* to

test her linguistic hypotheses; in Bresnan (1982), the purely linguistic argumentation takes place separately from its supporting psychological evidence. Nor is it clear that the evidence does not equally support competing linguistic theories which do not claim to be essentially psychologically orientated. In fact, given the vast armament of theoretical devices being used in LFG, it would be surprising if the psychological evidence did not support *some* proportion of the framework\*.

The point that I want to stress about Bresnan's 'psychologically real' grammar concerns the relationship between one's metatheoretical orientation and the form of one's theory. There is no doubt that the form of one's theory is informed by one's metatheoretical assumptions (thus the importance of the sorts of issue I am discussing), and this is as true for Bresnan's grammatical theory as it is of any other. What needs to be demonstrated by Bresnan is that *ONLY* her metatheoretical position, to the exclusion of other competing positions (such as mine, or Itkonen's, or Katz': cf chapters 4 and 5), gives rise to the sort of grammatical theory she proposes. If this is not the case, and I suspect that it is not, then there is no reason to take linguistic evidence in support of her theory to count as a vindication of her particular metatheoretical approach.

\* Given the range of theoretical apparatus available within LFG, one wonders whether there is any linguistic state of affairs that it does not allow for. For criticism of this aspect of LFG and a proposal that it generates non-possible human languages, cf Berwick & Weinberg (1986: chaps 3 & 4).

All of this suggests to me that, if one considers LFG (as a grammatical framework) and its motivation independently, Bresnan can be seen to be carrying out two distinct sorts of activity. The first of these is standard autonomous linguistic hypothesising and testing, carried out independently of psychological evidence, and the second is psycholinguistic investigation which, far from directly testing these hypotheses, is meant to suggest that they *fit* with the available psycholinguistic evidence. My interactionist methodology (cf chapter 5 for details) suggests that this is precisely the sort of relationship that would hold between autonomous linguistics and psycholinguistics. Nor does this methodological position suffer from unnecessary 'mystical' suggestions as to the ontological status of the object of autonomous linguistic inquiry: it is far from mystical to assume a linguistic reality which is intersubjectively established as a result of social and psychological processes, but which is distinct from these. However, I pursue this matter in greater detail in part two.

The adoption of this sort of realism means that any given theoretical construct in AL (autonomous linguistics) is a potential candidate for reality, but it does not mean that we assume the reality of a construct without applying the test of heuristic fertility. One of the differences that emerges between my version of realism and Chomsky's can be seen in the way he denies the reality of certain theoretical notions. Consider the construct 'system', for instance.

In phonology, Chomsky has long since argued that the idea of a phonological system is nothing more than a set of derivative correlations among members of a distinctive feature matrix. This is closely tied in with his view of the status of the phoneme, since he assumes that in using a representation like /p/, one is not referring to either a unitary phonological whole or a unit within a system of such wholes. Thus, such representations are 'to be regarded as as nothing more than convenient ad hoc abbreviations for feature bundles, introduced for ease of printing and reading but lacking any systematic import' (SPE: 64). Given an appropriate set of phonological representations and rules which mediate between those and the level of phonetic representation, one can apparently construct a model of phonology which simply does not refer either to objects corresponding to representations like /p/ or to Praguian-like systems such as /p , t , k /.

The methodological issue here seems straightforward enough: according to the sort of realism I have discussed, if there are no generalisations capturable by using /p/-type representations, then there is no justification for adopting a realistic interpretation of such a construct. The same goes for the Praguian (and Saussurean) notion of 'phonological system'. It does seem rather difficult to account for certain phonological phenomena, such as the Great Vowel Shift, without reference to the notion of system, however. While it is true that many generalisations about the vowel shift are capturable by means of distinctive feature-based rules and representations, it is not

clear that in constructing these alone we capture the relational nature of phonological units and the idea of distance within the vowel space. And yet it is arguable that this sort of idea is central to an understanding of what this sort of phenomenon is about (cf Lass 1984: 7.2 for arguments to this effect). Thus, it seems that the notion 'system' is methodologically fruitful and therefore to be interpreted realistically. To deny this, Chomsky must either define the nature of his realism so as to exclude the construct from consideration as a real object, or show that the methodological evidence in its favour can be argued against convincingly.

Of course, I allow that there are terms/constructs which cannot be allowed 'real' status, and the question of whether a given construct must be interpreted this way will depend on its heuristic fertility. Furthermore, as I argued in 1.1, not only can we not assume that real status is once and for all, as in Harre's framework, but nor can we assume that falsification or the decision not to grant a term real status are once-and-for-all matters either.

An example, from physics, of this tentative epistemic status of theoretical constructs\* would be the construct 'phlogiston', which failed the heuristic test because the

\* I ought to note that Harré (1972) does in fact allow that scientific constructs never enjoy a permanent, fixed epistemic status. It seems to me that this observation on his part is in conflict with his claims about incontrovertible proof of existence via ostension.



phenomena it covered did not emerge as a unified whole in succeeding theories, but was seen to cover disparate phenomena which were accountable for under distinct sets of principles in succeeding theories. Consider how one might argue such a case in linguistics. If one takes the construct 'fortis' (in the fortis/lenis dichotomy), one might argue that it explains nothing not explained by other constructs, such as 'voiceless'.

Now, in accordance with the sort of realist methodology I have discussed, expressions like 'force' are to be granted real status despite their being said to be 'reducible to' a variety of derivative phenomena. However, if they allow us to express generalisations not otherwise expressed concerning these phenomena, then real status is merited (Putnam's warranted assertibility). If this were the case for phlogiston, or if it were resurrected and shown to be so, then real status could be re-assigned to the term. For 'fortis', if it adds nothing to the range of phenomena already covered by 'voiceless', then it looks heuristically weak and not available for a realist interpretation. And I do not think there has ever been any evidence that it does in fact allow us any heuristic gain over 'voiceless'\*

Similarly, in the case of 'tense' (and 'lax': the dichotomy is similar to the fortis/lenis one in that it appeals to

\* Note that at least one standard phonetics textbook, Abercrombie (1967), covers the subject matter of articulatory phonetics without reference to the distinction, which is rather suggestive.



putative differences in degree of muscular tension \*), if it were shown that there are generalisations extending over the phenomena subsumable under 'high', 'back' and 'long', then tenseness would be available for real status. Lass (1976, 1984) has long since argued that the 'tense' construct does not express anything over and above these, that statements containing the expression can be 'reduced' to statements containing (one or more of) the former three, with tenseness being discarded as a theoretical tool (1984: 92). The point that should be stressed, as I have argued, is not, as Lass would have it, whether reduction of statements seems possible, but whether we can get from the term heuristic mileage in the form of generalisations not otherwise expressible. If this is missing, then we are not warranted in granting real status to the feature [‡tense].

Interestingly, Lass' methodology is an instrumentalist one (cf 2.2), so that it ought only to be methodological considerations, and not ontological ones, that come into play in assessing the status of the construct (there is no question, in an instrumentalist methodology, of whether the expression should be taken to correspond to something real).

This would not be the case, of course, if Lass were

\* Jakobson & Halle (1964) in fact propose that the phenomena which these two constructs cover are subsumable under one single generalisation, ie that tense/lax and fortis/lenis are manifestations of the same principle.

prepared to admit of a realist interpretation of terms in phonetics, and then interpret these phonological terms phonetically. Alternatively, he could argue that these simply are phonetic terms, and need not be regarded as phonological terms reduced phonetically. However, there is no doubt that this latter avenue is problematical: in discussing these constructs, we are discussing distinctive features, which are (to say the least) rather closely tied into matters phonological, and it is not clear to me just how much of phonology Lass is prepared to interpret phonetically (quite a lot, I imagine, given the remarks in his 1984). Lass has a very sophisticated and sensitive awareness of these methodological problems, and it is perhaps unfair to use his 1984 to discuss his methodological position, given that it is a textbook which of necessity makes little of matters methodological. However, I do think that Lass' position on the extent to which phonology is reducible to phonetics is much more reductionistic than mine. I make it clear (5.2) that my position on this is that to interpret phonological terms phonetically amounts to an untenable reductionism which deprives the subject of its very rationale.

One interesting comment which Lass makes is that 'there seems to be no particular evidence either for the utility OR THE REALITY of such a feature.' (1976:39, emphasis added). He then goes on to ask the obvious question, namely 'what does it mean to say that a feature is 'not real' ?'. He cites three

different factors in this connection : the first of these is an argument that tenseness can only be identified by means of its EFFECTS (on vowels). This is, I think, not a good objection. Many, if not most, of the things for which we might want to claim real status are available to us only through their effects. And the fact of evidence from effects is something which Popper uses as his principal argument for the reality of a postulated entity. Forces are a good example of this.

Lass' second point is that there is 'no empirical (instrumental, perceptual) evidence given for the feature'. Again, this is a weak objection, I think. There is no reason why there should be instrumental evidence for the reality of such a feature unless, of course, one wants to have phonological realities interpreted in an entirely phonetic manner.

As Lass points out, these aren't actually arguments against the feature per se. But nor are they good methodological arguments either. His third point is, however, the sort of argument which I think counts against the real status of this construct, and it is the sort of argument which is important in the version of realism I adopt. Here, Lass argues that there is no methodological reason to adopt the feature 'tense', since there are no generalisations available that are not otherwise available without the construct. This is about right, I think. The argumentation must be to do with the capturing of generalisations, that is, it must be phonological, and Lass' third argument is of this sort. He argues that reification of tenseness 'reduces an arbitrarily chosen set of observables to

a hidden 'generalisation'. The point is that there is in fact no generalisation to be had that is not already available, and this lack of heuristic fertility is the most condemning factor against the possible reality of tenseness. Were there a generalisation to be had, there would be no reason to object to it on the grounds that it might be 'hidden': all generalisations express something over and above the data, and if interpreted realistically, must be said to be about a 'hidden' reality which induces the phenomena.

As I have mentioned, there is a defence of the distinction in Jakobson & Halle (1964), but it has two major defects. The first of these is that they accept a definition of the distinction which is essentially the 'degree of deviation from the neutral position' sort which Lass shows to be untenable. The second is the fact that they use as evidence data from languages with ATR harmony. The principle point of methodological interest where ATR harmony is concerned is that such phenomena are now taken by phoneticians (in the light of subsequent development in general phonetic theory) to be quite distinct from phenomena such as the English 'tense/lax' vowel pairs\*. Additionally, Jakobson & Halle identify the tense/lax distinction with the fortis/lenis one, and assume that there is a generalisation to be captured here, namely the identification of these as instances of the same general

\* Regarding the purely phonetic distinction between tense and lax, note that work as early as Jones (1950) casts considerable doubt on its phonetic content.

principle. Were such a generalisation\* attainable, this would indeed be an compelling reason for assigning real status to the construct(s) in question. However, what we have here is a case of a range of phenomena being apparently subsumable under a single principle, and later turning out, in the light of subsequent theory development, to belong to separate categories. Note that there is nothing irreversible in this: the tense/lax distinction has neither been shown to be incontrovertibly real nor to be incontrovertibly non-existent. Rather, there has been a change in the assessment of the heuristic fertility of the construct. We see in this case an example of the extent to which methodological considerations determine our acceptance or otherwise of a construct.

It might be thought that this emphasis on methodological considerations is a rather instrumentalist-orientated approach to take to the evaluation of constructs; this is therefore an appropriate point at which to try to sustain the realist position in the face of the instrumentalist case against realism.

\* Note that I have not said anything about what might count as a significant linguistic generalisation, despite my relying rather heavily on this notion in arguing for a realist interpretation of linguistic constructs. The only work I am aware of that seeks to explicate the notion is Hurford (1977). Hurford's paper takes this sort of significance to be statistically definable; he shows that under such a statistically interpreted notion of significance, word order universals count as significant generalisations. There is a problem here for my methodological stance: if I accept Hurford's statistical interpretation, and if he is right about word order universals, then I must interpret them realistically, rather than interpreting as derivative phenomena which are to be explained by 'deeper' grammatical principles. I have not yet examined the methodological status of word order universals, however.

## 2.2 Instrumentalism in linguistics

Having examined (in 1.2) some of the main instrumentalist arguments against realism, I now want to look at some of the interpretations of an instrumentalist sort which have been made of constructs in linguistic theory, i.e. aspects of what might be called 'the instrumentalist tradition' in theoretical linguistics. The first of these is in what might loosely be called 'structuralist', i.e. pre-generative linguistics of the sort carried out in the United States in the thirties and forties (another term might be Bloomfieldian and post-Bloomfieldian linguistics). Here the influence of Logical Positivism is apparent; I examine some of its most central claims and try to assess the extent of its influence on the methodology of linguists working within this period. The second is a more recent, and generally more sophisticated, set of assertions about the way in which we should interpret our theoretical constructs: those made by Lass (1976, 1980 and elsewhere), as reflected in his view of our constructs as 'uninterpreted calculus'. Finally, I examine interesting instrumentalist elements in the work of Itkonen, which I return to in my discussion of his views in 4.1.

However, before I proceed to this, I must deal with an objection which, if it is valid, undermines the entirety of what I am trying to do here. It is this: the instrumentalism vs realism distinction is one that has been made in the philosophy of empirical sciences, and applies only to those sorts of

science. If theoretical linguistics is not such a science, then the distinction is irrelevant. This objection is incorporated into a recent statement by Itkonen :

'It is tempting to view the dichotomy of AL (autonomous linguistics: PC) vs psychologism as exemplifying the distinction between the trends in the philosophy of empirical science known as 'instrumentalism' and 'realism'. For this comparison to be valid, however, it should first be proved that AL is an empirical science. But this has not been proved. If on the contrary, AL is a nonempirical science comparable to analytical philosophy or formal logic, as I have argued all along, there is no justification for relating it to the 'instrumentalism vs realism' issue.'

(1983: 284-285)

Not surprisingly, I am inclined to disagree with this. I must state firstly that I am NOT claiming that the autonomous linguistics vs psychologistic linguistics distinction exemplifies the instrumentalist vs realist distinction. In fact, I want explicitly to deny that this is so. I will argue later that the instrumentalist vs realist distinction is one that is applicable even if one abandons psychologism and proposes what Lass (1980:121) calls 'a radical autonomy thesis' for theoretical linguistics. Both Lass and Itkonen, I will argue, make the mistake of assuming that an autonomy thesis (whereby it is held that theoretical linguistics has an object of inquiry and a methodology of its own, qualitatively distinct from that of, say, the physical sciences) is somehow intimately connected with the adoption of an instrumentalist methodology; this is quite mistaken, I believe. However, I postpone discussion of this for the moment (see below on Lass and Itkonen in this chapter and on Itkonen in chapter 4).



Itkonen supposes that one can 'prove' that theoretical (or 'autonomous') linguistics is either an empirical or a nonempirical science. I do not think that proof is a particularly useful or relevant notion here; on the contrary, we are dealing here with metascientific argumentation rather than proof. Itkonen has argued that, in adopting his definition of what counts as an empirical science, autonomous linguistics emerges as nonempirical. I point out later (4.1) that it is not very interesting to argue over what the term 'empirical' OUGHT to mean, though one clearly ought to have an explicit definition of the term if one is to use it. I also point out that the observations about method in theoretical linguistics which Itkonen makes seem to me accurately to describe what theoretical linguists actually do (i.e. they do not collect sets of data and then abstract away from them; rather, they have sets of ill-formed or well-formed expressions as their data, where the ill/well-formedness is determined by intuitive grammaticality judgements). I am therefore quite willing to accept Itkonen's point that AL is not an empirical science IF 'empirical' means 'testable by observation of spatiotemporal events'. If one wants to define 'empirical' as 'falsifiable', however, then I am happy to accept that AL is an empirical science. That is, I take it that theoretical proposals in AL are falsifiable, but not on the basis of the observation of spatiotemporal events.

This is distinct from Itkonen's position: he seems to think (see discussion below for evidence of this), not only that

theories in AL are not falsifiable in this way, but that they are not falsifiable at all. This means, within my (Popperian) definition of what counts as a science, that AL is not a science (this is Lass' conclusion, in fact). Itkonen is using as a definition of 'science' one that is very different from Popper's, as it includes analytical philosophy. Since he appears to take science to be essentially 'axiomatic' in nature, he characterises formal logic and analytical philosophy as well as theoretical linguistics as sciences (Lass, on the other hand, defines 'science' in much the same way as I do, but unlike me, then decides that theoretical linguistics is not thus a science).

This at least clears up the terminological problems, and shows where Itkonen and I disagree on the subject of AL as an empirical science. And this is directly related to the question of whether the instrumentalism vs realism distinction is relevant for a discussion of linguistic theory. My position on this is as follows: the realism vs instrumentalism debate is about the status of theoretical constructs, particularly the question of whether they refer or not to extratheoretical objects. Whether these sorts of notion are of any use in investigating the status of theoretical constructs in AL is an open question. We need to look into the question to find out. This is precisely what I have been doing here. From my consideration of the various aspects of the debate and of the sorts of methodological proposals that have been made in autonomous linguistics, I conclude that we can in fact gain a

fair amount of insight into metatheoretical issues in AL by considering the realism vs instrumentalism dichotomy. I intend to demonstrate this further here by considering what I call the instrumentalist tradition in linguistics; interestingly, this sheds light on the work of Itkonen himself, and ties in with the sorts of ontological problem that Itkonen is concerned with, which I consider in subsequent chapters.

(i) Twaddell, Bloomfield and Harris

To take the first of my three areas of discussion, the nature of logical positivism and its influence on structuralist linguistics: the sort of importance attached to observability which I mentioned in the introduction to this chapter is very much evident in, and in fact constitutes much of the basis of, the logical positivist philosophy of science. A central component of their set of proposals was an attempt to demonstrate that metaphysical propositions are not meaningful, that the only meaningful propositions are those that are elementary and correspond to simple facts of observation, or those that are more complex than this, but which are constructed out of series of elementary propositions via the logical operations, such that their truth or falsehood is entirely dependent on the truth or falsehood of the elementary statements in question, the elementary statements being expressed wholly in observation language. They also held that

such elementary propositions are empirically verifiable, and that we gain knowledge via this process of building up meaningful propositions out of elementary empirically verifiable statements. It follows from this that when we attempt to communicate via metaphysical statements, which are not so constructed, we literally fail to communicate at all: we have not said anything meaningful (i.e. we have said nothing; note the similarity between this and the Wittgensteinian private language argument: whereas metaphysical assertions might reflect some sort of inner state of the speaker, they cannot be said to be meaningful, where meaning is intersubjective).

These fundamental doctrines of logical positivism, as expressed by the members of the Vienna Circle (such as Schlick, Carnap and Neurath), can be seen as constituting an alternative to realism when one considers the proposition 'there is a transcendent external world'. Since this is not reducible to a set of elementary observational statements, it must be taken to be a metaphysical assertion. As such, it is taken to be meaningless within the logical positivist approach. Schlick (1932) was at pains to point out that they were not DENYING the existence of an external world, but were saying that such assertions, and also their negations, were meaningless metaphysics, that in asserting them we assert nothing. This is, for instance, what Carnap's (1956) 'criterion of significance' states: only statements in observation language are meaningful, and that which cannot be reduced to observation language has no meaning.

I have already outlined the principal objections to this sort of observation/theory distinction in chapter 1; note that it can lead to a version of sensationalism such as Mach's very easily, since the notion 'physical object' can be objected to by saying that the proposition 'there are external physical objects' is equally as metaphysical as 'there is an external physical world'; all we have as a certain basis for our knowledge are observation experiences, i.e. sense experiences. Consider Mach's comment that our theoretical constructs are best interpreted as devices for stating as economically as possible sets of laws about our sense experiences:

'Properly speaking, the world is not composed of "things" as its elements, but of colours, tones...in short, what we call individual sensations.... the whole affair (of constructing a physical theory: PC) is a mere affair of economy.'

(Mach 1893/1966: 579)

An interesting consequence of taking this line is that it ends up in a philosophy which becomes increasingly solipsistic: the only foundation for our knowledge is our subjective sense experiences. There are a couple of ways of attempting to avoid this conclusion and still retaining positivism, however. One is Russell's (1912), in which a distinction is made between the content of our sense experiences as individuals (these are distinct even in those circumstances when two or more individuals might be said to be perceiving the same object) and what he calls their structure, by which he means the circumstances under which they occur; it is this which is

remarkably similar from one individual to another. This is no real solution to the problem, though, since it is impossible to tell that the circumstances under which our shared sense experiences occur are similar unless one makes the metaphysical leap of simply asserting that this is so in order to make sense of our reports of sense experiences. A more workable solution is Schlick's, which is to avoid saying that physical bodies are 'complexes of sensations', and to say instead that 'propositions concerning bodies are transformable into equivalent propositions concerning the occurrence of sensations in accordance with laws' (1959:107), so that the subject matter of a physical theory is not sensations, but laws. As a way out of Mach's solipsistic sensationalism, which is a fair way down the road to idealism, this is certainly an improvement on Russell.

Obviously, the problem will not arise if one adopts the realist position of the sort that I stated in 2.1: we can assume, with Quine, that both the notions 'physical world' and 'physical object' are myths which allow us a great amount of progress in managing our experiences, and we can add the realist argument that unless we assume that there really are physical objects and a physical world, there would be no account of why these notions should do so much work for us. As I've said, this consequence of logical positivism, that one should end up so close to such an idealistic philosophy of science, is particularly interesting: one would have thought, given the logical positivist conception of the meaninglessness of

metaphysical assertions which merely express subjective states, that the last thing one would want to reduce science to would be the subjectivity of our individual sense experiences. As Popper has repeatedly pointed out, a major factor in favour of realism is that it allows us to maintain the objectivity of science, while positivism sees it diminish into, at best, laws about our sense experiences.

Regarding the claim that metaphysical propositions are literally meaningless: this has been adequately countered by Popper (1959, 1963 and elsewhere) whose version of realism incorporates a demarcation criterion which distinguishes falsifiable (scientific) from non-falsifiable (non-scientific) theories, while not proposing this as a criterion of meaningfulness. By thus allowing that metaphysical propositions may be meaningful, and allowing that scientific frameworks may, and frequently do, develop from metaphysical 'pictures of the world' (metaphysical research programmes), Popper gives a very convincing account of how scientific frameworks arise. Examples are the heliocentric view of our world, the planets and our sun, which did develop into a falsifiable framework from its roots in mythology, and metaphysical research programmes such as Darwinianism, whose central picture of the way in which living systems evolve, is not, as Popper points out, itself falsifiable, but which yields hypotheses which are. Some (eg Hesse 1966) have taken this 'picture of the world' notion very seriously and stressed that scientific theories are best viewed

as models or metaphors, emphasising this rather than falsifiability and convergence on the truth as a means of characterising the scientific enterprise. This has tended to lead to a non-realist interpretation of scientific theories, with the emphasis on the fact that there may be many different possible 'world pictures' of equal value. However, allowing that there is this metaphorical aspect to scientific theories need not diminish the realist's position; Popper has adequately incorporated the notion into his convergent realism, and others such as Boyd have taken an even more extreme realist stance, claiming that our theories as metaphors 'cut the world at its joints' (Boyd 1979:393).

What does seem to have emerged as a consensus, among realists and non-realists alike, is the view that our theories do have an irreducibly metaphysical ingredient, whether one refers to this in terms of metaphysical research programmes underlying theories, or theories themselves as metaphors. The logical positivist attempt to divorce scientific theories from metaphysical assumptions can be said to have failed. However, my aim here is not to criticise logical positivism at length, and one wants to avoid setting up a positivist straw man in order to maintain realism; it is clear from 2.1 that some of the core notions of this sort of 'consistent empiricism' (as it was also known: cf Schlick 1932) can be developed into sophisticated instrumentalist arguments which do seriously weaken realism. What I do want to do is to examine the effects of logical positivism on the methodology of linguistic analysis in the



United States in the thirties and forties, much of which is important because it forms the methodological background against which generative principles were formulated.

In talking about the influence of positivist thought upon linguistics, I am not concerned with establishing that, say, Bloomfield, or Harris, read and was consciously guided by the works of the Vienna Circle. Rather, I want to suggest that many of the central ideas of positivism came to constitute a general intellectual climate of opinion concerning the nature of science, (Miller 1973 makes this point very clearly) and that this had important repercussions for the way in which linguistic analysis was carried out (and of course, this in turn had consequences for the way in which post-generative work was done). An interesting statement of an instrumentalist methodology for linguistic analysis comes from Twaddell's (1935) paper on possible interpretations of the 'phoneme' construct. Having starting with an argument from idealisation (that we arrive at the phoneme via abstraction from a corpus of data), he asserts that :

'The macro-phoneme is a fiction, defined for the purpose of describing conveniently the phonological relations among the elements of a language, its forms. The sum of such relations among the elements is the phonological system of the language. This phonological system is of course nothing objectively existent: it is not definable as a mental pattern in the minds of the speakers of the language; it is not even a 'platonic idea' which the language actualises.....The phonological system is the phonetician's and phonologist's summarised formulation of the relations; it is not a phenomenon, nor an intuition.'

(1935: 76)

This paper is interesting because of its rejection of even

a physical definition of the phoneme, which represents the consensus view among structuralists of the period and is itself largely influenced by positivist physicalism. The rejection of a psychological interpretation is typical of the positivist influenced work of the time: the idea that science is concerned with observables is here interpreted such that mental phenomena are taken to be unobservable and therefore not the proper concern of the scientist. Of course, logical positivists themselves did not assume that mental phenomena were not a proper object of scientific inquiry; rather, they assumed that notions such as 'mental' were reducible to statements concerning observable behavioural phenomena: here we see how the legacy of positivism helped mould methodological trends, rather than functioning as the explicitly adopted scientific method for linguistic inquiry.

Many of the instrumentalist arguments against realism can be seen in Twaddell's methodological position: pragmatism (stressing the purpose of the investigator), the denial that theoretical constructs have theory-external referents, the emphasis on patterns and relations among the phenomena. But note the extent to which Twaddell cannot help but incorporate a realist ingredient into his instrumentalism: he allows himself to talk of relations and (phonological) forms in a realist manner. If the phonological system is nothing which actually exists, and if it is the sum of the relations among the forms, are we to conclude that the relations and forms themselves are

not objectively existent? Or do we assume that we can legitimately speak of the existence of forms and the relations between them, assuming that any theoretical statement is reducible to a statement about forms and their relations, and not anything over and above these? This would seem to accord with Schlick's reductionist position. It is open to the usual objections to reductionism: one cannot help smuggling theoretical concepts into the picture (recall Putnam's point that even in the most 'observational' of statements, terms like 'physical object' appear), and these themselves are used in a realist way: Twaddell speaks of relations as if they were observable, but they are in fact abstractions of the very sort that he wishes to assume no ontological commitment to. Thus he would be forced, in order to maintain a consistent instrumentalism, to allow that even his 'observables' are abstractions, and would have to retreat to the kind of solipsistic position I described in relation to logical positivism, where the only realities are sense experiences.

I have concentrated here on what I think are the weakest points in Twaddell's methodological position; it should be pointed out that his arguments against physicalism and psychologism are impressive (it is interesting, given subsequent historical developments in the philosophy of linguistics, that he considers a Platonistic interpretation of the phoneme construct). My main point is that, in abandoning these, and in appreciating the fact that our constructs do not correspond in any simple way to extra-theoretical entities, one need not

embrace instrumentalism as the only remaining alternative. This is, in fact, precisely the point I want to make about the work of Lass and Itkonen on the subject.

In fact, it is this combination of a non-instrumentalist methodology coupled with an autonomy thesis of the sort proposed by Lass (1980) that constitutes the main thrust of my proposals; it is a combination which has been overlooked. As evidence that it has been overlooked, consider Twaddell's conclusion that the construct 'phoneme' cannot be said to correspond to a physical, to a psychological, or to a Platonic reality, and his assumption that we must therefore adopt an instrumentalist position. My proposals suggest that these do not exhaust the available options. The same can be said in relation to Katz'(1980) proposals (cf 5.1): he assumes that if we abandon instrumentalism and psychologism, Platonism is the only option left open. Similarly, Lass and Itkonen assume that instrumentalism is the only avenue left open when one adopts an autonomy thesis. I hope to show that this combination of realism and an autonomy thesis of a particular sort (not the Platonic sort) constitutes a fruitful methodological basis for interpreting grammatical theories.

To return to structuralist linguistics: it may be argued that Twaddell is not wholly representative of the instrumentalist position in structuralist methodology; since, as Joos (1957:80) points out, his position on the interpretation of the phoneme was not widely adopted, it is perhaps best to

examine the views of Bloomfield, and later those of Harris, as being more representative of the period.

Bloomfield's methodological views generally represent a much more thoroughly positivist position than those of Twaddell, incorporating a version of physicalism very similar to that of the logical positivists. However, as Itkonen (1978:70) has pointed out, Bloomfield did not subscribe to the view, held by Twaddell and Harris, that a corpus of utterances constitutes the database for linguistic analyses, and in this respect, his philosophy of linguistics is less positivist than it might have been.

The following indicates the influence positivism did have on Bloomfield's methodology:

'The only useful generalisations about language are inductive generalisations....when we have adequate data about many languages, we shall have to return to the problem of general grammar... but this study, when it comes, will not be speculative but inductive.'

(Bloomfield 1935: 20)

This is in stark contrast to Chomsky's views on the matter, and is linked to Bloomfield's reductivist insistence that every structural unit postulated by the linguist must be reducible to some physical phenomenon; take, for example, his definition of the phoneme: 'A minimum same of vocal feature is a phoneme or distinctive sound' (1926:27). Thus inductivism and reductivist physicalism constitute the core of Bloomfield's methodology. I argue that physicalism is not a viable basis for interpreting linguistic constructs. And while it may not seem especially novel to criticise the physical definition of the phoneme, I

want to argue that it is still with us, and that reductionism of this sort in phonology has never really gone away: cf 5.3 on 'phoneticism' in phonology. I will not concentrate on Bloomfield's statements as to linguistic meaning in relation to stimulus-response behaviourism, which has been adequately criticised elsewhere (cf Itkonen 1978: 68-71 for some interesting criticism, though Itkonen assumes, mistakenly I believe, that Bloomfield's physicalist definition of the phoneme has been adequately dismissed). However, I do want to point out that there is some considerable similarity between the (untenable) physical definition of the phoneme and the interpretation placed upon the notion within generative linguistics, which one would normally take to be non-physicalist and non-instrumentalist (in this respect, cf 3.2 for my account of Chomsky as a reductionist).

Consider the comments by Jakobson and Halle (hereafter J&H) on the physical definition of the phoneme in their early defence of distinctive features:

'This so-to-speak 'inner', immanent approach, which locates the distinctive features and their bundles within speech sounds, be it on their motor, acoustical or auditory level, is the most appropriate premise for phonemic operations, although it has been repeatedly contested by 'outer' approaches which in different ways divorce phonemes from concrete sounds.'

(1968: 415; revised version of J & H 1956)

J & H then go on to criticise Twaddell's fictionalist treatment of the phoneme on the grounds that it divorces phonological analysis from its 'correlate(s) in concrete experience' (418) and demand that 'any distinctive feature and,

consequently, any phoneme treated by the linguist, have its constant correlate at each stage of the speech event and thus be identifiable at any level accessible to observation.'(loc cit). This position is identical to Bloomfield's and is open to all of the criticisms that have been made of it since (at the very least) Twaddell's in 1935 including the point that there is no evidence that there are any such constant physical features which can be picked out in any given instantiation of a phoneme. Were it not for the fact that this early paper was revised before its 1968 publication, this might seem a kind of pre-generative statement from Halle. Yet the same point of view is repeated in several papers by Halle in the early sixties. Consider his comments in the (1962) paper 'Phonology in a generative grammar' (reprinted in Fodor & Katz 1964)':

'..all references to segments as "/s/" or a "labial stops" are to be understood as unofficial circumlocutions introduced only to facilitate exposition, but lacking any systematic import'

(footnote 2, p.336)

That is, in this strikingly instrumentalist proposal, phonemes are reducible to distinctive feature clusters, and if these are entirely definable physically, then we have, within generative grammar, exactly the same methodological basis for interpreting phonological constructs as Bloomfield had.

I find this fascinating since it shows how persistently reductivism, physicalism, and instrumentalist interpretations of theoretical notions recur. And, as I point out below, instrumentalism need not appear in conjunction with reductivism

and physicalism, as Lass' and Itkonen's suggestions demonstrate.

If Bloomfield's views are unnecessarily burdened with positivist assumptions, and generative work has not entirely dispensed with them, it is interesting to consider the views of Harris, whose work is commonly taken to constitute the historical link between structuralist and generative linguistics. Unlike Bloomfield, and like Twaddell, Harris takes the data of linguistic theory to be observable events, or regularities among those events, selected from a corpus. The data, for him, are behavioural:

' Investigation in descriptive linguistics consists of recording utterances in a single dialect and analysing the recorded material. The stock of recorded utterances constitutes the corpus of data, and the analysis which is made of it is a compact description of the distribution of elements within it.'

(Harris 1963: 12)

'In investigations in descriptive linguistics, linguistic elements are associated with particular features of the speech behaviour in question, and the relations among these elements are studied.'

(op. cit.: 17)

I argue below (4.1) that the proposals as to the corpus-based nature of theoretical linguistic inquiry are neither viable as a description of how work ought to proceed in theoretical linguistics, nor of how it actually does proceed. For the moment, I will concentrate on the relationship which Harris takes to hold between what he takes to be the 'observable data' and the theoretical constructs we devise to account for them. For Harris, these are merely 'symbols' upon which operations can be performed:



'However, in the course of reducing our elements to simpler combinations of more fundamental elements, we set up entities such as junctures and long components which can only with difficulty be considered as variables directly representing any member of a class of portions of the flow of speech. It is therefore more convenient to consider the elements as purely logical symbols, upon which various operations of mathematical logic can be performed.'

(op cit: 18)

This reflects most of the instrumentalist preoccupations of the logical positivists, and all of their shortcomings. It is an attempt to separate theoretical from observational terms and to avoid ontological commitment to the former. Against it, not only does the realist argue that this separation is impossible; he also points out that if constructs like 'long' and 'junction' are heuristically fertile, this is sufficient warrant for granting them 'real' status. We can claim that linguistic systems do actually have length as a function, and that the temporal length we perceive is a manifestation of this (Arnason 1980 uses the terms 'quantity' and 'length', respectively, to distinguish these). If we find that constructs such as 'junction phoneme' do not have the heuristic fertility of other constructs, then we are justified in abandoning them in favour of others, which in turn we are warranted in claiming 'real' status for. What sort of reality we are dealing with here is the subject matter of my discussion in the chapters that follow, but it seems perfectly acceptable (to me at any rate) to say that realities such as 'quantity' are neither physical properties of speech events nor mental realities, but linguistic properties of linguistic systems per se. But more of this later.

One aspect of Harris' methodological position which is found not only in Twaddell's work, but also in the writings of generativists, is the claim that theoretical linguistics is corpus-based, and that analyses are abstractions from corpora. It is interesting that this should have persisted from pre-generative through to generative methodology. For a defence of this view (by Pulman) from within a version of Chomskyan rationalism, and my objections to it, cf 5.1. Itkonen (1978:75) provides an excellent critique of this notion and also points out the continuity here between pre-generative and generative work. Interestingly, though, Itkonen himself proposes a strongly instrumentalist interpretation of theoretical constructs in grammatical inquiry (see below for discussion).

(ii) Lass and uninterpreted calculus

One can trace the instrumentalism in Lass' work as far back as Lass and Anderson (1975) and then down through his 1976 to Lass 1980, where it finds its fullest expression; traces of it are even apparent in his 1984 textbook on phonology, which does not dwell unduly on matters methodological.

In the epilogue to his 1976 work, Lass adopts Popper's demarcation criterion and argues that linguistics lies on the metaphysical rather than the scientific side of the demarcation (but is nonetheless a rational activity); that is, he takes linguistic theories not to be falsifiable. For him this means that they are not susceptible to disconfirmation in the face of 'experience', thus:

'I think that most theories in linguistics are not in fact scientific in the strict sense, but belong to category (c) (theories which are not refutable: PC): linguistics is at this point largely - if not nearly exclusively- a form of philosophy or metaphysics.'

(Lass 1976: 216)

He then proceeds to argue for the validity of non-refutable theories (which for him means 'non-empirical') as a valuable kind of rational activity. He suggests that we recognise that this is so, and carry on with theoretical linguistics without pretending that it is an empirical science, or genuinely attempt to convert it into such a science by turning it into a properly experimental discipline, along the lines suggested by Derwing (1973) and Ohala, a move which Lass thinks would be counter-productive\*.

Lass' views are very close to Itkonen's on this score (see section (iii) below); my principal objection to them is that we have a third choice intermediate between empirically testable theories and non-empirical, non-testable theories, namely theories which are testable, but not empirically. Thus, one can test linguistic hypotheses, but not via 'experience' or the observation of spatiotemporal events. Rather, they are testable against the data of theoretical linguistics, in the form of sets of well-formed or ill-formed expressions. In addition to the data, we have the usual methodological factors which come into play when theories are to be tested, namely evaluations as to

\* I agree with him. I do not have access to the unpublished manuscript which Lass refers to; however, I discuss Ohala's metatheoretical position in detail in 5.3.

heuristic fertility. If we maintain this sort of realist position, we can still reasonably talk of there being referents for theoretical constructs in grammatical inquiry. What the ontological status of these referents might be is an interesting question which I deal with at length in part 2; it is clear that the two questions, one largely methodological, namely 'do our constructs have referents?' and the other ontological, namely 'what sorts of thing are these referents?' are closely inter-related. I attempt to provide a positive answer to the first of these and to try to answer the second by suggesting (in 5.2) that the object of inquiry is neither psychological nor social (assuming we define satisfactorily what 'social' means: cf chapter 4), nor Platonic, but something other than these, a largely autonomous object which belongs to a category akin to what Popper refers to as 'world three'; this suggestion I owe to Lass himself, but I have a rather different approach to the inter-relationship between the methodological and the ontological questions.

On the subject of this interconnection between the two questions, consider Lass' comments in his 1980. He proposes, in relation to the second of these, that it is useful to talk of 'a linguistic world three'(op cit: 122), whereby we can recognise the possibility of 'language without a knowing/using speaker', parallel to Popper's epistemology without a knowing agent. This strikes me as an excellent idea and it is one I try to defend (chapter 5). However, on the methodological question, Lass

takes quite a different tack from the one I take, and it is one which I find worrying; I also think it is rather in conflict with his response to the ontological question. He says that the consequences of adopting a view like this are something like those discussed by Eddington (1938), in which 'the study of a (putatively) 'empirical' domain is not to be viewed as a 'direct' study of the domain itself, but rather a study of our knowledge of it.' (123). Eddington's view of physics sees it not as an investigation of an entity, the external world, which our knowledge is said to describe, but an investigation of knowledge itself. This leads to an ontology of 'pure structure', where our constructs are said by Lass to be 'uninterpreted calculus'. This strikes me as being excessively instrumentalist; it reflects the fact that Eddington was something of an idealist, who was at pains to stress that the world consists primarily of contents of consciousness. There are striking similarities between this idealism and the logical positivist philosophy of science: both assert, against the realist, that it is only the contents of our sense experiences which we are warranted in claiming to exist. Take the passage quoted by Lass (p.125):

'To the question: what is X when it is not a sensation in any consciousness..? the right answer is probably that the question is a meaningless one - that structure does not necessarily imply an X of which it is the structure.'

(Eddington 1938:151)

Lass goes on to say that, with the addition of an instrumentalist metaphysics, this reflects the metatheoretical view of Harris. But this is an instrumentalist metaphysics.

Eddington is clearly an instrumentalist, as is Lass in following him. Both believe that our theoretical frameworks are not about extratheoretical entities. Lass (p.124) argues that Eddington is not an idealist in the strict sense, but is simply displaying 'an acute consciousness of the power of an epistemological framework to dictate the shape of its own contents - as well as the fact that it always stands as an insuperable barrier to 'direct experience' of anything (there are no theory-free observation languages' (loc cit). But Eddington's philosophy is clearly idealistic, thus:

'To put the conclusion crudely, the stuff of the world is mind-stuff'

'The mind-stuff of the world is, of course, something more general than our individual conscious minds'

'The mind-stuff is the aggregation of relations and relata which form the building material for the physical world'

'It is difficult for the matter-of-fact physicist to accept the view that the substratum of everything is of MENTAL character. But no-one can deny that mind is the first and most direct thing in our experience, and all else is remote inference.'

(Eddington 1927: 281, emphasis added)

And one can stress the fact that epistemological frameworks have the power to dictate their own contents without abandoning realism. One needn't see theories as barriers to the experiencing of reality, but as enabling devices which allow us to get at the structure of reality. Lass is right, of course, in stating that there can be no 'direct' experience of reality without intervening theoretical constructs, but that is partly what realism is about: the logical positivist notion of direct

sensory experience and of statements reporting it was not viable as a picture of how our perceptual system works, and of how we gain knowledge of the world. By adopting the realist's argument that our theories get at a reality which induces our sense experiences (cf Maxwell 1962 for a statement of this sort of 'causal theory of perception'), while still allowing that the theories shape our perceptions, we avoid the subjectivist trap of saying that our theories can only be about the contents of our own consciousness (Eddington) or sense experiences (Mach, logical positivism).

It might be argued that Eddington is not committed to idealism in stressing the power of our theories to shape our perceptions ( I am grateful to Jim Hurford for pointing this out). I accept this, and I have said that an awareness of this feature of theories and their epistemological function is compatible with realism. Nor do I agree with Hempel (1966: 77), who claims that Eddington 'denies the existence of everyday objects'. However, it is the notion that the theory-external world in some sense possesses a mental character that constitutes Eddington's idealism.

If one is to allow that any philosophy is idealistic (say, Berkeley's), ie that there are idealistic philosophies, then one must allow that certain claims about the nature of reality are idealistic. The most common one I know of (discussed in, for example, Popper 1963 and Russell 1912) is this: that the physical world is partly (even primarily) mental and that we can only speak of the existence of something if we can speak

of perceiving it (esse = percipi). This is, I think, precisely what Eddington is saying. It is a position that seems to me clearly in contrast to the realist one, and has historically been taken to be so (cf Popper 1963 for discussion of the historical roots of instrumentalism in idealism, particularly Berkeley's, and Putnam 1975 for the view that idealism has historically been in opposition to various versions of realism).

I also think that Lass is mistaken in assuming that the adoption of an autonomy thesis for linguistic structure (e.g. whereby we take it to be of the 'world three' sort) means that we should follow such an instrumentalist interpretation of our theories. In fact, if world three objects, as Popper (1972) describes them, are objectively existing objects, independent of our knowledge of them (epistemology without a knowing subject), then we are in conflict with this interpretation in adopting instrumentalism: we cannot both claim that the object of inquiry exists as an autonomous reality and then DENY that our theories about it are theories about such an objective reality. It is only by adopting a realist methodology that we can square our methodological assumptions with these sorts of ontological assumption.

Lass' theories-as-uninterpreted-calculus would function as a thoroughgoing instrumentalism if it were not tied to an autonomy thesis of the sort he wishes to adopt. And the ontological assumptions can be pursued if one abandons the instrumentalism. What I want to do is to adopt Lass' autonomy



thesis and abandon his instrumentalism, replacing it with a version of realism. And, having argued for a realist philosophy of linguistics, I proceed, in part 2, to show why I think that an autonomy thesis of this sort is superior to the other currently available ontological positions. Before doing that, however, I want to look at another instrumentalist philosophy of linguistics which also brings out the interesting sorts of relationship which develop between methodological and ontological assumptions.

(iii) Itkonen's pragmatism

One of the most recent philosophies of linguistics, that developed by Itkonen (1979, 1983), whose ontological assumptions are discussed at length in 4.1, contains a great many of the strands of instrumentalism I discussed in 2.1. I hope to show here that it is precisely these strands which make it such an inappropriate basis for describing the methodology of theoretical linguistics. I will also point out what I think are interesting links between these issues in the realist/instrumentalist debate and the question of the ontological status of the object of inquiry (the subject matter of part two).

Without going into detail on the bases of Itkonen's philosophy of linguistics at this point, it can be said that it centres on the notion that theoretical linguistics has a qualitatively distinct status from disciplines such as physics, that its object of inquiry consists, not of observable events

and regularities, but of social norms which we know intuitively, and which form the basis for our (speech) actions. These norms, also referred to as (atheoretical) rules, are describable by means of rule-sentences. Thus, the rule-sentence 'Complements follow their governors in English' would describe a norm (atheoretical rule) which governs our speech acts (where 'speech act' is used in a very wide sense to refer to any act of speaking). Thus a speech act corresponding to 'William ate the pie' or 'John is happy that you've come' would be judged intuitively to be 'correct' whereas 'William the pie ate' or 'John is that you've come happy' would not. There is no doubt that these sorts of rule-sentence are things that we would want to say about English, but there is equally no doubt that in theoretical linguistics we want to achieve more than simply a list of such statements. Indeed, a set of 'observations' of this sort (I use the terms 'observation' here NOT to mean 'spatiotemporal event observed' but something like 'non-spatiotemporal phenomenon noted': more on what I'm getting at here later).

Assuming that we want to achieve more than such a set of (rather uninteresting) observations, and that we want to establish some sort of account of how our set of observations comes to be so, we can say that we want the observations to follow in some way from some systematic, principled account of the structure and functioning of the language in question. For this, we adopt a range of theoretical constructs, such that sentences containing them express propositions about this

systematic whole. I have argued that if such constructs possess a certain amount of heuristic fertility, then we are as warranted as we can be in asserting that they are candidates for 'real' status. Under this realist approach to the interpretation of theoretical constructs, then, we are warranted, for the time being, and until we come up with a heuristically more fruitful account, in saying things like 'There are such things as governors and complements', 'There are verbs of aspectual meaning', 'Progressive BE cannot occur in its own complementation because it is a verb of aspectual meaning and such verbs cannot constitute part of the complementation of progressive BE', and so on.

How are we to interpret such statements, if not in a (suitably sophisticated) realist manner? According to Itkonen, our theoretical statements, which include our hypotheses about why the data should be as they are, are not falsifiable, but are merely 'pictures' or systematisations of sets of rule-sentences, which in turn are about norms (normative rules). Furthermore, rule-sentences and hypothetical statements are about the same thing. Thus:

'...non-trivial grammatical theories, whose truth or falsity is NOT known, are not empirical theories, but hypothetical conceptual descriptions, given that both (atheoretical) rule-sentences and (theoretical) grammars speak -in different ways- about the SAME normative reality.'

(Itkonen 1978:166, emphasis in original)

Allowing for the fact that I accept his point that grammatical theories are not empirical, if empirical means not

testable by means of spatiotemporal observation, it is clear that Itkonen is NOT allowing that grammatical theories are nonetheless testable in some other way. This is mistaken, I believe, for the following reasons. If we distinguish the data which is to be accounted for from our theories on the one hand and the some underlying principles, structures or systems of which the data are manifestations, and which we attempt to guess at via our theoretical constructs, then it is clear that our theories are about the underlying principles, and only indirectly about the data. Rather, the data are what we hope will fall out from our theories, such that a statement of the rule-sentence sort will follow in some systematic way from the set of underlying principles which we postulate. Itkonen adopts a typically instrumentalist view when he claims that the systematisation of a set of rule-sentences is nothing more than a systematisation, i.e. the theoretical terms we use are not actually ABOUT anything other than the data: they do not refer to anything, have no referents. Thus the propositions expressed by statements containing them lack truth value.

My objections to this sort of instrumentalism are precisely those expressed in 2.1. If we allow, and we must, that not just ANY systematisation will do, we allow that some are more heuristically fertile than others, and Itkonen would allow this. But we are left with no explanation as to WHY some theoretical frameworks are more fertile in this way. It is only by making the realist assumption that some frameworks get closer to the nature of some theory-external reality that we can offer some

explanation for theory success. Like Duhem, Itkonen simply accepts that some theories are more fruitful than others and leaves it at that\*. And this induces just the sort of complacency that we want to avoid: if one framework gives us a fair amount of insight into the phenomena, why should we attempt to improve upon it?

If this very relativistic approach to rival theories is heuristically unsatisfactory, Itkonen's pragmatism is even more so: he denies (in his 1983:129) that it is even possible to talk in a non-circular way of one theory being more fertile or successful than another (as I pointed out in 2.1, footnote 4). Incidentally, this rather contradicts his comments about the growth of science in his 1978 (p.171) where he seems to think that believing the earth to be flat is less desirable than believing it to be round; surely this sort of comment presupposes precisely the sort of conception of success that Itkonen says we can only define circularly, i.e. in relation to arbitrarily changing purposes. From the realist point of view, the heuristic fertility of supposing the earth to be spherical

\* Itkonen accepts (CLT: 134-135) that we CAN choose, on non-arbitrary grounds, between competing linguistic theories. The choice between them, however, is not determined by correspondence between theoretical terms and unobservable entities, as in the empirical sciences, but by whether a given theory provides us with a 'good overview of the subject'. This simply begs all of the questions we have been trying to answer, and since he insists on saying that the criteria of goodness vary from one linguistic school to another, he robs his discussion of any nonarbitrary grounds for choosing between competing theories.

is sufficient warrant for saying that it really is so, unless we come up with an even more fruitful proposal.

In theoretical linguistics, we end up, in adopting Itkonen's instrumentalism, by accepting all proposed theoretical frameworks and assuming that there is nothing at issue when it comes to deciding between them. This is a warrant for heuristic complacency, and is interestingly not in accord with what practitioners actually do (even if one seeks an eclectic synthesis of current theoretical proposals, one is still attempting to produce a theoretical proposal distinct from any single contributing theory, and attempting to select from the contents of individual theories what one takes to be their most fertile parts).

It is clear from this that many of the sorts of methodological assumption discussed in 2.1 have been explicitly adopted by Itkonen; they appear to have precisely the defects in investigating method in theoretical linguistics that they have in investigating the methodological basis of natural sciences.

In discussing instrumentalism in theoretical linguistics, I have tried to show that the core components of the instrumentalist challenge to realism have continued to be proposed, despite the overt switch, since the advent of generative linguistics, from an explicitly non-realist to an explicitly realist programme. I think I have shown that there are interesting similarities in methodology between such diverse writers as Bloomfield, Twaddell, Harris, Lass and Itkonen. I

think this reflects the strength of many of the core instrumentalist arguments, with their emphasis on the power of theoretical frameworks to shape what we perceive and what we count as success in linguistic analysis. However, I've argued here and in 2.1 that realism can accommodate these arguments, and that they can even be turned to the realist's advantage. By thus accommodating instrumentalist considerations into a realist methodology, we arrive at a version of realism sufficiently sophisticated as to allow us to make claims about the 'real' status of theoretical constructs without falling into the pitfalls of the sort of naive realism which has been evident in some work in generative linguistics.

I conclude that the fundamental assumptions underlying the realist position are more appropriate for theoretical linguistics than their instrumentalist rivals. In what follows, I will assume a suitably sophisticated version of realism and try to tackle the ontological questions which arise from it.

PART TWO

THE ONTOLOGICAL STATUS OF THE OBJECT OF INQUIRY



## Introduction

Given a realist philosophy of theoretical linguistics, one is faced with the ontological question, i.e. the problem of specifying the nature and status of the reality under investigation. What I intend to do in what follows is to describe the range of possible positions which could be taken on this issue, to identify particular linguists with these positions, and to argue for a version of autonomism based on Popper's notion of an autonomous category of objects consisting of man's mental products. I will therefore attempt to characterise the positions taken by Botha, Chomsky, Fodor, Itkonen and Katz and to argue for my position on the basis of what I take to be the weaknesses of theirs.

I appreciate that the methodological and ontological questions are not sharply distinguished, and that we are not dealing here simply with a logical progression from the adoption of a version of realism to the examination of ontological questions. Rather, the two sets of questions are intertwined in complex ways. However, it is easier from an expository point of view to proceed this way, and there are clearly different versions of realism in theoretical linguistics which depend for their differences, more or less, on matters ontological.

I will identify three general categories within which a variety of positions can be located. These are: linguistic objects as psychological realities, as social realities, and as realities which are neither social nor psychological. I will argue throughout for (a version of) the third of these.

## CHAPTER 3

## LINGUISTIC OBJECTS AS PSYCHOLOGICAL REALITY

## 3.1 Materialism and reductionism

## (i) Reductionism in psychology

Consider the case for materialism put by Smart (1963). He argues for a realist interpretation of (macroscopic, microscopic and sub-microscopic) terms in physics, and rejects an instrumentalist account of these in terms of sense data statements. That is, he takes it to be untenable to devise a realist interpretation of macroscopic objects and at the same time an instrumentalist or phenomenalist interpretation of sub-microscopic objects (16-49). He then argues (50-63) that we must interpret terms and statements in biology and psychology (and any other 'emergent' domain of inquiry) in an entirely instrumentalist way. Thus, he proposes that we reduce all statements in biology and psychology to 'genuine' law-like statements in physics. Thus there are no 'emergent laws and properties' (52) but only 'empirical generalisations' (52) in these two disciplines. This follows from his physicalism: since there are only physical realities, and since physics constitutes the study of such realities, then any laws there are will be physical laws, stateable solely in the terms of physics; any generalisations which can be made outside of physics are reducible to genuinely explanatory laws of physics.

It follows from this that our linguistic reality-as-psychological reality must be describable in terms of 'genuinely

explanatory' physical laws. This is a very strong version of reductionism, then: generalisations in theoretical linguistics, taken to be about psychological states-of-affairs, must be reducible to physical laws. Indeed, this very position has been adopted by Botha (1979), which I discuss below (95 - 101). Before considering the implications of such a reductionism for theoretical linguistics, however, consider the problems of such a version of reductionism for psychology alone.

What Smart is saying is that there are no genuine laws of an explanatory sort in psychology. I have a methodological objection to his position, however. If he is to reject phenomenalism, this means rejecting the reductionism it involves (cf 2.1 on reductionism in phenomenalist philosophies of science). Consider the phenomenalist's reductionist position: it incorporates an assertion that we give epistemological priority to sense experiences, i.e. it takes these to be the only solid realities of which we can be absolutely certain. Since our knowledge of these is the only certain knowledge we may have, it follows for the phenomenalist that talk of anything over and above these involves a metaphysical leap not warranted by the phenomena. Generalisations concerning so-called 'realities' over and above the phenomena are taken to be reducible to statements about sense experiences. Thus there are not generalisations about 'physical realities', only generalisations, of a truly explanatory sort about sense experiences.

Considering that Smart rejects this position, it is rather odd that he proposes a remarkably similar approach to generalisations in biology and psychology. In Smart's case, we see a kind of epistemological priority being given to macroscopic physical realities (16), which is extended to microscopic and sub-microscopic realities. But the possibility that there may be realities over and above these, e.g. those of a biological and psychological sort, is ruled out, in much the same way that the phenomenalist rules out the possibility of physical realities over and above the phenomena. If the phenomenalist approach to sub-microscopic realities is impoverished because it 'gives an ontological priority to everyday concepts (i.e. concepts relating to the macrophysical, such as 'table', 'door', etc) (48), then surely Smart's own reductionism regarding biological and psychological phenomena is equally impoverished in giving ontological priority to everyday concepts of a physical sort. Interestingly, the 'metaphysical leap' that is required of the realist in assuming that there are physical realities (such as forces, etc) over and above the phenomena is very similar to the 'leap' required of the non-reductionist in assuming that there may be biological and psychological realities.

It seems to me that the appropriate response to this question of whether we ought to assume the existence of psychological realities relates to the sorts of methodological consideration I discussed in 2.1: just as our realist assumptions about physical objects (and forces, etc) afford us a

certain amount of heuristic gain, so assumptions about the existence of biological and psychological realities may similarly be heuristically fertile. If the objects, processes and structures of a biological and psychological sort which we postulate do possess such heuristic fertility, then we are warranted in asserting of them that they are potential candidates for reality.

This would mean that we would allow that it makes as much sense to speak of there being states of mind, intentions, and memories in existence as it does to speak of there being stones, stars and trees. Nor would we have to insist that propositions concerning states of mind be reducible to those concerning more primitive categories, anymore than we would have to speak of propositions concerning stones being reducible to those concerning sense phenomena.

In short, I am arguing that the objection to Smart's reductionism is of the same sort as the objection to phenomenalist reductionism, which Smart himself is opposed to.

#### (ii) Physicalist psychologism in linguistics

To see what physical reductionism in linguistics amounts to, consider the position adopted by Botha (1979) in his 'progressive mentalism'. He rightly points out that Chomsky fails to 'specify in clear and precise terms the content of his expressions "to impute existence to theoretical constructs" and "to attribute psychological reality to theoretical constructs"' (1979: 17), claiming that Chomskyan mentalistic claims are

ontologically indeterminate. In this I agree entirely with Botha (cf 3.2). However, that is as far as Botha's and my ontological assumptions coincide. Having thus criticised Chomsky, he outlines the basis for a physical mentalism, incorporating a set of conditions on the ontological status of linguistic entities, an essential one of which is his Physical Basis Condition, stated thus:

'A theoretically postulated mental entity cannot be granted existence unless it is somehow realised in the (physical) mechanisms of the brain.'

(1979: 64)

He takes this even further by proposing another ontological condition, namely the Neurological Condition:

'A mental entity postulated by a general linguistic theory cannot be granted existence unless certain neurons exhibit particular properties before they have been exposed to linguistic experience.'

(66)

I think this reflects a physicalism rather similar to that of Smart. As such it reflects a lack of understanding of non-physicalist realism, such as Platonic realism. Thus, Botha assumes that Platonism is not a version of realism at all, but a kind of instrumentalism:

'a progressive mentalism (is distinct from) a nonmentalism such as Platonism whose claims are about fictitious, nonreal objects'

(61)

This claim is simply false if Botha is claiming that Platonists do not claim real status for their ideal entities. Katz, for instance, makes the following statement:

'I now realise there was all along an alternative to both the American structuralists' and the Chomskian conception of

what grammars are theories of, namely the Platonic realist view that grammars are theories of abstract objects..'

(Katz 1981: 3)

It is clear that 'ideal' in the Platonist sense is not the same as 'idealised'. An idealised theory is abstracted away from some set of phenomena. Thus an idealised physical theory would utilise constructs whose properties are arrived at by abstracting away from actually encountered singular entities. An example of this would be frictionless planes, where we abstract, or factor out, certain properties of actually occurring planes and ignore others. With mental theories, we would single out certain relevant properties of mental phenomena and ignore others (we would have to do this in fact: cf 1.1), the relevance being decided by the theoretical assumptions we made. However, such idealised constructs still relate to actually existing entities (theories referring to frictionless planes are still about actual spatiotemporal planes). Ideal (or 'abstract') entities of the Platonic sort, on the other hand, are not of this sort. Rather, they are taken to be real, but non-spatiotemporal, entities. In the Platonist's view, they are not fictions at all. Maintaining this distinction between ideal and idealised', we see that 'abstract' in the Platonic sense is parallel to 'ideal' and 'abstracted away from' parallel to 'idealised'. Botha notes this important distinction but makes the mistake of assuming that 'ideal' in the Platonistic sense means fictitious. It does not.

This point is important, as it undermines Botha's claims

about the ontological status of the object of inquiry in theoretical linguistics. He states that 'No form of nonmentalism makes any claim about an underlying reality', and this forms the basis of his attempt to formulate an ontological 'import' for his progressive mentalism. The first condition he imposes on his progressive mentalism, the Reality Condition (61) does not, as he supposes, demarcate Platonism from his version of mentalism. It is stated thus:

'In order for any form of mentalism to be progressive, its ontological claims must refer, ultimately, to entities which are both real and uniquely identifiable.'

Botha mistakenly thinks that this condition draws a distinction between his progressive mentalism and Platonism. This is not the case, as we have seen, since Platonism is a version of realism which does in fact make ontological claims which refer to entities which are both real and uniquely identifiable.

Of course, Botha could argue that there is no basis for assuming the existence of ideal, Platonic objects, but this is not what he has argued.

There are other methodological problems with the foundations of Botha's ontological proposals. One of these is the 'general metascientific perspective' (66) from which he views these proposals. He states that 'The general tenet of such conditions (ie his ontological conditions as described above: PC) is the following: the existence or non-existence of a mental entity is reflected by the manner in which it does or does not interact with other kinds of entity or process which may



be assumed to exist.'. Botha quotes Dudley Shapere (1969) to state the philosophical basis of his ontological conditions: 'To say that A exists implies ...that A can interact with other things that exist'. Botha then goes on to state that fictitious entities (which is what he takes Platonic objects to be) cannot interact with other existing entities.

This is quite mistaken, and in an interesting way, though Botha could have turned this comment into something worth saying about a particular version of Platonism (Katz'). The interesting mistake that Botha makes is to cite as his methodological rationale the very rationale that Popper uses to support a position diametrically opposed to his. Popper cites the following argument to support his anti-reductionist proposals for a 'world three' which is ontologically distinct from the physical world:

'One of my main theses is that World Three objects can be real...not only in their World One (physical:PC) materialisations or embodiments, but also in their World Three aspects. As World Three objects, they may induce men to produce other World Three objects and, thereby, to act on World One; and interaction with World One -even indirect interaction- I regard as a decisive argument for calling a thing real.'

(Popper & Eccles 1977:39)

We see here that the metascientific perspective that Botha thinks differentiates his physical reductionism from nonreductionist programmes is EXACTLY the perspective that Popper uses to argue for a nonphysicalist ontology. Thus, I am happy to adopt this metascientific perspective as a means of assessing whether an entity can be judged to be real, but like

Popper, I see this as a tool for arguing AGAINST physical reductionism.

The upshot of all this is that neither Botha's metascientific perspective nor the specific ontological conditions he proposes are sufficient to rule out nonreductionist ontologies.

I did mention, however, that there is a point that can be made about Katz' Platonism which DOES relate to the argument from interaction. That is that Katz' Platonic objects are said not to enter into causal relations with mental or physical realities, and this means, of course, that it is a principle of Katz' Platonism that interaction cannot occur. The obvious response to this is that we cannot surely then have any way of coming to know such Platonic objects. Here, Botha really could attack Platonism by means of an argument from interaction (as we have seen, it is not an argument that holds against Popper's interactionism). Naturally, Katz has a reply to this, which I take to be unsatisfactory, but that is one of the subjects of discussion in 5.1.

I have one more methodological comment to make about Botha's reductionism, and it is this: that his proposals do not accord well with the actual practice of theoretical linguistics. The discipline proceeds by means of linguists thinking up hypotheses and testing these against the data, which are arrived at by means of grammaticality judgements. This method has yielded interesting results and has not used neurological investigation either as its starting point or as a point of

reference for that matter. This suggests that it is a viable activity in itself, and that we need not refuse to assign real status to its constructs because of the absence of neurological correlates. I do not doubt that the object investigated by theoretical linguists interacts with, and is constrained by, neurological factors, in ways which we do not yet know, but this fact would be recognised, and investigated, in the sort of interactionist programme I assume (5.2). By thus adopting interactionism, which allows for a strong autonomy thesis, we can successfully characterise the way that theoretical linguistics actually proceeds while allowing for interesting discoveries about the way in which neurological factors do come into play. All of this makes for a considerably more sophisticated, and heuristically more fruitful, research programme than the sort of physicalist reductionism which Botha proposes.

Having said that, one must not assume that the sort of reductionism I have just discussed constitutes the only version of a psychologically interpreted theoretical linguistics; therefore I now turn to versions of non-physicalist psychologism.

### 3.2 Nonphysicalist psychologism

#### (i) Introduction

One of the most salient debates in the philosophy of mind is the question of whether mental processes are reducible to physical

states of affairs, and the debate in which most of the arguments on this topic have been proposed is the debate between physicalists of varying sorts, and dualists. This debate can be referred to as the mind/brain, or mind/body, debate, concerning as it does the question of whether properties that are attributed to mind are reducible to brain states. This is a long-standing and complex philosophical problem, and I make no attempt to contribute to it. It is relevant for my concerns, though, as I am attempting to consider various differing versions of the view that the object of linguistic investigation is psychological in nature, and one has to give a coherent account of what is meant by 'psychological' if one is to adopt this view.

If one is to consider non-physicalist accounts of psychological phenomena, the work of Descartes is as good a place to start as any, particularly since (a) the issues he tackled are relevant to what I am discussing and (b) his claims regarding innate ideas have played a central role in the philosophy of linguistics since Chomsky (1966). I will try to bring out what I see as a crucial link between Descartes' dualist, non-physicalist position and his proposals concerning innateness. My concern is not with innateness per se, however, but with alternatives to physicalism.

#### (ii) Dualism and psychologism

There are two principal aspects of Descartes' philosophy which are important for this discussion: dualism and the

doctrine of 'innate ideas'. Regarding the first of these, Descartes states, in the sixth meditation:

'In this inquiry, what I first note is the great difference between mind and body, in that the body, from its very nature, is always divisible, and mind altogether indivisible. For truly, when I consider the mind, that is to say, myself in so far as I am a thinking thing, I can distinguish in myself no parts; I apprehend myself to be a thing single and entire...The opposite holds in respect of a corporeal, i.e. extended, thing.'

(trans.Smith 1952: 261)

'In the next place, I take note that the mind is immediately affected, not by all parts of the body, but only by the brain, or rather perhaps only by one small part of it, viz by that part in which the sensus communis is said to be.'

'Finally, I note that each of the motions that occur in the part of the brain by which the mind is immediately affected gives rise always to the one and the same sensation, and likewise note that we cannot wish for or imagine any better arrangement.'

(op cit: 262)

This partly states the basis of Descartes' dualism: mind is unextended whereas brain is extended; the two are qualitatively different and they interact in the sense that brain stimuli are registered as perceptions in the conscious mind, and conscious activities of the mind somehow get transmitted via the brain to the physical world. Of course, few now are willing to accept such a non-physicalist view of the mind, and perhaps the principal reason for this is that Descartes' philosophy was unable to explain how two such distinct entities are able to interact at all.

However, I want to bear in mind the details of just how much of a nonphysicalist interpretation of the mind Descartes had in discussing his proposals concerning innate ideas. On

innateness, he states in the third meditation:

'To consider now the ideas, some appear to me to be innate, others adventitious, that is to say foreign to me and coming from without, and others to be made or invented by me.'

(216)

In a reply to Regius concerning innate ideas, he writes:

'I have never written, nor been of the opinion, that the mind needs innate ideas in the sense of something different from its faculty of thinking. I observed, however, that there were in myself certain thoughts that did not proceed from external objects, nor from a determination of my will, but only from the thinking faculty that is in me; and therefore, in order to distinguish the ideas or notions that are the content of these thoughts from other ideas which are adventitious or manufactured, I called them innate.'

(trans Anscombe & Geach 1954: 302)

However we regard Descartes' typology of ideas set out here, and regardless of whether his notion 'idea' was consistent (cf Kenny 1967 for a critique of the notion 'idea' in Descartes' work), it is clear that innateness for Descartes is MENTAL innateness, where 'mental' is interpreted as above, ie relating to 'thinking substance', unextended and distinct from brain process. This is important, since we cannot interpret what sort of innateness Descartes is proposing unless we first understand his dualist philosophy of mind. It is clear that it would be possible to posit an innate capacity of a purely physical, corporeal, extended sort; if we did, we would not be proposing the sort of innateness that Descartes was proposing.

What is equally as clear is that, with regard to a specifically linguistic innate capacity, we must specify whether we are positing as physically or nonphysically (as in Descartes)

innate faculty. Any proponent of an innateness hypothesis must make it clear what FORM of the hypothesis he is proposing, ie whether it is of the strictly Cartesian dualist sort, of a non-dualist sort, or of a kind neutral with regard to the dualist/physicalist debate.

The question which naturally arises here is whether Chomsky adopts a fully Cartesian innateness or not. Having done this, we can then begin to establish what sort of psychologism Chomsky is proposing. It is as well, in trying to establish exactly what Chomsky is claiming, to trace Chomsky's thoughts on the matter chronologically, beginning with Cartesian Linguistics. It should be emphasised that my concern here is not with the rationalist/empiricist debate (cf Cooper 1972, Sampson 1975 for criticism of Chomsky on this) but with the question of the nature of Chomsky's psychologism.

It is clear from his comments in Cartesian Linguistics that Chomsky appreciates Descartes' dualism. Early on in the work he states:

'Arguing from the presumed impossibility of a mechanistic explanation for the creative aspect of normal use of language, Descartes concludes that in addition to body it is necessary to attribute mind - a substance whose essence is thought - to other humans.

(1966: 5)

He sums up Descartes' arguments concerning evidence from language in favour of mind as follows:

'In summary, it is the diversity of human behaviour, its appropriateness to new situations, and man's capacity to innovate - the creative aspect of language use providing the principal indication of this - that leads Descartes to attribute possession of mind to other humans, since he regards this

capacity as beyond the limitations of any imaginable mechanism.'

(op cit: 6)

We can conclude thus far that Chomsky is at least aware of, and appears to accept, Descartes' conception of mind as non-corporeal. Later in the same work, he adopts the Cartesian notion of innateness as part of a universal grammar approach to language:

'By attributing such principles (the organising principles that make language learning possible: PC) to the mind, as an innate property, it becomes possible to account for the quite obvious fact that the speaker of a language knows a great deal that he has not learned.'

(60)

In Language and Mind (1968), Chomsky expounds the Cartesian idea of a *res cogitans*, a non-corporeal mind alongside body, and states that 'with all its gaps and deficiencies, it is an argument that must be taken seriously (1968: 7). However, having later argued against a purely behavioural approach to mental phenomena, he states:

'On the other hand, the proposals of the Cartesians themselves were of no real substance; the phenomena in question are not explained satisfactorily by attributing them to an "active principle" called "mind", the properties of which are not developed in any coherent or comprehensive way.'

'It seems to me that the most hopeful approach today is to describe the phenomena of language and of mental activity as accurately as possible, to try to develop an abstract theoretical apparatus that will as far as possible account for these phenomena and reveal the principles of their organisation and functioning, without attempting, for the present, to relate the postulated mental structures and processes to any physiological mechanisms or to interpret mental function in terms of "physical causes".'

(1968: 14)

I quote at length because it is clear from this that



Chomsky appears to be adopting, not a Cartesian dualist account of innateness, but a 'neutral' account of the sort I discuss in 3.3, whereby the physicalist/non-physicalist issue is left undecided or simply not addressed. I have suggested that this be called the 'ontology-neutral' approach, but the term is, interestingly, not too appropriate for Chomsky, since his position is not entirely neutral. He does go as far as to claim that the reality under investigation is mental, and this constitutes the beginnings of an ontological interpretation of the object of inquiry. The problem is that he does not go on to specify exactly what we are to take 'mental' to mean. It is this fact which has given rise to much of the discussion concerning the content of Chomsky's claimed psychological status for grammars, and it is this which Botha rightly describes as the ontological indeterminacy of Chomsky's psychological reality.

The question of Chomsky's ontological assumptions becomes more complex if we consider the remarks in his later work. In Reflections on Language (1970), we find firstly the following statement, which corresponds to his position in Language and Mind:

'With the progress of science, we may come to know something of the physical representation of the grammar and the language faculty...For the moment, we can only characterise the properties of grammars and of the language faculty in abstract terms.'

(36)

However, a later comment seems to suggest a non-neutral ontological position:

'Learning is primarily a matter of filling in detail within a structure that is innate. We depart from the tradition in several respects, specifically, in taking the "a priori system" to be biologically determined.'

(39)

For the present discussion, this departure from the Cartesian framework of assumptions is important; it is a definite indication that we are dealing with the 'biological' rather than with Descartes' *res cogitans*, the non-corporeal mind alongside body. And Chomsky explicitly recognises this departure from the Cartesian tradition.

So far, then, there are three different positions which Chomsky has to choose from: a strict Cartesian 'non-corporeal' innateness, a biologically interpreted one, or a neutral one. While he appears to have appreciated the nature of Descartes' innateness, he never explicitly adopts it. He does consistently state that our knowledge of physical instantiation is too underdeveloped to make any exact physical claims and that we must make do with abstract characterisations of cognitive structures. This position looks like a neutral one, but it is arguable (and Chomsky himself argues this, as we shall see) that the biological view is not neutral with respect to the dualist/physicalist debate. The trouble for the dualist here is that 'physical representation' is ambiguous, given dualist assumptions. It may mean either (i) that our innate structure is entirely physical, but we are not at present in a position to say exactly what those physical structures are, or (ii) that the innate structure is nonphysical, but has physical correlates,

which again we are not in a position to specify.

As evidence that Chomsky's position is not a dualist one, consider the following statement in Rules and Representations (1980):

'For Descartes, mind is not a part of the biological world...One might then argue that we are not studying Descartes' problem when we consider the human mind as a specific biological system, one with components and elements of varied kinds, to be explored as we would any other aspect of the physical world.'

(30)

This does suggest that the innate structures Chomsky is interested in are decidedly within the physical side of the dualist's dichotomy. Thus, I am inclined to agree with Chomsky that he is not working within a Cartesian dualist framework. However, consider the following remark of Chomsky's on the matter:

'This conclusion holds, however, only if we regard Descartes as an irrational dogmatist, that is, as putting forth doctrines that define the domain of inquiry, rather than as arguing for principles that he believed he had established within an inquiry more broadly construed. That seems to be a questionable move.'

(op cit: 30-31)

I think we have to take this with rather a large pinch of scepticism; it is tempting (personally, I find the temptation irresistible) to see him as confusing Descartes with Chomsky. We cannot begin to revise what Descartes said in the light of how dogmatic we think he might have been about it, otherwise we end up with any number of interpretations of his philosophy. Consider whether we ought to avoid Descartes' innateness proposals altogether, along with the other basic principles of

his philosophy, on the grounds that Descartes intended them in a non-dogmatic spirit. I assume that Chomsky would rather object to that, and for much the same reasons that I think we should object to Chomsky's reinterpretation of Cartesian dualism. We have little alternative but to recognise that Descartes was a dualist (in fact, he is always taken to be the classic dualist) and to see Chomsky's proposals as a departure from Descartes. In doing so, we are hardly committed to seeing Descartes as an 'irrational dogmatist'.

That Chomsky is proposing a non-dualist ontology, and is thus departing from the Cartesian tradition is made evident in the following statement in *Rules and Representations*:

'When I use terms such as 'mind', 'mental representation', 'mental computation', and the like, I am keeping to the level of abstract characterisation of the properties of certain physical mechanisms, as yet almost entirely unknown. There is no further ontological import to such references to mind or mental representations and acts...the inquiry belongs to the study of mind, in the terminology that I will adopt, though it need in no sense imply the existence of entities removed from the physical world.'

(1980: 5)

Since Descartes WAS asserting (and not merely implying) the existence of entities removed from the physical world, it is clear that this is a non-dualist position. Note, however, the cleverness of Chomsky's argumentative strategy in the succeeding paragraphs:

'It is perhaps worth stressing, in this connection, that the notion of 'physical world' is open and evolving...It may be that contemporary natural science already provides adequate principles for the understanding of mind. Or perhaps principles now unknown enter into the functioning of the human or animal minds, in which case the notion of 'physical body' must be

extended, as has often happened in the past, to incorporate entities and principles of hitherto unrecognised character. Then so much of the so-called "mind-body" problem will be solved, ...by invoking principles that seemed incomprehensible or even abhorrent to the scientific imagination of an earlier generation.'

(op cit: 6)

This does constitute a marked departure from the Cartesian tradition in that Chomsky is conceiving of shifting the very terms of reference in which Descartes framed the problem and his response to it. That is, if Descartes represents the classical dualist perspective on the mind-body problem, Chomsky is tackling it by assuming that the dichotomy itself be obviated, and thus the problem. This is a perfectly legitimate, not to mention interesting, approach to take to the physicalist/nonphysicalist philosophies of mind, but it cannot be construed as a continuation of Cartesian dualism: in fact, it contradicts it. There is also something of a problem for this approach, namely its terribly provisional nature. While speculative approaches to traditional philosophical problems are perfectly valid, we are left with much that is merely promissory in Chomsky's suggestion that we may come to understand, somehow, the nature of physical instantiation, to the point where we redefine the very notion 'physical'. One recalls Bloomfield's hope that the progress of physical science would somehow solve the problems of semantic analysis. Botha, too, hopes that neural discoveries will constitute the real referents for our theoretical terms, although his position is much more reductionistic than Chomsky's since Botha thinks that our

current conceptions of 'the physical' will suffice, whereas Chomsky does not.

Botha, then, is a naive physical reductionist whereas Chomsky appears to opt (finally!) for an 'ontology-neutral' position. However, I think I have shown that there are at least undertones of some sort of physical reductionism in Chomsky's proposals; I say 'some sort of' since, with the sort of indeterminacy that Botha mentions in Chomsky's ontology, it is hard to pin him down. In this connection, I want to suggest that the hint of physical reductionism in Chomsky's work is at odds with his long-standing position on behaviouristic reductionism.

It is striking that Chomsky has long since championed the case against behaviouristic reductionism, and one might then expect him to take an anti-reductionistic position on the possibility of a physicalist reductionism in a psychologically interpreted theoretical linguistics. To the extent that Chomsky's position is (physically) reductionistic, it reflects a potential conflict, within his philosophy, on the question of reductionism in general. Of course, there is nothing like the naive reductionism of Skinner (1957), with its impoverished ontology, in Chomsky's work, but so long as there is this ontological indeterminacy, we can accuse Chomsky of not having fully renounced reductionism.

Note that at least one interpretation (Steinberg 1982) of Chomsky's philosophy takes it to be anti-physicalistic, which confirms my comments (and Botha's) about the indeterminacy of his position and the subsequent scope for widely differing

interpretations of it:

'Mentalists, such as Locke, Descartes, Putnam, Chomsky, and the Gestalt psychologists would answer each of these questions (Do humans have non-physical minds, do these minds influence their behaviour, should the contents of these minds be studied in psychology and linguistics: PC) in the affirmative.'

(Steinberg 1982: 89)

I have no intention of engaging in a critique of Steinberg's typology of philosophies of mind here (to my mind, it is too narrowly constricted and thus obscures some of the problems involved), but I think that my assessment of Chomsky's philosophy of mind is rather more insightful than his. I have already shown that Chomsky cannot be said to have answered in the affirmative to the question 'Do humans have non-physical minds?' (nor can Putnam, for that matter: cf his 1982 for a statement to this effect), even though he can be said to have answered the other two of Steinberg's questions thus. The most interesting thing about this misinterpretation of Chomsky is that one might well expect an anti-behaviourist like Chomsky to oppose physical reductionism, which is presumably why Steinberg takes him to be a dualist.

In conclusion, I think it is clear that Chomsky wants to avoid BOTH dualism of the Cartesian sort AND any naive physicalism of the Botha sort. Unfortunately, he has not actually come up with anything in the way of coherent proposals for dealing with the dualist-physicalist problem, nor has he adopted an explicitly neutral view. I now want to look at the work of someone (Fodor 1975) who has tried to do both of these.

### 3.3 Neutral psychologism

#### (i) Fodor's alternative to naive reductionism

Fodor's approach to a psychological interpretation of linguistic realities is less radical in its rejection of reductionism than I would prefer it to be, but it is more clearly articulated than Chomsky's and results in the development of a useable research programme, which, I have argued, Botha's does not. This fact is of particular significance in my view, since I stress the importance of methodological, especially heuristic, factors in assessing the merits of philosophies of linguistics.

In his 1968, he argues that one must distinguish between mentalist views in general and the strictly Cartesian view, of the sort I have been discussing, in particular. That is, he points out that a mentalism need not be of the Cartesian dualist sort, and that one could adopt a non-dualist mentalism without going to the behaviourist extreme of rejecting any and all mental phenomena as primary data for a theory of psychology. This is what I have been stressing, and it represents a much more insightful view of the varieties of mentalism (what I call psychologism) than given by Steinberg (1982) mentioned in 3.2. This general approach is, I think, much more interesting, and more likely to be fruitful, than the Smart/Botha reductionist one (recall that Smart explicitly rejects the idea that mental phenomena can count as primary data for a theory of psychology).

Fodor is therefore attempting to propose an alternative to



the two positions, physicalist reductionism and dualism, that I have been discussing. In this sense, his is an attempt at an 'ontology-neutral' psychologism (bearing in mind my comments on the slight inappropriateness of this term in this context: there is an ontological position here, but the neutrality is with respect to the physicalist/dualist dichotomy).

In The Language of Thought (1975), Fodor outlines an alternative to both behavioural and physical reductionism. I will try to assess whether he has succeeded. He points out that we can reject both the view that psychology should deal only with behaviour as its primary data and the view that we can only legitimately take 'inner' processes as primary data if they are assumed to be wholly physical. His approach relies heavily on a distinction between type (or kind) and token. Arguing against physiological reductionism, he claims that it is unlikely that every kind will turn out to be a physical kind, thus:

'The reason it is unlikely that every kind corresponds to a physical kind is just that (a) interesting generalisations can often be made about events whose physical descriptions have nothing in common; (b) it is often the case that whether the physical descriptions of the events subsumed by such generalisations have anything in common is, in an obvious sense, entirely irrelevant to the truth of the generalisations, or to their interestingness, or to their degree of confirmation, or, indeed, any of their epistemologically important properties; and (c) the special sciences are very much in the business of formulating generalisations of this kind.'

(Fodor 1975: 15)

This set of observations is very important for linguistics as a special science. Observation (b) in particular is relevant to what I have said about Botha's set of ontological conditions

and Smart's comments about the status of generalisations in psychology. However, his ontological position differs from the interactionist one (with its strong autonomy thesis) which I propose, as follows: Fodor argues against physical reductionism in psychology by arguing that 'the kind predicates of the special sciences cross-classify the physical natural kinds.' (op cit: 25). This is a clever move which, incidentally, is much more sophisticated and insightful than Pateman's (1983) argument that the objects of linguistic theory are natural kinds (cf 4.2). In adopting this argument, Fodor can oppose naive physiological reductionism by insisting that there may well be no one-to-one correspondence between psychological and neural structure, but rather a state of affairs whereby psychological representations are correlated with neurological function, which is taken to cross-cut neural organisation. This rules out the sort of naive requirement made by Botha, where specific neurons must be correlated with psychological (specifically, linguistic) entities. I have no doubt that this is an improvement on Botha's set of proposals, but I still think that it is not sufficiently removed from physicalism, if we assume that within Fodor's framework, we are still dealing only with realities of a physical sort. That this assumption is justified is made clear by the following:

'Still, if mental events aren't to be reduced to behavioural events, what are we to say about their ontological status? I think it very likely that all of the organismic causes of behaviour are physiological, hence that mental events have true descriptions in the vocabulary of an ideally completed physiology.'

(9)

Fodor argues that this comment does not commit him to reductionism, because of the fact that he opposes type physicalism and proposes what he calls token physicalism. That is, psychological kinds or types cannot be expected to correspond to physical types or kinds, but psychological tokens may well be found to correspond to physical tokens. My reaction to this is to argue that physicalism, even of the token variety, is still physicalism, and that it is mistaken to confuse objects of one ontological category (the physical) with those of another (e.g. the psychological). Such a confusion constitutes reductionism, in my view. However, Fodor's proposals do result in a research programme which is freed from any over-restrictive physiological constraints of the Botha sort, and is therefore de facto non-reductionistic. It may be argued that it is not therefore possible to distinguish in practice between a research programme based on Fodor's de facto anti-reductionism and a programme based on a radically anti-reductionist psychologism (which isn't what I propose, since I do not adopt any version of psychologism). Both would amount to a licence for an autonomous psychologically based linguistics (autonomous in relation to physiology or neurology).

Consider what Fodor makes of these fundamental assumptions. He wants to use them to establish the basis for a research programme which is freed from physiological restrictions and which rests upon the notion of computation as a model for psychological processes, and representation as a fundamental

part of the computation process. That is, he takes mental phenomena (decisions, surveying of options, etc) as primary data for a theory of psychology rather than restricting the primary data to observable behaviour or physiological structure. I take this to be perfectly justifiable: mental phenomena simply are the legitimate object of inquiry for the psychologist; cutting out one's own object of inquiry on the grounds that it is not 'strictly observable' would be an odd suicidal manoeuvre on the part of the psychologist, inherited from positivist confusions regarding the status of observation and the nature of scientific inquiry. It would be equally justifiable, I argue, to insist that the object of linguistic inquiry is simply linguistic structure per se, rather than associated neural (or even psychological) phenomena. However, that is to anticipate the discussion in 5.2.

It might appear that there is a contradiction in Fodor's position on reductionism, since he claims (9) on the one hand that the vocabulary of psychology is quite different from that of physics, but, on the other hand, that mental events have true descriptions in an 'ideally completed' physiology (205). To establish whether there is a contradiction here, consider his rejection of reductionism in detail. Attacking what he takes to be a strong version of reductionism, of the Smart variety, he cites (10) the following representation of reductionist claims:

- (1)  $S_1 x \longrightarrow S_2 y$   
 (2a)  $S_1 x \rightleftharpoons P_1 x$   
 (2b)  $S_2 y \rightleftharpoons P_2 y$

$$(3) P_1 x \longrightarrow P_2 y$$

Here, (1) is to be interpreted as a law in a 'special science' (eg some non-natural science such as psychology, economics, linguistics) which states, roughly, that 'all events which consist of  $x$ 's being  $S_1$  bring about events which consist of  $y$ 's being  $S_2$ .'. The formulae in (2) are taken to be bridge laws which contain predicates from both the special science to be reduced and the physical science (or lower level science) to which the reduction is to be made. The formula in (3) is then the law in the reducing science to which law (1) is taken to have been reduced. It is then assumed that all the laws of the special science in question can be reduced in the same way to laws in the reducing science.

What Fodor wants to do is distinguish between reductionism of the classical sort and what he calls token physicalism, which he takes to be entailed by reductionism. The distinction is as follows: reductionism claims that there are natural kind predicates in an ideally completed physics and that these correspond to each and every natural kind predicate in any of the (ideally completed) special sciences. From this it follows that all events that are described by the laws of the special sciences are physical events. Fodor claims that one can accept the second of these components (all events are physical events) without accepting the first (correspondence of natural kind predicates between special and physical sciences). It is the conjunction of both components (or, just as easily put, the

assertion of the first one) that counts as reductionism for Fodor, and the acceptance of the second one only that counts as token physicalism.

Fodor achieves this separation of the two components by observing that a law in a special science could easily contain natural kind predicates for that science which do not correspond to natural kind predicates in a physical science, even if the statements in the special sciences can all be reduced to statements in the physical science. The example he gives is that of Gresham's Law in economics. If we assume that this law is valid, it tells us something about what happens in monetary exchanges under certain conditions. It may well be the case, Fodor admits, that every such event described by this law can also be described wholly within the vocabulary of a physical science: this is what constitutes his token physicalism (it is not a position I am willing to accept). The rejection of type physicalism which he proposes in this example is as follows: while we may be able to formulate descriptions within a physical science of each and every event subsumed under Gresham's Law, the events so described are not likely to form a natural physical class. That is, the corresponding physical statements describing instantiations of Gresham's Law are not going to turn out to contain natural kind predicates.

That the latter observation is perfectly sensible is made clear if one considers the following example. Let's say that one monetary exchange is the exchanging of sheep, and another is the deletion of characters on a visual display unit linked to a

computer. It is rather obvious that it is not likely to turn out to be the case that these two physical events are included in some naturally occurring class stateable under some physical law. Economic kinds, in short, are not likely to turn out to be physical kinds.

All of this seems transparently obvious and unsurprising from a non-physicalist point of view ; from a Popperian point of view, it is unsurprising since economic systems are world three, not world one, objects, and from an anti-physicalist position such as Itkonen's (4.1), acts of economic exchange are intentional actions (not spatiotemporal events), carried out on the basis of rules which are of a mutual knowledge sort, by conscious agents, and thus qualitatively distinct from the sorts of spatiotemporal events describable under the laws of physics. But this state of affairs is very damaging for the reductionist. Worse still for the reductionist, as Fodor points out (15), is the fact that he must also postulate, in addition to nonexistent physical kinds, natural laws (of the bridge law sort) which will reduce the economic law to the supposed physical law. In my example, this means devising a law which will place sheep exchange and VDU operations within the same natural class (a rather amusing state of affairs, I think).

It is in opposition to this untenable reductionism that Fodor introduces the idea of cross-classification of physical structures and entities by higher levels of organisation. What one winds up with at the level of the reducing science is a

disjunction of predicates rather than a set of natural kind predicates. His diagrammatic exposition of this is as follows:

Law of

special science:

$S_1 x$  ----->  $S_2 y$

Disjunctive

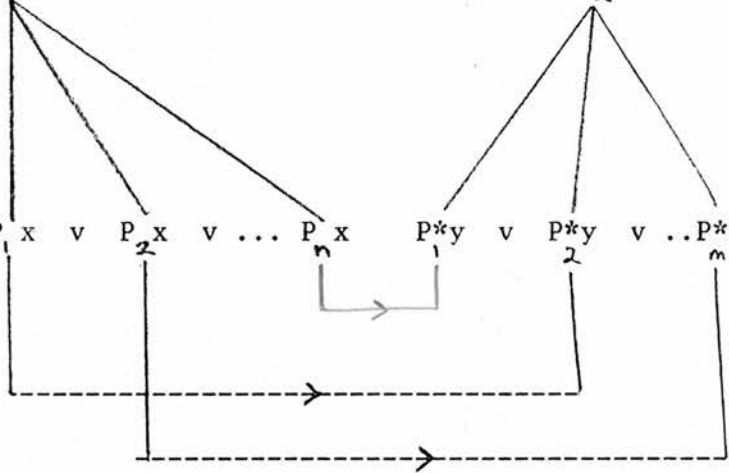
predicates of

reducing science:

$P_1 x \vee P_2 x \vee \dots \vee P_n x$      $P^*_1 y \vee P^*_2 y \vee \dots \vee P^*_m y$

Laws of

reducing science:



This demonstrates the cross-cutting relationship between, say, economic processes and their physical instantiations, or equally between linguistic-as-psychological processes and their physical instantiations.

## (ii) Problems with token physicalism

Fodor is committed to saying that psychological and economic processes can in fact be correlated with purely physical events, if in the cross-classifying way indicated. I think this is to get matters round the wrong way. It has long been argued (this is Itkonen's point, for example, and Saussure's) that it is *only* by virtue of being linked to some economic, psychological (including linguistic) system that a physical event qualifies as being economic, or psychological or linguistic. Clearly, the



significance (linguistic, economic, whatever) of a particular event is instantiated in the physical event, and thus there is an embodying of linguistic or economic form in substance, but one cannot possibly pick out physical properties of events which constitute the linguistic or economic significance in question.

Why not, one might argue. Surely, for instance, one can pick out specific phonetic properties of acoustic or articulatory events which *do* bear formal (linguistic) significance, e.g. voicelessness or aspiration. But voicelessness, aspiration, etc, have no meaning in themselves; they only have meaning *in virtue of the linguistic system*: that is a long accepted Saussurean observation. And the system is not constituted by its substantive realisations, even if the system is instantiated in the substance. Thus the system is not describable in terms of the properties of the physical mode of instantiation. It seems to me that the linguistic (or economic, or whatever) function of physical substance is not describable in physical terms. To check whether it is, we can ask 'In what physical terms would the function of voicelessness in English obstruents be describable?' I find it rather difficult to interpret this as a coherent question, since voicelessness itself, but not its linguistic function in that particular system, is describable in physical terms. This rather suggests that Fodor's schema will not work for linguistics. Thus, the token physicalist's attempt to classify linguistic (economic, etc) systems and conventions as purely physical objects is a

difficult, and in my view impossible, task.

Fodor does not tackle the following point either: if we are to allow for cross-cutting as he does, we must inquire what the ontological status of the cross-cutting is. For Fodor, it would have to be physical. But in what possible way could the cross-cutting of sheep exchanges and VDU operations be physical? There would appear to be no way of stating this particular example of cross-cutting, or of cross-cutting in general, in purely physical terms. Token physicalism, then, turns out on close inspection to be no more viable than type physicalism; it seems that, not only are physical types not equatable with linguistic types, but nor are physical tokens equatable with linguistic tokens. Physicalism, of any sort, is untenable as a basis for a psychologically interpreted theoretical linguistics.

Whether some other psychologically interpreted theoretical linguistics, which is radically non-physicalist, can replace the sorts of psychologism I have discussed is an interesting question. It is interesting to note in this connection that it does seem possible to obviate the dualist/physicalist debate in the philosophy of mind *if* one avoids the psychological interpretation altogether. Thus, Katz' Platonism steers clear of the issue, and can afford to since the object of inquiry, for them, is not psychological, and the problem of specifying what one means by 'psychological' does not therefore arise (note the way in which Katz, throughout his 1981, uses the expression 'the human mind/brain': this reflects an awareness of the mind/body debate and an avoidance of any committment on the subject). Of

course, the Platonist is faced with a multitude of ontological problems anyway, most of which are as difficult as the one I have been looking at (cf 5.1).

The question does arise whether we ought to characterise the object of inquiry as psychological at all, and Platonism constitutes a firm negative response to this question, as do Itkonen's and my positions. Following my anti-reductionist line of argument, I will argue that just as it is mistaken to assume that linguistic reality is physical, so is it mistaken to assume that linguistic reality is psychological in nature.

## CHAPTER 4

## LINGUISTIC OBJECTS AS SOCIAL REALITY

## Introduction

In this chapter, I will consider two quite distinct views of linguistic structure as a social\*, rather than a psychological, reality. These are Itkonen's hermeneutic account of linguistic rules as social conventions and Pateman's view of languages as both natural objects and objects of social theory.

I will argue that, just as the psychological reality approach is ontologically impoverished, so is the social reality account.

\* There is a sense in which my ontological assumptions about linguistic objects, whereby I take them to be intersubjective, non-Platonic, non-normative in nature (cf chapter 5 below), may be said to be social (Itkonen, personal communication, wants to say this, for example). However, they cannot be said to be social in either Itkonen's or Pateman's senses: I regard them (pace Itkonen) as existing largely independently of either social needs and functions, and I do not take them to be objects of social theory, as Pateman does. More on this below.

#### 4.1 Hermeneutics

##### (i) Spatiotemporality and normativity

The most highly developed conception of linguistic structure as a social reality that I am aware of is Itkonen's hermeneutic view, as established in his (1978) Grammatical Theory and Metascience\* (henceforth GTM). In this and in Causality in Linguistic Theory (1983), Itkonen presents a fully developed philosophy of linguistics, of which I want to outline the principal arguments and assumptions. I accept some of them, but I have some rather major misgivings about others. I have already discussed some of Itkonen's methodological remarks in 2.2; here, I am concerned principally with his ontological position, though I will inevitably deal with methodological issues in relation to these.

A central set of distinctions in Itkonen's work is that between spatiotemporal events, which are not intentional in nature, actions, which are intentional and are carried out by conscious agents, and socially agreed upon norms, which constitute the basis for our rule-governed activities. This set of distinctions is important because it means, among other things, that grammatical inquiry is qualitatively distinct from physical inquiry. Physics has as its testing area spatiotemporal events, whereas grammar has as its testing ground intuitive

\* Revised version of Itkonen (1974).

reactions of native speakers, which are actions rather than spatiotemporal events. Defining empirical sciences as those which are falsifiable on the basis of spatiotemporal occurrences, it follows for Itkonen that grammatical inquiry is a non-empirical science.

Two points ought to be made about this conclusion. Firstly, it assumes that we take grammar to exclude psycholinguistics and sociolinguistics, which are both partly empirical (they both involve experimentation and corpus-based investigation, as in physics). Secondly, it assumes that Itkonen's definition of empiricalness should be accepted. Itkonen's statement that theoretical linguistics is not an empirical activity has, not surprisingly, generated a certain amount of hostility (cf Dahl 1975, Linell 1976, Sampson 1976 for some of these, and Itkonen's replies in his 1976). I think we have to accept his distinction, which seems hard to deny; at the least, one would have to provide a theory of grammaticality judgements as spatiotemporal events to counter it.

However, one must distinguish between the validity of this methodological observation and the terminological matter of whether we do actually use the term 'empirical' for only the former of the two sorts of falsifiable activity. If we use 'empirical' for both, we had better distinguish between empirical<sub>1</sub>, say, and empirical<sub>2</sub> to distinguish the two. Itkonen's use of the term 'science' for disciplines which are based on axiomatisation, and 'empirical science' for that

subset whose testing is spatiotemporal in nature seems as clear a decision about the terminological problem as any. And at least Itkonen has a clear conception of what he takes 'empirical' to mean, which is perhaps more than can be said for much current work in linguistics, where the expression is used without its meaning being spelled out explicitly. In fact, one suspects that much of the heat in the reaction to Itkonen's use of the term comes from an assumption that any activity, to be scientific, must be empirical, i.e. an assumption which equates 'empirical' with 'testable'. What most people, including myself, dislike about Itkonen's claim is that he does appear to be saying that grammatical theories are not testable, where testable means falsifiable (this is what I take it to mean, following Popper).

Let's distinguish three positions on this matter: (i) the cases in which the theories within a discipline are falsifiable spatiotemporally, (ii) those where they are falsifiable, but not spatiotemporally, and (iii) those where they are not falsifiable at all. Under Popper's definition of science, where scientific theories are characterised by their falsifiability, only those under (i) and (ii) count as scientific endeavours. Under Itkonen's, where sciences are characterised by axiomatisation, all three may count, so long as the discipline in question is of the axiomatic sort. Now to theoretical linguistics. There are clearly those who feel that it belongs in category (i). I am not one of them, nor is Itkonen. Then there is Itkonen's position, which is that theoretical linguistics belongs in

category (iii). It is not clear to me whether Itkonen allows that there is a category (ii), but I do, for reasons to be explained, and it is here that I think theoretical linguistics belongs. Thus, I agree with Itkonen that linguistic theories are not falsifiable via the observation of spatiotemporal events, but I disagree with him that they are not falsifiable at all. I will have more to say about this in due course.

So much for the terminological and conceptual preliminaries. Continuing with the fundamentals of Itkonen's proposals: connected with the above distinctions is the distinction between observable regularities (for instance, the regularities observable in the movements of planets and stars within a solar system) and spatiotemporal manifestations of social norms, i.e. patterns of observable behaviour which are rule-governed. It is clear that the former are properties of events whereas the latter are properties of actions. Or it is when we characterise these. In the former case, there is no question of the observed regularities being correct in any sense, whereas correctness in the latter case is an essential part of the action (or so Itkonen wants to claim; I'll go along with him for the sake of the argument). To take an example, the uttering of the following could not be counted as a potentially falsifying fact about our claims about tense marking in the English verb group ('verb phrase' if you don't like verb groups or think that auxiliaries fall outside of them; 'sentence' if you think auxiliaries fall outside of the verb phrase; I'll



ignore those who want to argue about the auxiliary/main verb distinction):

Maynard have be eats the spinach.

We simply disregard the fact that this has been uttered, on the grounds that it is 'incorrect' (In Itkonen's sense) and does not therefore even count in our attempt to falsify particular grammatical hypotheses, at least not if it is taken to represent a syntactically well-formed sentence of English. On the other hand, and in contradistinction to this case, no observable event in the solar system can be written off as being 'incorrect', since the term does not apply to physical phenomena (this isn't to say that potentially falsifying observable events can't be written off on other grounds).

I will not pursue the question of the status of the expression 'observable' in relation to 'events' here, though it is interesting to note that Itkonen's use of this fundamental distinction relies rather heavily on some sort of observation vs theory distinction, which is rather worrying, considering the theory-laden nature of observation. Those of a variationist and anti-competence vs performance disposition will be somewhat disturbed at the use of the notion 'correct' here, and at the claim that we can simply ignore much of the data in a recorded corpus on the grounds that it does not count as instantiating well-formed expressions in English. Itkonen has always allowed that there are non-clear cases when it comes to grammaticality, but claims that these do not impugn the status of the clear cases. For my part, I am happy to ignore the worries of the

variationists, and to dispense with much of what is in a given corpus; it is clear that corpora play little or no part in theoretical linguistics.

Itkonen further points out that while it is clear that actions have a spatiotemporal aspect, it is not the case that they can be reduced to purely spatiotemporal events. This, I pointed out, is true of my example about acts of monetary exchange in chapter 3: what constitutes an act of monetary exchange is not the physical event of, say, coins passing from one hand to another, but the underlying conceptual scheme which determines what will and what will not count correctly as a monetary exchange. Actions, then, are irreducible to events, and furthermore, the norms which function as the basis for actions are not reducible to actions. Thus, if actions are characterised by the fact that they are carried out by conscious agents, and are therefore intentional in nature, as opposed to events, which are not, then intentionality and normativity are not reducible to ontologically more primitive factors.

This approach is hermeneutic in that it stresses the hermeneutic understanding we achieve in investigating our rule-based social actions as opposed to the observation we engage in when investigating spatiotemporal phenomena. While I wish explicitly to accept Itkonen's view of physical sciences as being qualitatively distinct from sciences such as theoretical linguistics, it is unfortunate, I think, to restrict the term 'understanding' to the knowledge we achieve in non-physical

investigation; under almost any philosophy of science, it is common practice to assume that we do in fact achieve understanding of the physical universe within the physical sciences. Perhaps it is as well to mention another terminological point here. Hermeneutic sciences for Itkonen are opposed to physical sciences, and the term 'hermeneutic' is taken to characterise all those schools of thought whereby a qualitative distinction is made between physical and human sciences. This he opposes to the 'positivistic' view which takes physical and human sciences to be methodologically parallel. Thus Chomsky is a positivist under this scheme, whereas the approach to the methodology of theoretical linguistics adopted by Lass (cf 2.2) is hermeneutic.

My approach to this question is not easily characterised under Itkonen's dichotomy; whereas I agree that the methodology and object of inquiry of theoretical linguistics is qualitatively distinct from those of the physical sciences, I still take them both as involving the construction of falsifiable theories about their respective objects of inquiry, and attempting to falsify them in their respective manners. It is this which, under my Popperian definition of what counts as a science, characterises both as sciences. As I've indicated, I am dealing with a trichotomy here whereas Itkonen is dealing with a dichotomy.

Itkonen's distinction between positivism and hermeneutics is likely to cause confusion since the term 'positivism' is also used to refer to the sorts of philosophy of science proposed by

the Vienna Circle, as discussed in chapter 2. Under this latter definition of positivism, Popper is clearly not a positivist, but a realist (he claims responsibility for having 'killed' logical positivism, and I've shown, in 2.2, how his views differ from theirs), whereas under Itkonen's definition, Popper *is* a positivist, since he adopts a version of methodological monism: he takes human and non-human sciences alike to be characterised by the same method. Itkonen is aware of this, and notes the problem in his GTM; I mention it mainly to avoid confusion with terms discussed in earlier chapters, and to note an unfortunate terminological problem (actually, terminological problems are rarely 'merely' so, and this is certainly true in this case: the explication of the terminological problems reveals interesting differences between Popper's, the Vienna Circle's, and Itkonen's philosophies of science).

It is clear that Itkonen wishes to see norms and actions as entirely social, rather than psychological, realities, thus:

'It is possible to abstract from every action the intentional element which, properly speaking, constitutes an action qua action. (This 'intentional element' is to be understood, not as some psychical substance, but as a 'pattern'.)'

Furthermore:

'....intentions, which are necessary constituents of actions, must be at least potentially conscious: to do something, one must be able to know, at least under some description, what one is doing. Thus knowledge is, in principle, inseparable from action....knowledge is necessarily social.'

(GTM: 122-123, emphasis in original)

It should be stressed just how strong this claim is: that norms are not only socially established as the basis for our actions, but are entirely constituted by their social context. They constitute a kind of mutual knowledge, which he refers to as 'common knowledge', an inter-subjective reality:

'Common knowledge literally constitutes concepts and rules as what they are, whereas their (social) existence is independent of the subjective knowledge of any individual person...'

(op cit: 322, n.67)

This argument derives from his adoption of the Wittgensteinian argument against the possibility of private languages; I will summarise Itkonen's statement of it now, as it is relevant to the social vs psychological distinction which constitutes the rationale behind this, the preceding, and the following chapters.

Firstly, he argues (GTM:109-110) that there can be no concept of 'I' without the corresponding concepts of 'you', 'we' and 'he/she'. From this he concludes that an individual could not privately invent and follow a rule of language since, without the concept of others, the individual in question could not even have the concept of 'myself'. This appears to me to be fairly weak. Notice that Itkonen does not mention the concept 'it'. I would have thought that, even if one existed in a world without other people, one could acquire the concept 'myself' in contradistinction to the concept 'it' (relating to inanimate and animate, non-human, objects).

However, his second point is more convincing. Here he

takes the case of someone's living in the public world and speaking an intersubjective language, but then attempting to invent a private language which is independent of any intersubjective language. Each term in this language would have to refer only to purely subjective experiences which are private to the individual in question, and which are therefore not susceptible to public identification. Itkonen makes the Wittgensteinian point that if such experiences do exist, there is nothing that can be said about them, and that if this is so, we cannot even begin to discuss the question whether we could devise a private language referring to them. I intend to accept this argument as being the most convincing of all his anti-private language arguments; the main purpose of this discussion is to establish the fundamentals of Itkonen's philosophy of linguistics and to criticise them, rather than to attempt to tackle the private language argument per se and its related philosophical problems, so I shall not devote much space to arguments against the possibility of private languages. (It does strike me that we may well have private experiences which turn out to be similar to other people's private experiences, and that we try to establish this via language, but I assume that this is irrelevant to the point that language is necessarily public and that the notion 'private language' is therefore incoherent or contradictory, a point which I accept.)

A third and final argument is cited by Itkonen against the possibility of private languages; I mention it only briefly as

it strikes me as being rather unimpressive. Here he considers the objection I raised above that one could conceivably have the concept 'myself' in a world where there are no other persons but only natural objects. His reply to this is that the concept 'myself' thus acquired would not in any way resemble our concept of person. I take this to be an unimpressive reply in that it is circular: our concept of person is arrived at intersubjectively (or so Itkonen claims) and any concept of person arrived at without reference to other persons is not really a concept of person because it is not arrived at intersubjectively.

However, he does make the point that one could have no independent checks, in such a situation, on whether one was following a rule (of language) correctly. One would simply use a given linguistic element in exactly the same way as one first used it, with the result that one could never manage to evolve the notion of correctness.

All of this is important for Itkonen because from it he concludes that rules of language are social rather than psychological realities. Among other things, this results in a rejection of the Chomskyan competence/performance distinction by Itkonen, since knowledge of linguistic rules is inseparable from language use, both being social in nature. And while he would not deny that we have some sort of internalised representation of linguistic rules, such rules are primarily objects of common knowledge rather than objects of a subjective, psychological nature. (Itkonen does allow that psycho- and socio-linguistics have their own methodology and object of

inquiry, but he takes these to be distinct from those of theoretical linguistics: more on this below.)

Before leaving the private language issue, I should stress that Itkonen's use-based view of linguistic rules is not only social but also functional: rules exist in response to some social function. I want to argue below, following Lass(1980), that this functionalism is untenable, and that one can reject the possibility of private rules without having to accept a functionalist interpretation of linguistic structure. This is done by distinguishing between the clearly social processes whereby language emerges and the non-social aspects of linguistic structure as a product. I deal with this later, however.

I want to introduce one further distinction of Itkonen's at this point, that between rule and rule-sentence, and to point out what I think is a rather serious problem for him. The term 'rule' may refer, either to a socially constituted norm, outside of the grammarian's analysis and available to us by means of intuitive awareness, or to a statement in a grammarian's analysis of a language. The first of these is a rule in Itkonen's dichotomy, the latter a rule-sentence. The distinction is one between an atheoretical object (rule), which is normative in nature, an object of mutual knowledge, and a theoretical one (rule-sentence).

Just what Itkonen takes to constitute the object of theoretical linguistic inquiry is an interesting question. I



want to suggest that he does not succeed in isolating an object of inquiry. It has become clear that use is central to Itkonen's conception of language, and this suggests that speech actions may be possible candidates as Itkonen's primary objects of inquiry:

'According to this functional or pragmatic conception, the speech act\* is the primary unit of language. Within the speech act, one may go on to distinguish between the level of intersubjective interaction between speaker and hearer....and the level of a reference to extra-theoretical reality....The traditional concept of a 'sentence' proves to be a unit secondarily abstracted from the (primary) speech act.'

(GTM:120)

In addition to this:

'Sentences and types of speech act are equally normative entities: the former are concepts exemplified by utterances which in turn are results of act-tokens exemplifying the latter. Speech act grammars analyse the concept 'correct (type of) speech act' just as sentence grammars analyse the concept 'correct sentence' ....Since sentence grammars are much more well-established than speech-act grammars, it is understandable that I shall concentrate on the former, in spite of the fact that they have just been shown to be logically secondary with respect to the latter.'

(op cit)

I confess to not understanding all of this. Let's assume that we follow Itkonen for the time being in taking traditional grammars to be 'analysing the concept 'correct sentence' ' (though most theoretical linguists wouldn't, I suspect). Even then, it is not clear precisely what relationship holds between sentences, utterances, and 'speech

\* The term 'speech act' here means speech action, and is therefore distinct from, and not to be confused with, the normal Searlean/Austinian use of the term.

act types'. Itkonen's statement that sentences themselves are concepts is not readily understandable; nor is his claim that, as concepts, they are 'exemplified' by utterances, which are said to be 'results' of act-tokens 'exemplifying' speech-act types, I am not at all sure what this relationship 'exemplification' amounts to. Nor can I see what precisely Itkonen takes to be the difference between speech act tokens and utterances. Lyons' (1977:26) distinction between utterance act and utterance signal springs to mind here; perhaps utterance signals are equivalent to Itkonen's utterances, and utterance acts equivalent to speech act tokens. At any rate, the onus is on Itkonen to spell out these rather vague remarks more explicitly; they do, after all, constitute the basis of his claims as to what constitutes the primary object of study in theoretical linguistics.

One thing that does emerge from all this is that sentences do not play a particularly central role in Itkonen's scheme of things. Furthermore, if Itkonen is serious in insisting on a use-based view of language where norms as the basis for speech actions are central, it is not clear that he need utilise the notion 'sentence': one could easily dispense with it altogether and assume that the grammarian is engaged in formulating and then systematising rule-sentences which define 'correct speech act type', whereby speech act types are instantiated by speech act tokens.

The most worrying aspect of this schema so far is that it not only diminishes the status of the sentence as a unit of

analysis, and as theoretical construct whose methodological role in grammatical inquiry is crucial but it allows us to dispense with it altogether. This is worrying because the construct 'sentence' does so much work in current grammatical frameworks, and is in fact a central part of the methodological basis of the generative enterprise: one would have to have very strong grounds for dispensing with it, or taking it to be of derivative status, as Itkonen does. Furthermore, when one considers that Itkonen takes his characterisation of AL to represent the tradition in grammatical inquiry, and that this tradition has been almost exclusively concerned with the sentence as an object of inquiry, one wonders just how close Itkonen's characterisation actually comes to mirroring what grammarians actually do.

All of this is quite distinct from the standard Chomskyan conception of the object of inquiry which, with languages as sets of sentences and grammars as sets of rules which generate these, taking the object to be the 'internalised grammar' or even the principles underlying this. It is different on two (closely related) major counts: in its assumptions as to (a) what the object of inquiry is and (b) what the ontological status of that object is. These may even amount to the same thing in this case. Itkonen does not even allow that the object is a set of any sort, even of rules-as-norms.

As I've said, it is doubtful whether Itkonen's framework of assumptions even ties in at any point with the sorts of grammatical analysis that have actually been done (despite his

claim, which I agree with, that the tradition in linguistic inquiry for centuries has been of an 'autonomous' linguistics sort). A 'grammar' (I doubt if the term is appropriate) which has as its only, or as its primary, task the formulating of rule sentences specifying or defining 'correct speech act type' is a rather uninteresting object. However, Itkonen does allow, as I pointed out in 2.2, that grammars are 'systematisations' of sets of rule-sentences. Unfortunately, these are nothing more than systematisations since the systematisation itself is interpreted instrumentally, such that one does not assume that there is some extratheoretical object of which the systematisation is a description. Thus the bulk of grammatical description and analysis carried out in the past is little more than a systematisation of rule-sentences. My proposal is that all of these systematisations were actually *about* something, and something other than rules of the sort described by the rule-sentence 'In English the definite article precedes the noun' (GTM:158).

This is best demonstrated by considering this innocuous-looking rule-sentence, which Itkonen takes to be descriptive of a norm. It is true that such a rule-sentence can easily be related to speech actions in the way that Itkonen describes: it is concerned with linear ordering, and our actions display temporal linearity in what Itkonen calls their spatiotemporal aspect. There are serious problems, though, in maintaining the claim that what the rule-sentence describes is entirely normative. Even simple and relatively uninteresting rule-

sentences like this involve more than simply linearity. Consider why this rule-sentence is so uninformative: it does not distinguish between immediate and non-immediate precedence, nor does it tell us anything about what elements may or may not intervene between the definite article and the noun, whether those intervening elements are more closely bound up with the noun or the article, what elements may follow the noun, how closely they are bound up with the noun and in what way, and how all of the elements preceding and following the noun (and we'd be obliged to say 'head' noun) relate to the article. We'd need some account of when the article can and cannot be omitted too. In short, all of the concerns of the grammarian need to be dealt with before we could turn the rule-sentence into an insightful statement about the language in question.

Of course, it is evident that in attempting to improve on this uninteresting and rather uninformative rule-sentence, we need to appeal to notions such as function, constituency, hierarchicality, modification, complementation, and so forth. And just how these can be characterised as *normative* is not clear, and they would have to be for Itkonen's claims to stand. It will not do to say that such notions are part of some systematisation of rule-sentences, since rule-sentences *must* contain grammatical terms, and grammatical terms are defined by the grammatical theory they are contained within, which, of course, is what Itkonen insists on calling a 'systematisation of rule-sentences'. Itkonen gets it the wrong way round, surely:

rather than the grammatical framework being built up as a systematisation of sets of rule-sentences, the grammatical framework is what allows us to frame rule-sentences in the first place. And if the referents of the grammatical terms aren't normative, rule-sentences cannot be about normative states of affairs. Whether such terms can be defined as conventions in a normative way remains to be seen; Itkonen has not done this. We can conclude that even if rule-sentences describe purely normative objects, sets of these alone would be hopelessly insufficient as grammars.

This seems to me to be a fairly major defect in Itkonen's proposals: he *excludes* the objects of grammatical inquiry (sentences and their properties) in his philosophy of linguistics by claiming that the objects of inquiry are normative rules describable by means of rule-sentences. I've tried to show that, even if we wanted to accept that rule-sentences describe rules-as-norms (which I do not), the terms contained in rule-sentences have not been shown to refer to normative objects. And if the terms contained in rule-sentences do not refer to normative objects, I cannot see how the rule-sentences themselves can be said to refer to normative states of affairs.

There is a further problem, closely connected to this one, with Itkonen's views on what grammars are: the question of whether they are testable. Itkonen allows (GTM:252) that a grammar must be more than a collection of rule-sentences, that it must be a systematisation of those which captures

generalisations, but claims that these are not themselves falsifiable. He further claims that simple rule-sentences of the sort he cites about the relative order of the definite article and 'the noun' in English are not testable, since they are either clearly true or clearly not true, according to our intuitive grammaticality judgements. This is mistaken, however, and the best way to demonstrate this is to show that his example rule-sentence, as it stands, can be shown to be *false* ( and therefore falsifiable), and to consider why it is false.

Consider: *I knew the man the woman shot.* This sort of sentence, with no overt relativiser, is grammatical; it contains an instance of a definite article following a noun rather than preceding it. Thus the rule-sentence, as it stands, is false. However, consider why it is false. It is because it is not sufficiently theoretically sophisticated. We need to say a great deal more about constituency, function and order to change it into a statement which fits the facts and tells us something about the object of inquiry. This is a case, surely, of venturing a hypothesis about the object, amending it in the face of contradictory facts, and doing so by means of constructing a theoretical framework which not only accomodates the facts, but allows us some insight into why the facts happen to be as they are.

This demonstrates that there is no simple dichotomy between rule-sentences and grammars: all rule-sentences will be theoretically informed, and the better the theoretical framework

which determines their meaning, the better they will be as rule-sentences. This exemplifies what I have said about the relationship between theory and observation in relation to physical sciences. Although the data in grammatical inquiry are not, as Itkonen so accurately points out, spatiotemporal events, what I have been calling 'observations', and what Itkonen calls rule-sentences, interact with and are related to theoretical assumptions, in a way which is very similar way to the way in which observation and theory inter-relate in the physical sciences. The realist assumptions which I have made appear to be better suited to explicating the nature of method in theoretical linguistics than those adopted by Itkonen.

These are what I see as the principal problems in Itkonen's proposals; I now turn to another aspect of Itkonen's interpretation of linguistic objects as social objects, the functionalism which is evident throughout his work, which I take to be unsatisfactory in various ways.

#### (ii) The Failure of Functionalism

I begin by restating Itkonen's position on the use-orientated nature of language. He states that the competence/performance distinction is to be rejected on the following grounds: if the definition of 'performance' includes what Itkonen calls speech actions, and if competence is the knowledge underlying performance, but distinct from it, then the distinction is invalid since, in Itkonen's view, speech actions are the primary object of inquiry, and the knowledge we



investigate in linguistic inquiry is necessarily social, and embedded in use. Thus the centrality of the term 'correct', referring to actions, in Itkonen's work.

There are two distinct points at issue here. One is the question of whether we want to make a distinction of the competence/performance or of the langue/parole sort (in this case, we needn't worry about precisely which of these distinctions we make, so long as we are only treating of the question of distinguishing between linguistic behaviour and something 'underlying' it). The other is the question of what sort of distinction one wants to make if one considers that it is necessary to distinguish linguistic behaviour from something over and above that behaviour. On the first count, it is not clear to me what Itkonen's position is. On the one hand, he seems to want to insist that we not divorce linguistic objects from actions. On the other hand, he seems to do precisely this, in that he distinguishes actions and the socially constituted norms which act as their basis. On the second count, it is perfectly clear that Itkonen, insofar as he is making a distinction here, is insisting that the nature of the knowledge 'underlying' speech actions is social, that is intersubjective, and not psychological and therefore private. To repeat the quotation cited above, 'common knowledge literally constitutes concepts and rules as what they are....their social existence is independent of the subjective knowledge of any individual person' (GTM:322, n.67).

I am inclined to agree that the object of inquiry is intersubjective in nature, but not in the normative way that Itkonen suggests. Rather, I want to suggest that the intersubjectivity of linguistic objects is something over and above social conventions and social functions. I have shown above what I take to be the principal problems with the claim that linguistic objects are normative; here I will concentrate on why I think linguistic objects exist independently of their social function and why stressing function as a means of explicating the nature of linguistic objects is bound to fail.

Consider the rule-sentence we have been discussing, ignoring its inadequacies for the moment, and assuming that it accurately describes a piece of mutual knowledge of the sort that Itkonen describes. What social function does this norm fulfil? We may say that having a rule (norm) at all fulfils our social need to perform speech acts in an interpretable way, but this is a feature of rules in general. What of the substantive content of the rule? Definite articles may, in principle, either precede or follow their (head) nouns. What the rule does is to tell us which is the case; it does not matter, from the point of view of function, *which* is the case, so long as there is a determinate state of affairs which will allow us to communicate. Either way, it is only by virtue of being a rule that this particular rule could be said to have any function. And, in fact, if *both* possibilities were correct, then the rule-sentence 'In English, the definite article may either precede or follow its (head) noun' would describe the social norm.

It is clear that in none of these cases can the rule be said to fulfil any specific function. This is because of the following rather simple fact: once we accept the very general idea that rules of language serve the general function of enabling intersubjective communication to take place, we must proceed from the general to the substantive, and begin looking at substantive questions as to the structure of the language in question. Because, in investigating substantive issues, we presuppose this very general fact about rules, it is redundant, and therefore uninformative, to state, for any given rule, that it fulfils this very general function. Whatever we might find out about definite articles and nouns in English, the fact that their linear ordering can be interpreted as a socially agreed upon norm is not going to count as finding out *anything* about them. And this very general point concerning the overall function of rules is all that we can come up with in terms of interpreting rules as use-orientated. We can never say what *particular* social function is being served by this, or any other, rule, only that, qua rule, it is social in nature.

Interestingly, if we take Itkonen's point about the questions WHAT and HOW, we discover a contradiction. Consider the following observation made by Itkonen in his review of Katz (1981):

'Linguists typically try to answer two different types of question: What are the properties of, for example, English sentences? and How are such properties acquired, stored, perceived, or changed? The what-question, to be answered by grammatical theory, is the logically prior one, which means that the how-question, to be answered by different forms of causal

linguistics, is in reality a what-and-how-question.'

(Itkonen 1983b:240)

In this I am entirely in agreement with Itkonen. However, his philosophy of linguistics tells us to substitute the question 'what are the properties of English sentences' for the question 'how do these function socially? '. He thus fails to satisfy his own methodological criterion, and ought to abandon either the criterion or the set of ontological assumptions on which it is based. I suggest that the criterion if valid, that we ought to distinguish what and how questions, and that it is social functionalism that should be abandoned.

Interestingly, both social functionalism of this sort, and psychologism conflate the what and how questions. If we accept that they ought to be distinguished, then we need a new set of ontological assumptions to replace social functionalism and psychologism. This I attempt to do below (chapter 5). First, I consider another attempt at characterising the object of inquiry as a social reality.

## 4.2 Naturalism

### (i) Languages as natural kinds

While it is true that both Pateman and Itkonen take the object of inquiry in theoretical linguistics to be social in nature, they differ considerably in their methodological and ontological views. Methodologically, Itkonen opposes what he calls positivism, the view that both the natural and the human sciences exemplify the same methods. Pateman's approach, on the other hand, is entirely positivistic in this sense. This is largely because he takes languages to be 'natural kinds', parallel with the sorts of natural kind investigated in the natural sciences. Furthermore, he sees languages as objects of social theory. It is this combination of languages as natural kinds and as objects of social theory which constitute the core of Pateman's proposals.

As ever, some terminological and conceptual preliminaries are necessary before I go into Pateman's proposals in any detail. And these preliminaries turn out to be more than merely terminological. I have been using terms like 'linguistic structure' and 'linguistic objects' to describe the object of grammatical (theoretical linguistic) inquiry. I pointed out that the term 'language' was insufficiently narrowly defined to be useful for my purposes, since it does not allow us to distinguish the objects of sociolinguistic, psycholinguistic and theoretical linguistic inquiry, nor very general, non-linguistic issues ('language and sexism', for example) from linguistic

ones. Pateman does, however, use the term 'language' throughout his work. In doing so, he emphasises the distinction between the expressions 'a language' and 'language'. He distinguishes both of these from the expression 'languages'. I offer a few comments on these, principally to establish whether, and when, Pateman and I are talking about the same thing.

Taking the first two of these ('a language' and 'language'): there can be no doubt that there are many problems relating to both of these terms. One is the question of whether the accepted view of terms such as 'English', 'Urdu', etc as socio-political notions is in fact justified, and another is the question of what we decide to subsume under the very general heading 'language'.

Consider the commonplace (in linguistics) distinction between a derivative socio-political notion such as 'Urdu' and some more specifically linguistic notion (a language as a set of sentences) which would count as one of my 'linguistic objects'. In an attempt to elucidate the terminological problem one faces with the expression 'a language', I want to distinguish between 'a language<sub>1</sub>', which corresponds to the standard generative definition of a language as a set of sentences, and 'a language<sub>2</sub>', which is the sort of thing we are referring to in propositions of the sort 'Urdu is a language distinct from Hindi'. That these are distinct is clear from the fact that we have many linguistic means of assessing the truth value of propositions about the objects described under 'a

language  $\phi$ ', but no linguistic means of assessing the truth value of propositions such as 'Urdu is a language distinct from Hindi'. There is more to this than the fact that there are difficulties in maintaining a language/dialect distinction: our linguistic theories do not treat of objects such as 'Urdu' and 'Hindi', and consequently there is simply nothing at issue, from the point of view of linguistic theory, when it comes to arguing over the truth or falsity of such propositions. Whatever arguments over such propositions might be about, it seems clear that they are more likely to be about socio-political matters than linguistic ones.

If this distinction is well-founded, then we can define pluralities of these by distinguishing between 'languages<sub>1</sub>', in comparing and making theoretical claims about the objects which grammars define, and 'languages<sub>2</sub>', the sorts of object under discussion in the 'Are Urdu and Hindi distinct languages?' cases. The question then arises as to the status of the expression 'language'. I can see a clear sense in which we can talk of 'language' when we are treating of 'languages<sub>1</sub>', but not when we are dealing with 'languages<sub>2</sub>'. This is unsurprising if indeed discussions of the sort centred around the Urdu/Hindi distinction are not linguistic questions (not questions which bear upon linguistic theory).

I am inclined, therefore, to accept the standard Chomskyan line on the socio-political nature of (the referents of) expressions such as 'Urdu' in propositions such as these. However, like many of the most fundamental notions in Chomskyan

linguistics, it is a line that has come to be challenged of late. Both Hurford (forthcoming) and Katz (1981) want to claim (albeit from entirely distinct methodological viewpoints) that expressions such as 'English' and 'Urdu' may reasonably be taken to be used to refer to objects of linguistic theorising. I discuss Katz' specifically Platonistic version of this claim below; for the moment I want to consider Hurford's remarks. He maintains that 'the study of language starts most naturally with languages' and that

'although in some sense fuzzy, languages are in practice sufficiently clearly defined to be susceptible to solid factual description, which in turn can support theorising. Discussions of how many languages there are spoken in the world correctly hedge their statements by pointing out the indeterminacy involved in the language/dialect distinction, but are usually not thereby deterred from concluding that there are in the region of 4 - 5000 of them. So languages are, at least roughly speaking, countable....it is clear that languages are very generally known as individual, describable, countable, and nameable objects. And it is with these pretheoretically available objects that the study of language can get to work.'

(Hurford forthcoming: 19 - 21)

My reaction to this is that what matters here is not so much the problem in maintaining the dialect/language distinction, but the factors which create the problem in the first place. If languages<sub>2</sub>, as the objects which expressions such as 'Urdu' denote, are not discrete (if it is simply not possible to identify their boundaries), then they are not countable in principle, and the question 'How many languages are there spoken in the world?' is methodologically ill-formed. We cannot answer it because it has no answer; it has no answer because the referents of expressions such as 'Urdu' and



'English' are not countable (because they are not discrete). I have also suggested that they are not linguistic. Rather than simply assert this, I want to observe that an object can reasonably be called linguistic in nature if it needs to be referred to in a linguistic theory. But there is no motivation for theoretical constructs such as 'Urdu' 'English' or 'a language<sub>2</sub>'. Discussions in which the discussants are not deterred from answering the question ('How many languages<sub>2</sub> are there spoken in the world?') are therefore methodologically ill-conceived. And such questions have no linguistic status.

I am unable, however, to resolve the problem of just what 'Modern Greek' means in the expression 'a generative grammar of Modern Greek'. Given that I take 'a language<sub>1</sub>' to be an abstract, intersubjectively real object (not of the Platonistic sort: cf chapter 5), and that I take generative grammars to define such objects, it is not clear just how justified we are in referring to a given grammar of this sort as a grammar of, say, Modern Greek. But I am also not certain that there is anything *at issue* when it comes considering how justified we are in thus referring to them.

If Hurford's remarks above cause me some measure of disquiet, his comments (20 - 21) on the undefinability of expressions such as 'Urdu' or 'English' in political terms do not. I agree with him that there has never been a political definition of such terms, and that this is probably not possible. However, the standard Chomskyan line need not include

the assertion that this is possible. One need only remark that whatever discussions of the above sort about 'Urdu' are concerned with, it is more likely to be socio-political/cultural than linguistic. One needn't say that 'Urdu' is *defined* socio-politically.

If it is important, and I think it is, to maintain the distinction between 'a language' and 'language', then it is important not to use the term 'languages' ambiguously to mean, on the one hand, languages<sub>1</sub> and on the other, to mean languages<sub>2</sub> or to indicate the more general notion 'language'. It seems to me that Pateman does just this.

The third chapter of his 1983 work Language as an Object of Social Theory is entitled 'What is a language?', and concerns the *meaning* of the expression 'a language':

'Though this chapter seeks to order some of the uses of 'a language', its concern is not with verbal definitions but with real definitions - definitions of the essence\* of a thing - and in that way is a contribution to ontological arguments about languages - about their mode of existence.'

(80)

There are two expressions being used here, 'a language', which may mean either 'a language<sub>1</sub>' (a set of sentences) or 'a language<sub>2</sub>', and 'languages', which, again, is used in both the 'languages<sub>1</sub>' and 'languages<sub>2</sub>' senses. Unfortunately, Pateman confuses 'languages' with 'language'. He begins by discussing Smith and Wilson's (1979) account of the notion 'a language' and

\* Real definitions need not be essentialist: cf chapter 1.

concludes that it is confused, or at the very least, confusing. It is best to cite the material from S&W which he uses. Firstly, concerning 'a language':

'...a language is definable in terms of a set of rules.'  
(S & W: 13)

Secondly:

'Since everyone will have heard a different set of utterances, it is not surprising the he comes to possess a slightly different grammar from those around him. Strictly speaking, then, we cannot talk of the grammar of English, but only of the grammars of individual speakers of English.'

(26)

Pateman observes that the reader is left concluding that 'a language has been given a definition which excludes English from the class of languages, though 'English' continues to be used in some pre-theoretical (or alternative theoretical) sense to gesture at something unavoidably present but not theoretically allowed for' (Pateman 1983:81).

There are undoubtedly problems for S&W's definitions, relating to the status of 'idiolects' and the intersubjective nature of linguistic rules: it should be clear from the preceding chapters that I follow Itkonen in not accepting that one can legitimately talk of the grammars of individual speakers. In this connection, note S & W's reference to the fact that each speaker has heard a different set of utterances: this fact is relevant to the Chomskyan notion of grammar, but its significance is not that it leads us to conclude that each speaker has a distinct, individual grammar. Rather, it leads Chomsky to conclude that the particular set of utterances that a

given individual is exposed to is not especially significant.

However, I do not think their account is confused in the way Pateman takes it to be. If we adopt the Chomskyan account of 'English' as a derivative, non-linguistic notion, of a socio-political sort, as I have, and if the expression 'a language' is defined linguistically in the usual Chomskyan way as a set of sentences ('a language<sub>1</sub>'), then we see clearly what linguistic content expressions like 'English' might have. As I have suggested, questions such as 'Is it the case that Scots is a language distinct from English?' or 'Is Urdu a language distinct from Hindi?' are clearly non-linguistic questions. The expressions 'a language' and 'English', if they refer to anything in these cases, do not refer to linguistic objects. When we speak of a generative grammar of English, on the other hand, we cannot be claiming that the set of sentences, the language<sub>1</sub> generated by our grammar, can be referred to as English in any strict way. Whatever the nature of the abstract object we attempt to characterise when we devise a generative grammar which generates a particular set of sentences, there is no clear sense in which that set can be referred to as 'English'. Nor, as I've pointed out, is it possible to conceive of a linguistic theory which had as a theoretical construct the notion 'English'.

With these preliminary remarks on the notions 'a language', 'language', and 'languages', Pateman goes on to present his view of the ontological status of languages. He wants to claim that 'a language' is both a linguistic entity and a 'natural kind'.

In attempting both to explicate 'a language' and advance his 'natural kind and linguistic kind' definition, he lists five different answers to the question 'what is a language?'. Note that the question is neither 'what is language' nor 'what are languages?'.

Pateman's five answers provide a set of (apparently) competing positions on the subject. I reproduce them here:

- ' (I) A language is a natural kind. (NATURALISM)
- (II) A language is an abstract object (PLATONISM)
- (III) A language is a name given to a set of objects (for example, a set of grammars, lects or idiolects, characteristically taken to be properties of individual speakers). (NOMINALISM)
- (IV) A language is a social fact, and that social fact is also a (or in a stronger version, the only) linguistic fact. (SOCIOLOGISM)
- (V) A language is a social fact, but that social fact is not a linguistic fact. (DUALISM, for want of a better word to indicate a view of reality as stratified and with at least 'weak' emergent properties)'

(op cit:81)

A few observations need to be made before I examine these positions: the expression 'a language is a (particular kind of) fact' is slightly odd. Since only propositions can be facts, and not languages directly, I take this to be a circumlocution for 'the existence of a language is a (particular sort of) fact' or perhaps 'That a given language exists is a fact of a particular

sort, depending on what sort of existence one supposes a language to have'. Alternatively, we can dispense with the use of the term 'fact' and simply say that 'a language' is a particular sort of entity'. Bearing this in mind, it seems that Itkonen's view comes closest to position IV (sociologism), but only if he defines 'a language<sub>1</sub>' this way as opposed to language in the general sense. Chomsky's position probably corresponds to V (what Pateman calls dualism; not to be confused with dualism in the philosophy of mind), with 'a language' defined sociopolitically but not linguistically, my 'a language<sub>2</sub>'. I will ignore II (Platonism) for the moment, but I should note that it can be adopted as an ontological position for language in general without its being adopted as an interpretation of the ontological interpretation of 'a language'(either 1 or 2); cf chapter 5 on this.

Pateman wants to combine view I (Naturalism) with a view of a language as a social fact (which one of IV and V, I discuss below). Considering the first of these, Pateman has this to say:

'The knock-down argument against languages (emphasis in original: PC) as natural kinds is surely and simply this: that tiger cubs brought up among humans become tigers, not humans, whereas Vietnamese orphans brought up in England talk English, not Vietnamese.'

'But this is not the end of the matter. For from the fact that a language is not a natural kind, it does not follow that languages are not a natural kind, that is to say, from the fact that English is not distinguishable from Vietnamese by essential, natural, replicable properties it does not follow that languages are not distinguishable from other human or animal semiotic systems by essential, natural, replicable properties.'

I think that what Pateman has done here is to shift the expression to be explicated from 'a language' to 'languages', where 'languages' corresponds to 'language' in the general sense described above. That is, unless Pateman is using the expression 'a language' (presumably 'a language<sub>1</sub>') in a generic sense, he has not defended the stated definition that 'a language is a natural kind'. Rather, he has used the expression 'languages are a natural kind' where he simply means 'language is a natural kind'. If this is so, then what we are left with is the familiar Chomskyan conception of language, or the human language faculty, as a natural kind. And, of course, Chomsky adopts this view without committing himself to the claim that a language is a natural kind. I assume here that by 'natural kind', Pateman is referring to a *physical* type which is a product of natural evolutionary forces, in much the same way that tigers, or 'a tiger', taken generically, are natural types/kinds.

If we interpret 'a language' generically, then Pateman's statements are not subject to the criticisms I have given, but it is not clear precisely what sense he is using the expression in, given that he precedes this discussion with comments about English as 'a language'. We need to add that for Chomsky (1980:217-218), both positions III and V are true. That is, 'a language<sub>2</sub>' is only a name, so far as linguistic investigation is concerned, but 'a language<sub>2</sub>' is a social object when considered independently of linguistic inquiry, and that object is not a linguistic one. As far as I can see, Pateman gives us no reason

to reject the Chomskyan position ( an adoption of nominalism and dualism combined with a rejection of naturalism, Platonism and sociologism).

Interestingly, and rather oddly I think, Pateman considers that Chomsky's adopting position V (dualism) precludes his adopting III (nominalism), thus:

'....by saying that languages are socio-political facts, Chomsky allows us to avoid resort to the kind of nominalist position already discussed, while leaving it open what sort of socio-political fact a language is.'

(110)

As I have just argued, this is not the case. It is true, however, that Chomsky does leave open the question of what sort of socio-political object 'a language<sub>2</sub>' is, and if Pateman could argue that it is interpretable under a version of naturalism, then he could arrive at a definition of a language as a social object and a natural kind. However, this would be uninteresting from a linguistic point of view, since we are concerned with 'languages<sub>1</sub>'. If the expression 'language' is only of linguistic interest in relation to 'a language<sub>1</sub>' and 'languages<sub>1</sub>', then it remains for Pateman to make a case for such objects as natural kinds and objects of social theory. I believe that his set of distinctions rests upon a confusion of languages<sub>2</sub>, which undoubtedly are social in some sense, and are not an object of linguistic inquiry, with languages<sub>1</sub>, which are objects of linguistic inquiry but are not social in the way that languages<sub>2</sub> are (they may be taken to be intersubjective, and thus social, objects, but this sense of 'social' is distinct from the one just mentioned).



I argued in chapter 3 that linguistic objects cannot be taken to constitute natural objects if one means by that physically-defined objects of an evolutionary sort (cf the discussion of type physicalism, which is what Pateman is adopting, I think). It should be rather obvious, therefore, that I am not inclined to accept Pateman's ontological claim that 'a language' (I prefer the expression 'linguistic objects' or 'the object of grammatical inquiry', since I am talking about 'a language<sub>1</sub>') is 'a natural kind'.

I have not said what Pateman wants to make of positions IV (sociologism) and V (dualism). His views can be summarised as follows: there are linguistic objects which are not social objects; thus any extreme sociologism (such as Itkonen's) is ruled out. Those linguistic objects which are not social objects are psychological in nature; at least some of these psychological objects are natural objects. There are facts about language which are social facts but not linguistic facts, and these must be viewed, under Pateman's type of social theory, as being constrained by 'natural processes'.

Some impression of the differences between my assumptions and Pateman's can be gained if I repeat that I recognise the following four-way distinction: physical objects as opposed to psychological states of affairs, as opposed to social objects, as opposed to objects which are a product of social and psychological processes, such as linguistic objects as investigated in theoretical (ie 'autonomous') linguistics.

Because I take it that linguistic objects are thus ontologically differentiated from social and psychological objects, and because I adopt a realist interpretation of linguistic theories, I am happy to adopt a 'radical autonomy thesis' of the sort mentioned by Lass and Itkonen, but without adopting either Lass' instrumentalism or Itkonen's ontology. Pateman, on the other hand, opposes autonomous linguistics (cf his introduction, in which he takes it to be 'abstractive linguistics', a position I criticise in chapters 3 and 5), and this follows from his ontological assumptions, which are largely physicalistic.

Since Itkonen's and Lass' views, as well as mine, entail a defence of autonomism, I want briefly to discuss Pateman's criticisms of Itkonen's autonomism, and to show that they need not worry proponents of autonomism.

(ii) Naturalism and the autonomy thesis

In discussing the fourth answer to the question 'what is a language?' (sociologism: the view that language is a social object and that this is also a linguistic object), Pateman cites Itkonen as a proponent of this view. This is the case only if Itkonen defines 'a language' this way, and it appears that he does not. Rather, he defines linguistic rules this way.

Pateman notes this:

'Itkonen is concerned with the ontology of individual rules, and not with the ontology of languages. As for the latter, he sometimes seems to treat these nominalistically and at other times supposes rules to form systems, to which names of languages refer.'

If Itkonen is not in fact proposing answer IV (sociologism), should we conclude that Pateman has simply failed to discuss what he purports to? In this case, I think we can still examine some important aspects of sociologism if we bear in mind that we are dealing with (some) linguistic objects, rather than 'a language'. That is, it is certainly true that Itkonen is proposing the following: that linguistic objects (not ably rules of language) are social objects. And this view is fairly labelled sociologism, as long as we do not interpret this to mean that Itkonen takes linguistic investigation to be a form of empirical sociology. Given that this is reasonable, it is interesting to note Pateman's criticism of Itkonen's sociologism.

The principal line of argument that Pateman follows is this: Itkonen's linguistic-objects-as-social-objects approach rests upon his adoption of and interpretation of the Wittgensteinian argument against private languages. If either the application of this to the methodology of theoretical linguistics or Itkonen's interpretation can be countered, then his sociologism can be countered.

Pateman attempts mostly to counter Itkonen's application of the private language argument to theoretical linguistics, on the grounds that the conclusions Itkonen draws from Wittgenstein concerning grammatical inquiry are insupportable. Pateman's arguments relate to the various issues raised by an innateness view of creolisation, second language learning and the signing systems developed by deaf children of hearing parents. What

Pateman wants to do is to show that there is strong evidence for an innateness account of these, and that this is in conflict with Itkonen's anti-private language thesis.

It is vital that we bear in mind that Itkonen has never shown any aversion to the notion of an innately-specified language acquisition device, and has frequently stated that the innateness/anti-innateness debate is simply a debate about *how richly developed* innate specifications are. Itkonen's objection to Chomskyan linguistics is that 'it maintains a conception of language which is demonstrably equivalent to the private-language conception' (1978:113). Now, while it may be the case that Chomsky arrives at this private language conception on the basis of his assumptions about innateness, it is not the innateness hypothesis itself which Itkonen is objecting to. Rather, it is the competence/performance distinction and the fact that Chomsky distinguishes knowledge from use. A defence of the innateness hypothesis need not, therefore, count against Itkonen's anti-private language stance.

To take one of Pateman's criticisms: he argues that Bickerton's (1981) account of creolisation constitutes evidence against the claim that linguistic rules cannot be private. He bases this on the fact that 'it appears to be empirically false that there is a pre-existing language to be acquired by (creolising) children' (1983:104), the point being that these children are following linguistic rules which are not constituted as shared knowledge in a pre-existing speech

community. From this he concludes that such children are following private language rules, by which he means rules which are privately invented on the basis of some innately specified 'bioprogram' (to use Bickerton's term).

However, the fact that there is no pre-existing creole-speaking community in these cases does not impugn Itkonen's argument at all. It will still remain the case that any given speech action within the set of first generation creole-speaking children will be subject to Itkonen's 'correctness' criterion, which lies at the heart of his argument against private language rules. That is, it need not be the previous generation who define 'correctness' for the creolising generation; it may well be the creolising generation themselves. It remains the case that the linguistic rules developed by the new generation are of an intersubjective sort, regardless of whether they are 'invented' on the basis of some sort of innately-specified 'ur-language' or not. The point for the private language argument is that the internal processes whereby a speech action comes to be performed are distinct from the intersubjective 'check' on well-formedness which Itkonen takes to characterise the following of a linguistic rule.

It is for this reason that I have not discussed Fodor's (1975) 'arguments in favour of a private language': what Fodor refers to as a 'private language' is simply some sort of internal 'mode of computation' which each human being presumably has in common. But this is not what Wittgenstein was talking about; and in fact it would make no sense at all, were we to

discover that such an internal mode of computing existed, to refer to it as either 'correct' or 'incorrect'. Nor would it make sense, and Itkonen himself makes this point (1978:320), to refer to any particular computation on the basis of such a 'language' as being 'correct' or 'incorrect'. Both Pateman and Fodor fail to see that internally specified means of encoding and decoding are simply not germane to what Wittgenstein, and therefore Itkonen, are claiming.

Pateman claims that there are two problems for Itkonen with regard to this supposed conflict between the innateness hypothesis and the anti-private language position. First, he claims that

'if there are innate and other cognitive structures which can legitimately be talked about and theorised, then there is the problem of determining the concepts which can legitimately be used in this enterprise, currently dominated by the computational paradigm in which concepts of 'rules' and its cognates are central'

(109)

I take this first problem to be, at best, a matter of division of labour, and certainly not a problem for Itkonen, who has made it clear what he takes 'rule' to mean, and has shown what he finds objectionable in the Chomskyan paradigm. That the existence of innate structures need not undermine the computational paradigm is clear from the fact that Fodor combines these: he proceeds on the hypothesis that the object of inquiry is both innately specified *and* that thought is computational.

The second purported problem for Itkonen concerns the

nature of what he has achieved. Pateman argues that Itkonen (1978) is best seen as 'a contribution to the study of the social side of language' (109). That is, he claims that Itkonen has most probably 'theorised the domain of a discipline parallel to psycholinguistics - namely sociolinguistics' (loc cit). This is quite mistaken. Firstly, the expression 'the social side of language' is hopelessly broad and vague. It simply begs the question 'what do you mean by the term language?'. I choose to avoid the term, and refer to 'linguistic objects' instead (though if one uses it, it is in relation to 'a language<sub>1</sub>' and 'languages<sub>1</sub>'). Itkonen carefully distinguishes atheoretical rule and rule-sentence, corpus-based investigation in psycho- and socio-linguistics as opposed to non-corpus-based grammars, and intersubjective rules of language vs private inner states. Secondly, it is not possible to interpret Itkonen as having theorised sociolinguistics; I will not even argue that this is so, since a reading of my summary of Itkonen's proposals will make this clear (or better still, a reading of the opening chapters of Itkonen 1978).

I think I have made it clear that none of the arguments in favour of innately-specified cognitive structures is even relevant to the private language argument. Thus, this line of attack upon Itkonen, and upon autonomous linguistics, cannot proceed. Nor, I think, can Pateman's attempt to introduce a version of naturalism into the philosophy of linguistics via arguments in favour of innateness. Neither my, nor Itkonen's

versions of autonomous linguistics rests on an anti-innateness argument. I proceed in the following chapter to flesh out more of the details of my own autonomy thesis, and I also consider the Platonist version of autonomism which, interestingly, *is* tied to the innateness hypothesis in a rather odd way.



## CHAPTER 5

## BEYOND SOCIAL AND PSYCHOLOGICAL INTERPRETATIONS

In this chapter, I deal with philosophies of theoretical linguistics which take the object of inquiry to be neither psychological nor social in nature. I examine a version of Platonism, as expounded in Katz (1981), and discuss some of the objections to Katz' proposals made by Pulman (forthcoming), Pateman (1983) and Itkonen (1983). I also state my own objections to Platonism and present my version of an autonomous linguistics based on interactionism, which I think preserves the merits of Platonist autonomism but avoids its more perverse consequences.

## 5.1 Platonism

## (i) Abstract objects, causality, and emergence

Katz' (1981) work Language and Other Abstract Objects constitutes a major departure from his earlier published views on the ontological status of linguistic objects. His paper 'Mentalism in linguistics' (1964), for example, argues for the sort of psychologism discussed in chapter 3. It is quite clear that Katz arrived at a Platonist philosophy of linguistics because of his interpretation of semantic representations in particular: his (1977) 'The Real Status of Semantic Representations' constitutes his first public statement of an overtly Platonist line on linguistic representation. And in fact, it is arguable that in this paper, Katz merely makes explicit what had been an immanent Platonism in his approach to semantics since his Semantic Theory (1972)\*.

However this may be, we can now take Katz, along with Postal, Langendoen and other members of the 'New York School of Platonism' (cf Postal & Langendoen 1984) to be the principal advocate of an explicitly non-psychologistic, non-social, ontology of linguistic objects, the details of which are as follows.

\* I am grateful to Noel Burton-Roberts for pointing this out to me; notice that it lends credence to the Popperian notion that our hypotheses may contain certain properties, such as contradictions, even if we as authors fail to perceive them. In this case, Katz' mentalism was contradicted by the Platonistic, non-mentalistic, nature of his view of semantic representation.

Katz adopts the tripartite distinction traditionally made in the philosophy of mathematics (and of logic), the nominalist/conceptualist/ realist distinction. He argues that there have been two principal philosophies of linguistics in the twentieth century, namely nominalism and conceptualism. The first of these is reflected in the works of many of the American Structuralist linguists whose work I described as instrumentalist in 2.2. He refers to it as being nominalist since, under this philosophy of linguistics, theoretical terms are no more than names and do not refer to extratheoretical realities. What Katz calls conceptualism is the view that theoretical terms refer to mental entities. He takes this to characterise the Chomskyan position which I have called psychologism. Both of these are taken to be distinct from realism which, in Katz' view, means Platonic realism. In this respect, 'realism' here is not synonymous with what is referred to as 'realism' in the philosophy of science. Thus, under the latter use of the term, both Chomsky's and Katz' philosophies of linguistics are realist (albeit of very different sorts) whereas in the former, only Katz' proposals are realist, and not Chomsky's.

I do not want to pursue at length the matter of whether the trichotomy in the philosophy of mathematics and logic, or the realist/instrumentalist dichotomy in the philosophy of science is the most appropriate means of identifying positions in the philosophy of linguistics. However, it is noticeable that one's choice between the two appears to be at least partly determined

by one's methodological and ontological assumptions about theoretical linguistics. The fact that Katz wants to identify theoretical linguistics as a discipline parallel in method to mathematics and logic, rather than the natural sciences, is reflected in his use of a philosophy of mathematics orientation. On the other hand, my use of a philosophy of science orientation might be taken to reflect the view that theoretical linguistics is a discipline parallel to those in the natural sciences. The choice of orientation thus begins to sound rather circular, that is, as if one's philosophy of linguistics determines how one is to go about talking about one's philosophy of linguistics.

While this may be the case for Katz (I do not argue that it is), I think I can defend myself from the accusation of methodological circularity. As I have mentioned (2.2), my approach to the question 'Is the realist/instrumentalist debate relevant to the philosophy of linguistics?' is that we need to examine the issues raised in the debate and find out whether they help shed light on problems in the philosophy of linguistics. I think I have shown that they do. Thus, my adoption of a philosophy of science approach to the philosophy of linguistics does not necessarily presuppose a version of 'scientism' (what Itkonen calls positivism), otherwise known as methodological monism, the view that the method in the natural sciences parallels that in the human sciences. In order to establish whether theoretical linguistics adopts the same methods as the natural sciences or not, one must go into the

methodological and ontological details, which is what I have done. I think I have established fairly clearly in what respects I take natural sciences to be parallel to theoretical linguistics, and in what respects they differ.

One further point needs to be made in this connection: it is mistaken, I think, to assume that these three positions (nominalism, conceptualism and realism) encompass the entire range of twentieth century philosophies of linguistics. I have already discussed such a philosophy which does not correspond to nominalism conceptualism or realism, namely Itkonen's mutual knowledge anti-positivism. Saussure and Hjelmslev can also be interpreted as embracing none of these three. Indeed, one could be forgiven for assuming, on a reading of Katz' book, that the entire philosophy of linguistics in the twentieth century took place within the United States; his account takes Sapir, Bloomfield, Harris and Chomsky to be the principal figures in this history, and excludes reference to Saussure. Hjelmslev is mentioned once, interestingly, as a possible forerunner of the sort of approach Katz takes. This is regrettable, as it leads Katz to assume, on several occasions, that Platonism is the only current alternative to nominalism and conceptualism, which it certainly is not.

Bearing this in mind, Katz' position can be identified by observing that he takes linguistic objects to be 'abstract' in nature, where 'abstract' means non-spatial, non-temporal, Platonic. Specifically, both sentences and languages (ie particular languages such as English) are abstract objects of a

Platonic sort. A reminder here of a point I made in 4.1: 'abstract' for Katz is not equivalent to 'abstract' for Chomsky. Chomsky's object is spatiotemporal, and his representations of it are abstract in that they attempt to describe aspects of speakers' knowledge abstracted away from individual speakers and non-linguistic factors. Despite being thus abstract, Chomsky's representations are taken to be potentially descriptive of (or 'characterising') spatiotemporal entities and processes (though note the problems with identifying functions spatially: 1.1). Katz' abstract objects, on the other hand, are not spatiotemporal at all. One could equally use the terms 'ideal' in relation to Katz' objects and 'idealised' in relation to Chomsky's, as I noted in 4.1.

Before I go on to discuss Katz' abstract objects, it should be stressed that Katz' ontology (of Platonic objects) is separable from his epistemology. That is, having established a case for Platonically real objects, Katz must erect an epistemology of how it is we come to have knowledge of such objects. This he does in the final chapter of the work, but it is conceivable that that Katz might abandon this epistemological framework and erect another which might fit his ontological assumptions better (of course, one may find it rather hard to conceive of what epistemology could possibly fit Platonism, but that is to anticipate). I mention this for two reasons: one, (i) I intend discussing ontology first, and epistemology second and (ii) the distinction between the two is

important when it comes to examining the details of Katz' proposals.

Katz' arguments for taking the object of inquiry to be abstract objects are as follows. Knowledge of something is distinct from that which we have knowledge of. That is, the 'knowledge of' relation is a two-place predicate. If we accept that this is so, it is clear that our knowledge of the structure of a sentence is distinct from the structure of the sentence per se. It follows from this that sentences themselves, as the objects of our inquiry, are not competence units, are not units of knowledge, but are objects of which we have knowledge. In engaging in acts of intuition, we gain direct access to these objects; thus grammaticality judgements involve the accessing of linguistic objects per se, rather than some internal cognitive structure. Katz claims that, in this respect, linguistics is like mathematics or logic, which equally involve intuitive access to extra-psychological entities and relationships. His claim is that mathematical theory is a theory of numbers per se, rather than the human cognitive capacity for storing and processing representations of numbers. Likewise, logic involves the study of logical relations via intuitive judgements as to what are and are not valid inferences.

This interpretation of logic and mathematics is of course defensible, but is not universally accepted; to support it, Katz cites Frege and Husserl as proponents of such an interpretation of logic, and Hardy (1940) for this interpretation of mathematics. However, even if one were to argue that logic and

mathematics are incorrectly construed as 'sciences of the intuition' in Katz' Platonistic sense, Katz could still argue for such an interpretation of theoretical linguistics. That is, Katz' Platonistic interpretation of grammatical inquiry is not dependent on the supposed parallel between it and mathematics and logic.

It is clear that intuition is thus distinct from psychological acts such as remembering, perceiving and introspecting. While the latter might seem similar to the act of intuiting, in that both appear to be 'mental' acts in some sense, it is important to bear in mind that for Katz, the object which one gains access to in an act of introspection is quite distinct from that which one accesses during intuition: one is an internal cognitive state of affairs, while the other is a non-subjective, and for Katz, Platonic, object.

These considerations lead Katz to argue that theoretical linguistics is an autonomous science with respect to psychology, which he takes to include psycholinguistics. Being a science of the intuition, theoretical linguistics is not an empirical science at all, whereas psycholinguistics is. It is interesting that Katz does not give the same detailed consideration to the question of what is meant by 'empirical' as is given by Itkonen, but he does define empirical sciences as being about 'experience or the external world itself' (Katz 1981: 23). While it is at least a reasonable approximation to say that the term means 'about experience', this is rather vague, in that acts of



intuition are experiences of a sort. The notion 'external world' is of little help either, as mathematical and logical realities could well be said to constitute elements of the external world, even when interpreted Platonistically. However, since Katz takes Platonic realities to be non-spatiotemporal, it is as well to assume that in Katz' view empirical sciences relate to the spatiotemporal, or, in Itkonen's more precise formulation, are spatiotemporally falsifiable. Katz is right in arguing, as I have, that it is mistaken to equate 'empirical' with 'falsifiable', and that nonempirical (in this sense) theories may nonetheless be falsifiable.

In addition to these proposals, Katz argues that psychologism is both too restrictive and not sufficiently restrictive a framework for linguistic theorising. His arguments are as follows. Psychologism is too restrictive in that it takes linguistic objects to be psychological objects and thus constrains the class of possible grammars to those that happen to be compatible with the contingent facts concerning human cognitive make-up. That is, anything which happens to be a factor in the human cognitive machinery can potentially count as a factor in the grammatical description of a sentence (or, more generally, in the grammar of a language). Katz argues that this may often turn out to be undesirable. He cites the case whereby a set of grammars contains all of the simplest grammars which 'predict and explain every grammatical fact about each sentence of English (86), but which are psychologically implausible, as opposed to a more complex grammar (ie

notationally more complex or more complex in its set of rules) which is plausible psychologically. He points out that the conceptualist would have to choose the grammar which falls outside of our original set of economic and explanatorily successful grammars, simply because it fulfills our psychological criterion for the evaluation of grammars. Thus, it is possible, Katz claims, that our strictly methodological criteria (simplicity, generality, descriptive and explanatory adequacy) may clash with our general metatheoretical demand that grammars be psychologically plausible.

By the same token, Katz claims (90-91) that such a requirement would be insufficiently restrictive in that it would not exclude from the class of permissible grammars those which are psychologically plausible but linguistically impoverished (ie which contain inelegant sets of rules which fail to capture significant generalisations).

Consider some of the objections one might have to these claims. The conceptualist may doubt that it is likely that we would end up with such a clash, that is, it is anticipated that simplicity, generality, etc are themselves likely to be the basis upon which processing, storage, etc take place. Under this view, we take the methodological criteria to be not merely a set of principles for the construction of grammars, but a reflection of the principles underlying human cognitive makeup. Thus, both our grammar, as formulated by the linguist, and the object grammar function according to the same general principles, which

of course is what psychologism is all about. The trouble with this objection to Katz is that it appears to fit badly with actual practice in theoretical linguistics. Considerations as to psychological plausibility rarely seem to figure in grammatical descriptions, and what linguists do seem to care about is the capturing of linguistic generalisations per se; it is thus perfectly easy to violate psychological plausibility while devising an elegant analysis. A case in point must be the emergence of transformational analyses during the heyday of the standard model, where such analyses were not guided by the desire to attain psychological plausibility, nor did they in fact satisfy any such desire. And yet such analyses were methodologically very appealing, and notions such as Raising and Equi still feature as descriptively useful constructs in much grammatical analysis (one still sees these constructs being used within more recent, non-transformational, grammar: cf Gazdar *et al* 1985 for example).

Consider another objection that might be raised by the conceptualist. It may be argued that it is vital to distinguish, in any theory of processing, between two or more logically equivalent grammars on the one hand and the set of possible modes of implementation of such grammars on the other. We might call these algorithms for grammars, by analogy with the computational distinction between a function to be computed and an algorithm or means of implementation for the computation of that function (say, in a particular programme written in a particular programming language). Conceptualism does not

require us, it might be argued, to reject a particular grammar just because some algorithm or other for that grammar\* is psychologically implausible. The grammar is not equivalent to any particular algorithm and conceptualism could be said to require us only to adopt an algorithm which is plausible. Precisely this sort of objection to Katz' argument is raised by Pulman (forthcoming), which I discuss in (ii) below.

The most interesting facet of such an objection is that it incorporates a tripartite distinction between the following: a level of hardware, to be taken into account in any theory of processing, the algorithmic level, where a given grammar is made to work by means of a series of implementation instructions, and the level of the grammar itself, which is the level at which two or more grammars can be said to be logically equivalent (or not). The question that is begged by this set of distinctions is: what sort of thing is this highest level object? If we take the human cognitive apparatus to contain a finite stock of hardware and a particular algorithm, or set of implementation instructions, then where does the internalised grammar fit in

\* The expression 'algorithm for a grammar' may seem odd here. If so, perhaps I can explain what I mean by using another, very similar, computational analogy, used by Fodor (1983). One can distinguish between a function to be computed by some system, the *virtual* architecture of the system, which specifies the set of instructions that need to be incorporated into a programme to get the function to be computed, and the *physical* architecture of the programme which embodies these instructions. The level of virtual architecture here corresponds to the algorithmic level mentioned above. It seems to me that one can view linguistic systems and their realisations in precisely this way (and indeed Pulman, forthcoming, does).

with these? And if we allow that machines and /or alien species may possess differing algorithms for the same grammar in the cases where such beings/machines can decode a natural language, what is the status of the grammar for which they have distinct algorithms?

It is very tempting to draw a parallel between this sort of tripartite distinction and that drawn by Popper, which is what I propose in 5.2. However, without pre-empting the discussion there, it is interesting to note that in anticipating this sort of objection by the conceptualist, Katz concludes (rightly, in my opinion) that if the conceptualist adopts such a set of distinctions, he collapses conceptualism into autonomism. This is because it amounts to autonomism to allow a third ontological category distinct from the hardware and the algorithmic levels. Recall Fodor's token physicalism and its faults (4.2): not only can we not expect grammatical kinds to turn out to correlate directly with algorithmic kinds, where the algorithm is some specific physical means of representing the grammar (type physicalism), but neither can we expect (physical) algorithmic tokens to correlate directly with grammatical tokens (token physicalism). This is because it is only by virtue of being a representation of a grammar that an algorithm has any meaning. We are compelled, therefore, to distinguish the grammar from both the algorithm and the hardware, and to allow that the grammar belongs to an ontic category distinct from either of these.

For Katz, this category is Platonic, thus:

'There are also incompatibilities between aspects of the mental or neural structures in these various groups of creatures. We cannot abstract away from them without abstracting away from the psychological medium in which competences are realised and paying attention only to invariances across the range of cognitive systems that reflect the grammatical properties and relations of English sentences. Such abstraction would collapse conceptualism into Platonism.'

(91)

While I agree with Katz that an attempt on the part of the conceptualist to distinguish grammars from their algorithms would collapse conceptualism into autonomism, I think Katz is mistaken in assuming that it is Platonism in particular, as a brand of autonomism, which would result from such attempt. There is no reason why, having allowed that grammars are distinct from their modes of implementation, we need assume that they are therefore Platonic in nature. This is one of the points at which my autonomism differs radically from Katz'. I will try to show (5.2) that this kind of autonomism is much more comprehensible than Platonism, and overcomes some of its major problems.

One of the principal problems with the notion that linguistic objects are Platonic concerns the possibility of our coming to have knowledge of such objects, that is the question of supplying an epistemology to fit the ontological assumptions. Katz attempts to do just this in the final chapter of his book, where he assumes that intuition is fallible, that is, that our intuitive judgements may turn out to be mistaken. Katz does not consider that this weakens the role of intuition in Platonist linguistics, since he assumes that fallibility and certainty are

properties of the knower rather than the known. Katz argues that it has been a mistake in the history of Platonism to draw an analogy between intuition on the one hand and both introspection and perception on the other, and to assume that intuition involves some kind of direct contact between the knower and the abstract object. This, he argues (201), amounts to claiming that knowledge of abstract objects is knowledge by acquaintance. He believes that this is mistaken, because it is not possible for abstract objects, being atemporal and aspatial, to exert causal influences upon a knower:

'Being objective, abstract objects do not occur as a constituent of the conscious experience of a knower, and, being aspatial and atemporal, they cannot act on a knower through a causal process to produce a representation of themselves in the manner of sense perception.'

The immediate question this begs is that of how we can possibly come to have knowledge of these objects if they cannot act upon us to induce some representation, for it is quite clear that we do have such representations.

Katz answers this by proposing a Kantian epistemology whereby intuitive awareness is the effect of an 'internal construction'. That is, we internally construct intuitive judgements, and these either do or do not correspond to external abstract objects (thus fallible intuitionism). These internal constructions are therefore representations of abstract objects rather than abstract objects per se. The reader may by now be amazed that our internal constructions happen so often to correspond to external abstract objects. Katz responds to this

by claiming that this apparently chance correspondence is not chance at all: we are endowed with an innately specified notion of 'abstract object' which 'specifies the ontological characteristics of the object that grammatical knowledge is knowledge of' (205).

Katz thus transforms Chomskyan nativism into a native knowledge of 'abstract object'. None of this gives us any idea of how we are supposed to have come to possess such an innate knowledge, especially when one considers that abstract objects are not available for causal interaction during the evolutionary process. And this is the principal problem with Platonism: if we cannot in principle interact causally with Platonic objects, how can we know they exist? And how can we come to have knowledge of them? This is in marked contrast with my version of autonomism, which stresses interaction, and furthermore, following Popper (1972), uses the fact of interaction as evidence that it is coherent to talk of objective knowledge. Interactionism also assumes that the emergence of such a kind of knowledge is an evolutionary phenomenon.

Another interesting problem with Katz' philosophy of linguistics, which has been pointed out by Pateman (1983), concerns the conflict between his methodological criteria for the assessment of linguistic theories and his epistemological and ontological proposals. Katz adopts a simplicity criterion for the assessment of linguistic theories (66-67; 234-237) and specifically urges us to adopt Occam's razor as a general principle of scientific methodology (237). Now, given that



Katz adopts both a nativism akin to Chomsky's (but with knowledge of 'abstract object' as the innately endowed knowledge) in addition to an ontology of abstract objects, Pateman argues that Katz ought to apply his own simplicity principle and dispense with abstract objects, leaving us with straightforward Chomskyan innateness. Certainly, Occam's razor was devised precisely for this purpose (sometimes referred to as the trimming of 'Plato's beard': cf Quine 1953: 2).

Katz might, I suppose, argue that his simplicity criterion is a requirement for the assessment of linguistic theories and not metatheories, and that we cannot overcome the problems of psychologism without recognising the existence of Platonic abstract objects. While I am inclined to agree that we need to avoid the possibility of there being 'more in the world than there is in our ontology' just as much as we need to avoid the possibility of there being more in our ontology than there is in the world (Occam's view, and Quine's for that matter), I think that Katz must concede that his Platonic abstract objects are rather difficult to take seriously precisely because they are said not to be capable of entering into causal relations with human minds.

There is a further point that I want to make about Katz' abstract objects, concerning the question of what counts as a linguistic, and therefore Platonic, object. As I have said, Katz argues that both sentences and particular languages count as objects of linguistic theory and are therefore subject to

this Platonic interpretation. This is relatively controversial; while it is perfectly reasonable to assume that sentences are linguistic objects and thus susceptible to such a Platonic interpretation, it is rather novel to argue that particular languages (and, in principle, any and all future and past languages) should be taken to be objects of linguistic theory. The received view on this subject (Chomsky's) is that expressions like 'English' are best interpreted sociopolitically, such that 'English' is not a linguistic object, and it is a (linguistically) arbitrary matter whether Hindi and Urdu, for example, are or are not 'the same language' (I have shown, in chapter 4, why I accept this).

Katz, however, argues that such expressions do denote linguistic objects, which of course he interprets Platonistically:

'...claiming that notions like 'English', 'French', etc and 'natural language' are not proper concepts of linguistics... is like claiming that the concept of number is not a concept of mathematics, but a sociopolitical one (or that the concept of implication is not a logical concept but a sociopolitical one)'

(79)

'The claim that linguistic theories are not about psychological phenomena but straightforwardly about sentences and languages rests on the general epistemological distinction between knowledge that we have of something and the thing(s) that we have knowledge of.'

(77)

I think that the first of these passages reflects a confusion. Quite apart from the fact that the concept 'natural language' is quite clearly a concept of linguistic theory, and

quite different in status from concepts such as 'French', it is very clear that 'French' does not bear the same relation to linguistics as 'number' does to mathematics, or 'implication' to logic. The act of intuition, which Katz makes a great deal of, does not, for example, involve judgements about a language. It does involve judgements about sentences, however, and if we then take such judgements to constitute the data and sentences to constitute the central objects of our inquiry, we can go on to characterise their properties and structure, yielding a whole set of objects within this domain such as 'constituent', 'syllable', 'complement', etc. While it is true to say that notions like 'sentence' are as much a central concept of linguistics as 'number' and 'implication' in mathematics and logic, it is not at all the case that any linguistic theory need make reference to notions such as 'French'.

I believe that Katz is also mistaken, in the second of these passages, in utilising the 'knowledge of' relation to justify taking 'French' etc as linguistic objects. He claims that what we have knowledge of are, not just sentences, but particular languages. One can take the knowledge of relation to have sentences and their properties as its object without having to make this claim. In having knowledge of a given set of sentences and their properties, or the grammar which underlies these, it is an arbitrary matter whether we refer to that grammar as 'French', 'Spanish' or whatever. Of course, part of the problem in discussing the notion 'a language' arises from the dual definition given for the term within the Chomskyan

framework (cf 4.2), where 'a language' is both 'a set of sentences' and 'a non-linguistic, sociopolitically defined entity'. I argued there that while this can cause confusion, there is not in fact any contradiction involved. One of these defines the object of inquiry, the other simply makes it clear that terms such as 'Japanese' have no explicit linguistic function. It matters not at all that once we have come up with a characterisation of what a speaker knows ('a language' in the formal sense: my 'a language<sub>1</sub>'), we may not be able to decide whether this should be sociopolitically labelled as 'Dutch' or 'German'. Katz is therefore mistaken in making the following remark:

'Thus, we have to know from the outset what 'English' refers to in the characterisation "the ideal speaker-hearer's knowledge of English". The characterisation employs the term "English" to specify the knowledge in question, just as the characterisation "the ideal reasoner's knowledge of propositional logic" employs the term "propositional logic" to specify the knowledge in question.'

(80)

Even if Katz had not made the mistake of taking expressions like 'French' to denote a linguistic (and therefore abstract) object, and had restricted his abstract objects to include only sentences and their properties, his Platonism would still run into the sorts of problem I have mentioned. He has, however, multiplied entities beyond necessity even more so than he might have done. The emergence of a Platonist philosophy of linguistics has, however, provoked a response, which can only help stimulate debate on matters methodological. Some of these

responses have been spurious, others important; I briefly examine some of these now.

(ii) Objections to Platonism

Pulman (forthcoming) gives several arguments against Platonism and in favour of a standard Chomskyan psychological ontology, for which he uses the term 'rationalism'. As above, I do not intend to discuss the empiricist/rationalist debate directly. For my purposes, rationalism means psychologism plus an innateness hypothesis; it is the former that I am interested in. I want to suggest that Pulman's criticisms, where they are arguments directed against autonomism in general, are mistaken.

The first of Pulman's arguments is that Katz is mistaken in claiming that the conceptualist (in Katz' terminology) or rationalist (in Pulman's) must make implicit appeal to the notion 'a language' as a linguistic object. I discussed this claim of Katz' in (i) and hope to have shown that it is based on a confusion. Pulman, however, reacts by trying to show that the Rationalist may in fact define the notion 'a language' (such as English or French) in a way that does not presuppose any reference to abstract objects. That is, he wants to show that talk of 'a language' is merely a convenient shorthand for talk about native speakers of natural language. His argument is as follows: talk of a language can be reconstructed as talk about speakers by assuming that the linguist collects samples of speech data, with utterance tokens as the raw data, and utterance types as abstractions away from those. The linguist

thus assembles a corpus, and his object of inquiry is an idealisation over this corpus (Pulman forthcoming: 10).

It is interesting that Pulman attempts to answer Katz: I think that he need not, since the rationalist is not guilty of the circularity which Katz accuses him of, as I hope I have shown in (i). As a reply to Katz, Pulman's account of linguistic methodology is not, I think, plausible, or attractive, for the rationalist. Itkonen's arguments against the myth that theoretical linguistics is corpus-based need not be rehearsed here; suffice to say that, as discussed in 4.1, theoretical linguistics neither is, nor needs to be, a corpus-based activity. Since it would be redundant for the theoretical linguist to collect utterance tokens corresponding to the sentences he analyses, there is no reason for Pulman to observe that 'in practice, linguists are seldom so virtuous' as to collect the corpus examples. If the sentences we analyse are indeed abstracted away from utterances, it remains a mystery how we can arrive at a set of sentences without having a corpus to abstract away from. Virtue does not enter into the picture; rather, we arrive at a set of sentences on the basis of some sort of knowledge which we possess (for Itkonen, mutual knowledge of intersubjective norms; for Chomsky, knowledge constituting the internalised grammar), and which we are trying to characterise. Oddly, this is precisely the standard Chomskyan rationalist view on the matter; Pulman, in attempting to defend rationalism from a criticism which is mistaken, abandons one of

the central components of rationalism.

Pulman's next criticism concerns the status of grammars in relation to Katz' claim that psychologism is over-restrictive. Pulman claims that a grammar can be interpreted either as a 'logical characterisation of the ability it describes' (15) or as a 'description of an actual algorithm, or program, for carrying out the computation of that function, or modelling the exercise of the ability in question' (15). He goes on to argue that Katz ignores these two different interpretations of grammars, taking only the algorithmic interpretation to be valid. He argues that if we adopt the logical interpretation, we can see that a psychologically implausible algorithm might still reflect an adequate logical-level grammatical characterisation. I discussed this argument in 5.1 (i) and pointed out, as indeed Katz has, that it presupposes a trichotomy of the sort any autonomist would want to propose. Thus, in constructing an argument against autonomism, Pulman collapses non-autonomist rationalism into autonomism.

Finally, Pulman objects to Katz' claim that the rationalist must take any species which is different in cognitive makeup from humans to possess a different grammar from ours even when the output is the same. This follows, Katz argues, from the fact that the rationalist takes internal cognitive structure to *constitute* the grammar. Pulman replies that Katz is ascribing to the rationalist an extreme version of reductionism, and that this need not be adopted by the rationalist. He cites Fodor (1981) in trying to show that such a reductionism is in no way

necessary for the formulation of an adequate psychological interpretation of grammars. This he takes to boil down to the fact that the same program can run on radically different sorts of 'architecture and hardware characteristics'.

Thus, he proposes a set of distinctions which consists of (i) the hardware characteristics of the machine, (ii) the particular program for computing the function, (iii) the algorithm which constitutes the set of instructions to be embodied in the program, and (iv) the grammar or function to be computed. If (i) and (ii) correspond in humans to neural hardware and its architecture (ie physical architecture) and (iii) corresponds to some set of psychological processes (virtual architecture\*), one wonders what (iv) corresponds to. Unless one adopts a purely instrumentalist interpretation of (iv), which would be odd, given that it is primary with respect to (iii), one will have to assign it real status, distinct from the psychological object in (iii). This is what autonomism does, and what Pulman fails to do. I have already argued (3.3) that Fodor's token physicalism results in an untenable reductionism, and cannot reconcile the difference between (iii) and (iv) with the claim that objects in (iv) are describable in a physical vocabulary.

While I share Pulman's reservations about the likelihood that linguistic objects are Platonic, I think his arguments

\* On the distinction between virtual and physical architecture, cf the footnote to my discussion of algorithms for grammars: p. 182



against autonomism in general are weak. The first of his arguments is unnecessary and in any case mistaken, the second collapses psychologism into autonomism, and the third both collapses these two and fails to avoid the pitfalls of reductionism. Thus autonomism, and even Platonism in particular, is left largely intact in the face of Pulman's objections.

Pateman (1983a,b) does, however, develop a very impressive case against Katz' epistemology, which has serious consequences for the ontological framework Katz proposes. Firstly, he proposes the sort of argument discussed in (i), that Katz ought to wield his own methodological criterion (Occam's Razor) and dispense with abstract objects altogether, leaving only a standard Chomskyan innateness view of linguistic objects. I suggested there that Katz might reply that his methodological criterion is one for the assessment of theories rather than metatheories, and that Pateman ought to give philosophical objections at the metatheoretical level to attack Platonism. In his 1983 (a), he does just this by showing that all of the properties possessed by abstract objects can be seen to be possessed by the kinds of mutual knowledge proposed by Itkonen. Thus, linguistic objects are not spatiotemporal because they are not regularities, but rules of mutual knowledge. And since Itkonen's mutual knowledge ontology poses fewer problems in terms of both epistemological and ontological difficulties, it is advisable to adopt Itkonen's.

In addition to this, Pateman argues that in a Kantian

epistemology, entities such as the abstract objects Katz proposes are unknowable in themselves, our knowledge being confined to the world of non-abstract objects. The most interesting consequence of this is that it would lead us to abandon abstract objects as entities to which our theories may refer.

It is clear that Platonism faces severe epistemological problems, and that it does not fit easily with the notions of emergence of linguistic objects and causal interaction between these and human cognitive capacities. However, autonomism still emerges as a plausible way of looking at such objects, and I hope to show in 5.2 that it is possible to propose an autonomism which is based on the idea of emergence through interaction.

## 5.2 Interactionism

Having argued for a version of realism, and that neither psychological nor social interpretations of linguistic realities are satisfactory, I want to present my version of autonomism which I think overcomes the problems discussed in chapters 3 and 4, as well as the problems with regard to emergence and interaction which beset Platonism.

Perhaps the most counter-intuitive aspect of Platonism is its assumption that linguistic objects are not products of human activity; along with this rather unfortunate fact come the problems associated with emergence and interaction which I have mentioned. If we reverse these Platonistic claims, however, and adopt some of Popper's proposals concerning emergent realities, we claim that linguistic objects are emergent, man-made products which interact with other physical and cognitive systems. However, if we still maintain the one viable aspect of Platonism, namely its autonomistic element, we claim that such objects are largely autonomous, in much the same way that the modules in a modular account of a linguistic system may be said to be autonomous but interacting. This seems to me much more intuitively appealing than Platonism; since it is based on some of Popper's proposals, I first outline these and then show how they can be extended to linguistic objects.

### (i) Popper's proposals

As part of his attempt to understand the nature of the

growth of scientific knowledge, Popper has proposed that we ought not to take scientific theories to be psychological in nature. That is, while some subjective, psychological process occurs, internal to the scientist, as a part of the formulating of a scientific theory, that is no reason to assume that the theory, qua theory, is a psychological entity. Rather, it is the objective content of the theory which is important, and this, Popper argues, is not psychological in nature, but intersubjective. If one considers, for example, the following properties of a theory, one need make no reference to the psychology of the scientist: its falsifiability, its internal consistency, its relationship to other theories, the relations between its sub-parts, its relationship to the problem situation it is designed to resolve and the context in which it is proposed (the context would in fact include the problem situation and any competing theories designed to resolve that situation: the two are closely interconnected).

As an illustration of this, Popper (1972: 170 - 80) considers the case of Galileo's heliocentric hypothesis in general, and his theory of the tides in particular. Taking the first of these, it is clear that regardless of either Copernicus' or Galileo's psychological states, the heliocentric hypothesis was falsifiable. Whether Copernicus was aware of it or not, it was possible to deduce from this hypothesis that the inner planets of the solar system would show phases parallel to those seen on the moon. The most important point about this is that it is a consequence of the theory that phases will be

observable on the inner planets, and this relation 'consequence of' is a relation between theory, corollary and object. And while this is no doubt a complex relationship, it is not a psychological one. We do not invent this particular consequence, though we do invent the theory which leads to it; rather, we discover it, if we are fortunate and clever enough. A particular proposition which is derivable from a theory may, in fact, never be discovered; but this is not to say that it does not exist. And the fact that a consequence of a theory may exist without anyone's ever noticing it shows that its existence does not depend on factors regarding the cognitive makeup of its creator.

While it is clear that internal processes and states enter into the growth of scientific knowledge, it is a mistake to confuse these with objectively existing properties of theories and objective problem-situations. Popper demonstrates this by showing the non-psychological nature of Galileo's adoption of an untenable, and ultimately false, theory of the tides. The psychologistic interpretation of Galileo's position on the subject claims that some sort of psychological state internal to Galileo caused him to reject the notion of lunar influence upon the tides. Of the many psychologistic 'explanations' (jealousy, ambition, dogmatism, aggressiveness, etc) for Galileo's rejection of this notion, one is the hypothesis that he was 'psychologically attracted' to the idea of a circular motion (rather than an elliptical one).

Against this, Popper (op cit: 174) argues that such speculations are superfluous to an understanding of the relation between the theory and the problem situation; he points out that Galileo adopted a mechanical conservation principle for rotary motions, and that this principle, rather than Galileo's inner state, ruled out the possibility of interplanetary influences, such as the influence of the moon upon the tides. Thus, not only does Popper's approach allow us a greater understanding of the problem, its intellectual context, and Galileo's response, it makes it clear that, even if Galileo were 'psychologically attracted' to the notion of a circular motion, this would be irrelevant to the the problem, its theoretical context, and the validity of Galileo's theory.

Popper argues that it is a mistake to interpret scientific knowledge exclusively as a kind of psychological state, and that this mistake stems from a tradition in philosophy whereby 'knowledge' is interpreted solely in a subjective manner. Thus the term is taken to refer to, for example, states of mind such as certainty or strong belief (Musgrave 1970 gives a good survey of this tradition). Popper argues that these are quite distinct from objective contents of theories, which have nothing to do with belief. As an illustration of this, he cites Newton's attitude to the theory of interplanetary influence: Newton found it very difficult to believe that the theory could be valid, and thus was possessed of nothing like certainty or strong belief; and yet he still saw that the evidence suggested that the theory was valid. From this, we see that the attitude or mental state

of the proposer of a theory is quite distinct from the properties of the theory itself, which constitutes scientific knowledge.

Having thus argued for the autonomous existence of theories, their properties, and the problem situations they relate to, Popper goes on to argue that we ought to recognise the fact of interaction between such objective realities and our subjective internal states. He illustrates this (1972: 109) with a quotation from Heyting concerning Brouwer's invention of the theory of the continuum:

' If recursive functions had been invented before, he (Brouwer) would perhaps not have formed the notion of a choice sequence which, I think, would have been unlucky.'

(Heyting 1962: 195)

We can follow Popper in analysing this state of affairs thus: the forming of the notion of a choice sequence is an internal, subjective process. It arises in response to an external, objectively existing, problem situation. Part of that situation is the set of then extant theories, and Heyting is pointing out that, had this situation been different (had recursive functions been invented), then Brouwer's internal process of invention might not have occurred. The interaction occurs, then, between the external problem situation and the inner mental processes whereby we invent hypotheses, and also between the product of this invention and the problem situation, which is altered once our product becomes part of it and its resolution.

This strikes me as being not only appealing as a picture of the complexity of the process of the growth of scientific knowledge, but as a picture which stresses the objectivity of science; as such it has been attacked, naturally, by relativists such as Kuhn (1962), and even more extremely relativistic, Feyerabend. However, since Feyerabend (1975) actively denies that rationality is of any value as an intellectual tool, I think we can take that as a point in favour of Popper's objectivism.

The idea of interaction seems fruitful too; I have mentioned the notion that psychological states ('world 2' objects) can be said to interact with objective products of cognitive activity (which are 'world 3' in their ontological status), but one can also see the physical world ('world 1') and our psychological states interacting. Popper wants to say that our intellectual products may have an effect on, and be affected by, the physical world via our psychological states. Thus, our theories may influence our physical environment in any manipulation of the physical world we carry out, such as the building of bridges, tools, machines. In these cases, we can say that our theories are in fact embodied in the very stuff of the physical world: a machine is more than a collection of physical objects, its structure and function are embodiments of theoretical constructs.

Notice how distinct this approach is from that of the Platonists: for them, we would have to take these 'world 3' objects to be pre-existing objects which cannot interact



causally with our mental states. In Popper's framework, it is assumed that both mental states and objectively existing intellectual products are emergent realities, resulting from the evolutionary process:

'...in a material universe something new can emerge. Dead matter seems to have more potentialities than merely to produce dead matter. In particular, it has produced minds - no doubt in slow stages- and in the end the human brain and the human mind...'

(Popper & Eccles 1977: 11)

'I suggest that the universe, or its evolution, is creative, and that the evolution of sentient animals with conscious experience has brought about something new. These experiences were first of a more rudimentary and later of a higher kind; and in the end that kind of consciousness of self and that kind of creativity emerged which, I suggest, we find in man.

With the emergence of man, the creativity of the universe has, I think, become obvious. For man has created a new objective world of the products of the human mind.'

(op cit: 16)

It is important to bear in mind that this notion of emergence is closely linked with Popper's philosophy of physics, in particular his view of emergence and interaction in the physical world itself, and along with these, the idea that the physical world is not a closed system. Popper illustrates this view thus:

'...in a universe in which there once existed (according to our present theories) no elements other than, say, hydrogen and helium, no theorist who knew the laws then operative and exemplified in this universe could have predicted all the properties of the heavier elements not yet emerged, or that they would emerge; or all the properties of even the simplest compound molecules such as water.'

(loc cit)

Popper is therefore taking the physical world itself to be

'open-ended', capable of evolving in ways that cannot be predicted. This is important, I believe, because it means that if we adopt Popper's suggestions and try to apply them to linguistic objects, we at least adopt an ontology which rests on a clear conception of the nature of the physical world. This seems to me to be better than either assuming that we already have such a conception, without spelling out what form it takes (Botha, Smart), or assuming that our conception of the physical world might somehow change to allow us to interpret our theoretical constructs as 'physical' in some way (Chomsky).

Exactly how interaction between different emergent objects takes place is something that needs to be investigated in an interactionist programme, but that, of course, is what the research programme is set up to establish. A good example of how we can go about establishing the nature of such interactions is the study of the relationship between numeral systems and number in Hurford (forthcoming); it seems to me that this sort of work allows us progress in research and discovery because it is based on the idea that both numeral systems and numbers are emergent rather than Platonic. And with this sort of approach, we have a richer basis for proceeding with such research than we do with Botha's idea that somehow we can find specific neurons that will correspond directly with postulated linguistic objects. Popper's framework suggests that the picture is much more complex than this, and that such a reductionism would fail to reflect the richly articulated nature of the relationship

between physical systems, cognitive systems, and products of cognitive systems.

By adopting something along the lines of Popper's proposals, we avoid the pitfalls of reductionism (oversimplification, impoverished conception of ontological diversity) and the excesses of Platonism (excessive ontological diversity, absence of a conception of emergent realities). This strikes me as being a potentially fruitful way of interpreting, and building, our theories. In particular, it is interesting to apply Popper's notions to the study of linguistic objects themselves.

(ii) Their application to linguistic objects.

Popper has repeatedly argued that the emergence of language must have been central to the emergence both of higher mental capacities and of other world 3 objects such as scientific theories:

'One of the first products of the human mind is human language. In fact, I conjecture that it was the very first of these products, and that the human brain and the human mind evolved in interaction with language.'

(op cit: 11)

The emergence of language, with its descriptive and argumentative functions, is important for Popper, since by means of the language faculty, we are able to begin to formulate the beginnings of what later became scientific theories. And Popper makes a great deal of these functions of language in the evolutionary process (cf Popper 1972: 120, for example). My

concerns are less broad than Popper's; I want to disregard any particular function of language and the details of the question of how it emerged (as I've said in 4.1, I think these are irrelevant to the concerns of the grammarian). Rather, I want to assume that it is emergent\*, and has such functions, and to concentrate on its form, on what I have referred to as linguistic objects and their properties.

If we apply Popper's autonomism to linguistic objects per se, we assume that linguistic objects, such as linguistic systems\*, are products of human activity but have an existence which is very largely independent of the cognitive, social, and physical factors which contributed to its creation, but which nonetheless interacts with these. A great many interesting consequences follow from the adoption of this view. Under such a view of the emergence of language, we would expect to find, for instance, that we would have to distinguish between historical changes within the system itself and change which involves interaction with physical, cognitive and social factors. Certainly there would be no question of denying that there could be such a thing as change within the system independently of these other factors. This would rule out the sort of functionalist approach (such as that expressed by

\* Naturally, Katz could not deny that our language faculty is emergent; what he would have to deny is that linguistic objects themselves are emergent. Rather, he would say that they are pre-existent and that it is our innately specified knowledge of the notion 'abstract object' that is emergent. Unfortunately, this is no less unimpressive than the claim that the language faculty itself is pre-existent, as I have pointed out.

Itkonen 1978 where changes are said always to be triggered by changing 'social needs') which I have argued against above (4.2)

This means that, for this particular issue concerning historical change, that I want to follow Lass (1980) in arguing that there is nothing about the *form* of a linguistic change that can be accounted for on the basis of social factors. Rather, what these tell us about is the *implementation* of some change of form within a speech community; this in fact follows from the Saussurean arbitrariness of the sign principle.

The same line of approach would apply to the relationship between historical change and cognitive factors, such as perceptual constraints. While it is clear that any linguistic system must function within the bounds of the constraints on our perceptual system, it is equally clear that there is no reason to include such non-linguistic phenomena (such as limitations on short-term memory) in any grammatical description. Thus, even if we establish, for instance, that centre-embedding is difficult for humans to decode and that it therefore tends to be eliminated when it arises, this would not lead us to incorporate the perceptual constraint on centre-embedding into linguistic theory. For the interesting thing is that centre-embedding does arise and is then eliminated. This seems to me to suggest that the emergence of such a state of affairs is a purely linguistic phenomenon, describable solely in terms of the system itself, independently of perceptual factors, whereas the elimination, or lack of adoption, of such strategies as centre-embedding, is best described in terms of the interaction between

the linguistic and perceptual systems.

One could, of course, argue that speaking of linguistic change in this way is merely a 'façon de parler', and that this gives us no reason to assume that there really is an independent linguistic reality whose existence is distinct from that of the perceptual system. But such a response is in conflict with the sort of realism that I have proposed. Under my realist assumptions, the very fact that the adoption of certain constructs in speaking about the phenomena makes it easier to account for them is the basis on which we claim that such constructs be given a realist interpretation.

This sort of distinction between linguistic objects on the one hand, and the factors involved in their production and implementation is one that Popper stresses, under the notions product vs process:

'...few things are as important as the awareness of the distinction between the two categories of problems: production problems on the one hand and problems connected with the structures produced themselves on the other. My second thesis is that we should realise that the second category of problems, those concerned with the products in themselves, is in almost every respect more important than the first category, the problems of production. My third thesis is that the problems of the second category are basic for understanding the production problems: contrary to first impressions, we can learn more about production problems by studying the products themselves than we can learn about the products by studying production behaviour. This third thesis can be described as an anti-behaviouristic and anti-psychologistic thesis.'

(Popper 1972: 113-4)

There are many other interesting consequences which follow from the adoption of these theses for the investigation of linguistic objects, and also the relationship between autonomous

linguistics (theoretical linguistics) on the one hand, and psycholinguistics and sociolinguistic on the other. I have suggested what sort of view of the nature of historical linguistic change this allows us; it also has interesting implications for, among other things, the way we look at the relationship between (i) phonetics and phonology, (ii) syntax and discourse, and (iii) autonomous and non-autonomous linguistics. There is also a point about modularity in generative linguistics which ties in very nicely with these theses.

Before I proceed to these, however, I must mention a reaction to the sorts of methodological issue I have been discussing which goes something like this: such issues are largely peripheral to the business of actually doing linguistics; the position one adopts on them has no bearing on actual analysis and theory construction, and they are thus at best an interesting luxury, at worst a waste of time. What I want to suggest, and hope will become clear, in the following sections is that methodological matters are not just a question of how one interprets what one is doing, but are at the root of how one actually does it. Thus they are tied in to the actual business of linguistic analysis in a very fundamental way. What follows will therefore have a dual function: I want to give an idea of the sorts of approach to analysis that follow as a consequence of adopting interactionism, and at the same time I want to demonstrate that, with the adoption such a

methodological position, certain ways of doing linguistics are characterised as being methodologically ill-conceived.

I deal with the three areas mentioned above in order; I do not think that these by any means exhaust all of the consequences of an interactionist methodology, but I do hope that they give an impression of the range of these consequences.



### 5.3 Consequences of interactionism

#### (i) Phonetics and phonology

If we adopt a metatheoretical framework\* of the interactionist sort, incorporating the assumptions about reductionism and emergence that I have described, then it shapes the sorts of theoretical framework that one is willing to develop. That is, since one's metatheoretical assumptions have a direct influence on the content of one's theories, the particular metatheoretical assumptions I make will rule out reductionistic theoretical frameworks. This is well exemplified in the case of phonological theory and its relation to general phonetic theory.

If we accept that linguistic objects are emergent in the way that I have suggested they are, and that one needs a non-reductionist theory to fully account for them, one will be inclined to take as methodologically ill-founded a phonological theory which takes phonological phenomena to be explained principally in terms of purely phonetic factors. I will refer to such a metatheoretical position as phonetic reductionism or phoneticism.

To demonstrate the validity of my anti-phoneticist position, I will consider Natural Phonology (Stampe & Donegan 1979), Hooper's (1976) Natural Generative Phonology, and

\* I also use the expression 'metaphysical research programme', following Popper, to describe the set of assumptions on which a theory is founded; cf chapter 2.1 for discussion of this notion.

Ohala's metatheoretical claims about 'phonetic explanation' in phonology, all of which contain, in varying degrees, versions of phoneticism\*. I then discuss Foley's (1977) proposals for a phonetics-independent phonology, which I also take to be methodologically ill-conceived.

Since I have said, pace Boyd (1973), that metatheoretical frameworks are not themselves falsifiable, the only way to assess my anti-reductionist metatheory in relation to the phoneticist position as exemplified to a greater or lesser extent by Ohala, Hooper and others, is to see what sorts of theory they give rise to. If phoneticism yields theoretical frameworks which are of greater explanatory value than their non-reductionist counterparts, then it is to be more highly valued as a metatheoretical stance. I will argue that phoneticist phonological theories are in fact impoverished from an explanatory point of view, and that non-reductionistic frameworks possess a higher explanatory yield, thus lending credence to the kind of non-reductionist metatheory that I propose.

I will also argue that the particular sort of non-reductionist programme I assume, with its emphasis on interaction between ontologically distinct objects and systems, is especially attractive when one considers the inter-

\* These are, of course, quite distinct theoretical frameworks, and I do not suggest that they amount to the same theoretical position. However, they all incorporate versions of phoneticism, as I will show.

relationship between phonological and phonetic domains. I suggest that phonological theories which do not assume such an interaction, such as that proposed by Foley (1977) are just as impoverished as phoneticist theories. The position on the relationship between phonetics and phonology that emerges from my interactionist framework is this: that phonetic considerations are a necessary, but not a sufficient, part of the explication of phonological phenomena. Phoneticism amounts to the claim that such considerations are both necessary and sufficient in this way, and Foley's position (which I will call 'abstractism') amounts to the claim that they are neither necessary nor sufficient.

Firstly, then, Donegan & Stampe's (1979) proposals. They propose that explanation in phonology can only be achieved in terms of 'forces implicit in human vocalisation and perception' (126), thus:

'Natural Phonology properly excludes the topic of unmotivated and morphologically motivated alternations. Although these have often been lumped together with natural alternations in generative phonology, they should be excluded from phonology if it can, in principle, furnish no understanding of them. Of course, such alternations typically stem historically from phonetically motivated alternations, and these are in the province of phonological theory, as are the factors whereby the phonetic motivations were lost. The natural subject matter of explanatory theory includes all and only what the theory can, in principle, explain. In the case of natural phonology, this means everything that language owes to the fact that it is spoken. This includes far more than it excludes. Most topics which in conventional phonology have been viewed as sources of 'external evidence' are in the province of natural phonology as surely as the familiar matter of phonological descriptions.'

(127 - 128)

D & S tie these metatheoretical claims in with a hypothesis about the child's acquisition of phonological structure, namely that there are a set of 'natural' processes (eg word-final devoicing of obstruents) which the child will bring to bear on its language, but which may be 'supressed' in response to the state of affairs in the language of its learning (eg the child may simply have to supress the natural, process of word-final devoicing, as in English).

Some aspects of D & S's metatheoretical claims are rather worrying. For instance, if phonological theory is to include 'all and only what language owes to the fact that it is spoken', it is likely to be greatly impoverished as a result. Take their discussion of the [s]:[z] alternation in German (as in [haus]:[hauzə]). They claim that their anti-conventionalist approach to language, whereby it is seen as a 'natural reflection of the needs\*, capacities, and world of its users, rather than as a merely conventional institution' (127) enables them to single out the important and 'natural' fact that the [s]:[z] alternation is 'distinct in its nature, evolution, psychological status, and causality from the phonetically conventional aspects (of phonology)' (127). I find this odd. It is surely only by virtue of the fact that there is a phonological opposition between [s] and [z] in German that there can be said to be an alternation (as opposed to a free variation) between these; since this opposition is at the very

\* For comments on the limitations of this sort of functionalism, cf 5.1 (ii).

least fairly largely conventional, I cannot see how D & S can avoid reference to conventionality in order to make any sense of this particular aspect of German consonantal phonology.

I accept, of course, that the voiced/voiceless distinction among consonants is hardly uncommon, and that it is a phonetic phenomenon which is natural in D & S's sense. However, I think the limits of D & S's phoneticism are evident even where this extremely widely phonologised distinction is concerned. Rather simple cases such as the German alternations cannot be adequately explicated without reference to the conventional aspect of phonological alternations. And nor can the more complex, and more interesting, cases such as the (by now classic in the history of phonological theory) Russian devoicing rule, involving as it does a situation where there is an assymetry in the system as far as the voiced/voiceless distinction in obstruents is concerned.

It is not clear to me how D & S are to account for the facts of the devoicing process here. This assymetry, whereby voicing is distinctive among one subset of the Russian obstruents, but non-distinctive amongst another is an instance of the conventional nature of phonological systems; in order to capture the nature of the devoicing process, it is essential that we demonstrate that it operates over both sets, such that we end up with two distinct consequences of the devoicing process, one in which 'phonemic overlapping' occurs, and another in which it does not.

A rather less extremely phoneticist approach is adopted by

Hooper (1976), and also by Venneman (1974 and elsewhere). Natural Generative Phonology\* rests upon the supposition that the object of phonological theory is a speaker-internal reality (a representation stored and accessed by the speaker. Hooper makes specific claims about how the speaker arrives at this representation, as follows: she claims that speakers make generalisations across surface forms, ie from one surface form to another, rather than from an underlying representation to a set of surface forms. This is the essence of her True Generalisation Condition:

'...all rules express transparent surface generalisations, generalisations that are true for all surface forms and that, furthermore, express the relation between surface forms in the most direct manner possible...the rules speakers formulate are based directly on surface forms...these rules relate one surface form to another, rather than relating underlying to surface form.'

(Hooper 1976: 13)

Hooper takes this to be an important contribution towards constraining the notion 'possible rule' in phonology, since rules will not be of the sort that relate underlying to surface forms. This would be a gain, of course, assuming that one did

\* An interesting historical note: this section was written when these frameworks still constituted the most recent developments in phonological theory since the heyday of the SPE model. They now appear to have faded into the background considerably, and traditionally (in generative work) non-phoneticist models of phonological structure (Lexical Phonology, Autosegmental Phonology) have emerged as the most attractive new developments. This may, of course, be taken to reflect the faddish nature of theory development in linguistics, but it just might reflect the fact that phoneticism was bearing little fruit, perhaps because it is methodologically unsound in the way that I suggest.

not in the process diminish the explanatory power of one's phonological theory. However, I think it can be shown that this is precisely what happens, and it is not surprising that this should be so if my metatheoretical assumptions are on the right track.

Consider a particular case in which Hooper's principle is applied. She proposes that the /e/ epenthesis phenomenon in Spanish is not describable in terms of a *phonological* rule, since *all* surface sequences of /s/ + C are preceded by /e/ in word initial-position: there is no alternation between surface forms here over which a generalisation can be made, and there is thus no methodological justification for a rule of /e/ epenthesis of the sort Harris (1969) describes, thus:

$$\emptyset \text{ ---> [e] / \_\_\_\_ s [+cons]}$$

Her methodological position therefore forces her to abandon this as a phonological rule of Spanish and to opt for an analysis whereby all the esC\_\_ forms are entered *with* the epenthetic vowel in the lexicon. This leaves Hooper with the problem of accounting for Spanish speakers' pronunciation of foreign words with sC initial clusters. She achieves this by proposing a syllable structure constraint on Spanish which syllabifies sC clusters with a syllable boundary between the /s/ and the C and inserts a V before the /s/ (it is said to be 'preferred' for the epenthetic vowel to be inserted before, rather than after, the /s/ since this allows the original order of C's to remain). Hooper's task is still not complete, however, since she is still left with the problem of specifying

the nature of the vowel insertion.

Hooper appeals here to one of two putative universal principles: (i) that an epenthetic vowel must always be the 'minimal' (ie the weakest) one and (ii) that it must be a V whose features are copied from a nearby segment (she does not specify how 'nearby' is defined, and her formalisation of this second principle fails to indicate what it means). She claims that Spanish utilises the first of these, on the basis of factors to do with 'the prosodic characteristics of the language'\*

The final analysis is still not accomplished, even after this amount of theoretical apparatus has been brought to bear on the problem: Hooper must still specify the precise quality of the epenthesised vowel. She does this by appealing to a language-specific strength scale for Spanish vowels, whereby /e/ is the weakest, and therefore the one to be inserted (in accordance with the principle mentioned above).

Now, quite apart from the clearly over-elaborate nature of this analysis in comparison with the traditional one, there is evidence that supports the traditional analysis. Harris has pointed out (1979: 290-291) that allomorphs of the diminutive suffix vary according to the syllabic structure of their base forms, with /\_cita/ occurring with bisyllabic bases and /\_ita/ with bases consisting of more than two syllables

\* Specifically, to do with the notion that Spanish is stress-timed and therefore has vowel weakening.



(madre:madrecita vs comadre:comadrita). He points out that /esC/ forms, which have a *phonetically* trisyllabic structure, pattern like bisyllabic forms (estudio:estudiecito, espacio:espaciecita, but \*estudito, \*espacito). This is evidence that the proper lexical representation for these forms is just as Harris' analysis would suggest, ie without epenthetic vowel.

Thus, there do seem to be strong reasons for preferring the traditional (Harris-type) analysis over the Natural Generative Phonological one, and the metatheoretical implications of this are clear. The adoption of the sort of metatheory which Hooper proposes stresses the priority of (phonetic) substance over (phonological) form:

'A growing body of data shows that an interest in the way speakers analyse their language seems inevitably to lead to the study of substantive rather than formal principles of analysis, and substantive rather than purely structural evidence...NGP is an appropriate framework for the study of substantive principles.'

(op cit: 106)

This is a statement of what I have called phoneticism, and it is subject to the following criticism: that there is no reason why we should be forced to choose between a metatheory for phonological investigation which allows only for purely structural/formal evidence and analysis (Foley's is, as we shall see, and is consequently impoverished) and one which relies over-heavily on substantive principles of analysis (in the way that Hooper's does). Rather, an interactionist programme of the sort that I propose allows for the interaction of substance and

form in a way that neither phoneticist nor abstractionist frameworks do.

Additionally, it appears that this sort of phoneticism fails to provide us with heuristically more fruitful theories: the sorts of analysis one gets in adopting it are impoverished.

There is an interesting historical slant to the emergence of phoneticist metatheory in phonology in the seventies too: it is reminiscent of a very similar metatheoretical position adopted by structuralist phonologists in the thirties and forties (cf 2.2 for discussion of this), with its insistence on divorcing morphologised alternations from phonetically motivated ones, and the concomitant notion of giving priority to purely substantive factors\*. There are parallels in the consequences which follow from this, too: just as reductionistic phonological metatheories forced structuralist analysts to tolerate analyses which failed to capture the generalisations at hand (one is reminded of the famous Bloch problem regarding phonemic overlapping, or the much cited problem with Russian obstruent devoicing which I referred to above), so Hooper's NGP fails to capture generalisations and over-complicates the analysis of

\* It may well be the case that one wants, counter to the spirit of the SPE framework, to distinguish between morphologically-orientated alternations and those which are motivated on a purely phonetic basis. Thus, a recent theoretical development, Lexical Phonology, as outlined in Mohanan 1986, proposes a change in the way a model of phonological organisation represents these two sorts of alternation. However, the adoption of such a distinction need have no basis whatsoever in phoneticist metatheory, as indeed is the case with Lexical Phonology.

phonological phenomena.

I have one final remark to make on the methodological underpinnings of NGP, and it concerns Hooper's view that the object of analysis crucially relates to 'how speakers analyse their language'. I have suggested that an interactionist version of autonomism would allow for the possibility of dealing with linguistic objects per se, to a large extent independently of facts about speakers. This means that one need not be over-concerned with facts about speakers; at the very least, the interactionist can point to the fact that Hooper's concern with the idea that speakers generalise over surface forms does not lead to noticeably improved phonological analyses (if anything, the reverse is the case).

However, what if we had some kind of reliable evidence that speakers do indeed generalise over surface forms and utilise the sort of complex apparatus Hooper's analysis involves? Would we then be forced to abandon our rather simple and insightful analysis? I think not; I would be willing to say that in such a case, speakers are going about the task of computing a given function in a rather inefficient manner. However, it may be that speakers incorporate inefficient algorithms for the computation of a given function. What we can claim for our elegant analysis is that it is an efficient algorithm for the function in question, and furthermore, that if speaking about the phenomena in this way is fruitful, then we are justified in claiming real status for the analysis we propose (real status independently of the facts about speakers). However, as this

is, I suspect, the most difficult of my suggestions to swallow, and no doubt appears eccentric if not perverse, I will not pursue the point here.

It might be argued that I have not attacked phoneticism at its strongest point, and to deal with this possible objection, I want to consider Ohala's metatheoretical position on the relationship between phonetics and phonology. In his 1974 paper 'Phonetic explanation in phonology', he argues that the phonologist begins his task with 'sound patterns'\* and must then, if he is to be taken to be engaging in a scientific enterprise, seek to explain these. He may, of course, opt merely to construct a taxonomy and avoid the business of explanation.

However, science, claims Ohala, is essentially about explanation, and a scientific approach to the investigation of sound patterns would mean constructing an explanatory theory rather than a non-explanatory taxonomy. The bulk of Ohala's argument is that much of the theory construction in modern phonology is taxonomy masquerading as theory. That is, Ohala wants to say that the theoretical constructs devised and used by theoretical phonologists are lacking in 'empirical content' (253) and therefore are to be taken to be mere labels with no explanatory force. Such theoreticians are therefore, Ohala

\* By 'sound patterns', Ohala means 'regularities in the behaviour of speech sounds' (251). As examples of what he takes these to be, he cites historical sound changes and synchronic alternations (as products of such changes).

argues, crypto-taxonomists. Since Ohala wants to engage in what he takes to be genuine explanation in phonology, he argues that phonologists must abandon such crypto-taxonomic labels and seek genuine explanations of phonological phenomena. These, he argues, are to be found in a wide variety of areas, but most notably in phonetics (phonetics is 'one of the most important tools' (270) we have in attempting to provide explanations for phonological phenomena).

Thus, his view of the relationship between phonetics and phonology is this: that phonetics is a sub-part of phonology, and that phonological phenomena cannot be explained without reference to phonetic factors. To claim that phonetics is a distinct discipline from phonology is absurd, he claims (270: n. 3), since phonology is about explanation, and explanation in phonology is achieved principally by means of reference to phonetics. This view he takes to be in opposition to that expressed by Ladefoged (1971), who argues that a discipline must have a set of primitives which are defined outside of the theory in question, and that these primitives in phonology are provided by articulatory and acoustic phonetics, taken to constitute a domain outside of phonology proper.

It is perhaps best to assess Ohala's metatheory in relation to a particular problem of analysis, as I have done with Hooper's and Donegan & Stampe's proposals. In Ohala & Lorentz (1977), the analysis and status of labial-velars is discussed as a means of exemplifying the metatheoretical stance adopted by

Ohala. Here, it is argued that constructs in theoretical phonology are not to be granted a realistic interpretation, that it does not make sense to speak of a phonological reality distinct from phonetic reality, and that explanation in phonology is only achieved when phonological statements are reduced to phonetic statements (which, I assume, are taken by O & L to be interpreted realistically), thus:

'It is unnecessary to posit that the phonetic character of a segment differs from its phonological or 'underlying' character unless the latter terms are defined in fairly innocuous ways.'

(578)

By 'innocuous', they mean definitions whereby terms in phonology are merely 'descriptively convenient' (591), ie to be given an instrumental, rather than a realistic, interpretation. They distinguish between truly explanatory scientific theories and 'impressionistically-based, pre-theoretical taxonomies' (577), associating phonological descriptions with the latter and phonetic ones with the former. They take the SPE phonological framework to be an instance of such a pre-theoretical taxonomy which is awaiting proper 'scientific' reduction into the 'explanatory' terms of phonetics. The SPE treatment of labial-velars (in Chomsky & Halle 1968 and Anderson 1976, for instance) is thus seen by O & L as mere taxonomic pigeonholing; they suggest that a purely phonetic account of labial-velars would be truly explanatory.

Let us consider their proposed phonetic explanation. They take the acoustic properties of labial-velars to be the phonetic

key to an explanation of their behaviour, as follows:

'...labials and back velars produce similar acoustic effects...the explanation for this requires reference to the standing wave patterns of the resonant frequencies of the vocal tract...the rule is: a constriction at a velocity minimum raises the resonant frequency from what it would be for a uniform tube; a constriction at a velocity maximum lowers the resonant frequency from what it would be for a uniform tube...(this) explains why a constriction in either the labial or back velar position will have the similar acoustic effect of lowering the second formant and why simultaneous constrictions at both labial and velar regions will lower it even more.'

(582)

Taking this acoustic data combined with the articulatory and acoustic properties of nasals, they show that it will always turn out that labial-velars will behave like velars when it comes to nasal assimilation (we expect [nw] to assimilate to [ŋw] but not [mw], for instance). They then conclude, contra Anderson (1976), that labial-velars are not 'phonologically' either labial or velar, but, as their phonetic description suggests, both labial and velar; they are unitary, not ambiguous, in character, and their variable behaviour, whereby they function in some cases like velars and in others like labials, can be explained on the basis of either purely phonetic data (as in the case of nasal assimilation) or on the basis of historical development, which itself is to be accounted for in terms of purely phonetic motivation of sound changes.

What O & L especially object to in the 'mere pigeonholing' of labial-velars into either a velar or a labial phonological category, is the suggestion that a single phonetic phenomenon might be said to have variable systematic status in different languages (or, as we shall see, within a single language). In

fact, Ohala disparages the idea of gaining insight into linguistic organisation by reference to linguistic systems:

'...the behaviour of speech sounds is better understood by reference to system-external factors than system-internal factors.'

(Ohala 1979: 46)

It seems to me that Ohala's evidence is very encouraging for the sort of Interactionist methodology I propose. Recall that I propose a realistic interpretation (assuming it is justified for a particular construct) of constructs in phonetics, and also of those in phonology (again, assuming that we satisfy 'warranted assertibility' conditions). That is, I want to allow that it is coherent to take phonological systems, phonological rules and phonological representations to be realities (of what sort, our theories will hopefully allow us to guess) in addition to allowing of phonetic realities.

To try to spell this out, and to show how it contrasts with Ohala's view, consider his comments on the status of labial velars. He allows that there may be straightforward acoustic phonetic accounts of why these behave as they do under nasal assimilation. I am perfectly happy to accept that this is indeed an explanation for the assimilation facts. It is parallel, but in acoustic terms, to the phonetic explanation as to why velars front before front vowels. I am also willing to accept that synchronic alternations between velar-like and labial-like behaviour of labial-velars (and between velars and palatals in my acoustic example) may well stem from such purely phonetically-motivated states of affairs.



However, these purely phonetic phenomena (assimilations in this case) are rather dull in and of themselves, from a theoretical point of view. What is of linguistic interest is the effect such phenomena have on linguistic systems. Thus, a fronting of velars to palatal place of articulation can have interesting effects on the way the linguistic system is structured (and linguistic systems are real, as far as I'm concerned). This is true, for instance, for French, where realisations of /k/ collapsed into realisations of /s/. Indeed, this sort of phenomenon (mergers, splits, in phonological systems) is part of the stock in trade of the historical phonologist. One does not doubt that, as Ohala points out, synchronic states of affairs can result from purely phonetically-motivated processes, but it is the effect these have on a linguistic system that is of interest to the linguist. What we are dealing with in these cases is the interaction of form and substance, not the reduction of formal (systematic) states of affairs to substantive ones.

To illustrate this, consider Anderson's (1981) reply to Ohala. He cites data from Fula to show that we must postulate two distinct phonological units (segments, in SPE terminology) underlying surface occurrences of the phonetic segment [w]. In stems with initial consonants which are continuants, there are alternations, in certain environments, which consist of prenasalised stops at the same place of articulation as the underlying continuant (he does not actually cite any other than

the labial velar, but I take it these are of the /l/ --> [nd] sort). The interesting cases are the alternants for underlying /w/. It turns out that these are of two different sorts, as follows:

- (a) underlying /war/ ('kill') has alternants [war] and [mbar]
- (b) underlying /war/ ('come') has alternants [war] and [ŋgar].

These two sets of alternants are symptomatic of the behaviour of two large sets of morphemes which consistently have labial, but not velar, alternants, as in the 'kill' morpheme, or velar, but not labial, alternants, as in the case of 'come'.

This data leaves us with the rather evident phonological solution of distinguishing, within a single system, between two different labial-velars: one which we must represent as primarily a labial segment, and the other as primarily a velar one. The fact that there may have been a strictly phonetic motivation for this in the historical development of the language does not alter the synchronic fact that a single phonetic segment is phonologically ambiguous, rather than unitary.

There seems to me to be little methodological justification for interpreting the phonological analysis here as 'merely taxonomic pigeonholing': we simply cannot give an adequate analysis of the data without reference to the constructs 'system' and 'systematic unit', and according to the version of realism I adopt, this constitutes a satisfaction of the warranted assertibility condition.

In asserting that these postulated phonological units are

real, we are doing precisely what we do when we assert that phonetic constructs are to be interpreted realistically. To take an example, the construct 'wave', so crucial to acoustic phonetics, could be interpreted instrumentally as a kind of shorthand for a set of sense experiences and their properties in the Mach (1893) 'economy of expression' sense, but we are justified in interpreting it realistically in just the same way that we are justified in interpreting phonological constructs realistically: in terms of heuristic fertility.

Let us assume, then, that we can speak of a phonetic and a phonological reality and of an interaction between the two (phonetic events will often have far-reaching consequences for phonological systems, for instance), and that something like Interactionism is the appropriate framework for constructing phonological theories. We will be inclined to reject phoneticism on the grounds that it forces us to adopt impoverished analyses and reject elegant analyses which capture interesting generalisations. With such a framework, we will also be inclined to reject abstractionist methodologies which do not allow for the often complex ways in which phonetic and phonological realities interact. Such a metatheoretical position is argued for by Foley (1977).

Foley's position on the nature of phonological theory could not be further removed from that of Ohala. While Ohala thinks that theoretical constructs in phonology should not be interpreted realistically, and are to be reduced to acoustic and

articulatory states of affairs, Foley's view is as follows:

'The basic phonological elements are defined not by physical acoustic or articulatory parameters, but rather by their participation in rules.'

(Foley 1977: Foreword)

That he takes phonetic reductionism to be a methodological error (as I do) is evident:

'In the construction of a theory, the basic elements must be germane to the theory; just as the basic elements of a psychological theory must be psychological, so the basic elements of a phonological theory must be phonological elements. Chomsky & Halle, in attempting to create a phonological theory\* based on phonetic elements, consequently commit the reductionist error. A scientific linguistic theory would be based, not on physical properties of elements, but on abstract relations.

Conceptually we can recognise two types of features, phonological features, which refer to phonological relations, and phonetic features, which characterise the manifestations of the phonological units as sounds.'

(op cit: 5)

Foley cites the argument concerning the linguistic nature of systems which do not use speech sounds as their mode of manifestation to show that phonology is essentially not about speech sounds, but about relations between units in an abstract system. He takes *rules* to be the object of theoretical phonology, not speech sounds, and argues that a conflation of these two distinct realities constitutes an impoverishment of any theory of phonology. In this I am very much in agreement

\* It is interesting to consider the extent to which this framework has functioned as an orientation point for a multitude of theoretical issues, as well as metatheoretical ones. Ohala and Foley, representing metatheoretical poles, are equally critical of its methodological basis; I suggest that this is because its methodological basis is not sufficiently explicitly spelled out: cf chapter 2 for discussion.

with him. I will not attempt to argue about his characterisation of the SPE framework as being based upon a reductionistic methodology, though see my comments in chapter 2 on Chomsky's interpretation of 'systems' and Halle's metatheoretical remarks. Rather, I will concentrate on his views on the relationship between the phonetic and phonological domains.

Having made the entirely reasonable point that phonological realities are not phonetic realities, Foley isolates specific phonological elements and their phonetic realisations for discussion. He cites strengthening and weakening phenomena as evidence of phonological strength scales, and sees these as consisting of abstract phonological units, ordered according to their strength or weakness. Thus there is a scale which indicates propensity to spirantisation; this propensity is taken to be 'a manifestation of an abstract relation among phonological elements' (28) which Foley labels  $\alpha$ . The scale indicates that velars are the weakest category in relation to spirantisation propensity, followed by dentals, with labials as the strongest.

Foley then establishes a  $\beta$  relation, which looks rather like a version of the sonority hierarchy and denotes the relation between, for instance, voiced stops and voiced continuants, the latter being weaker than the former. This is then followed by the citing of several other labelled relations concerning the phonological strength relations among different phonological units.

It is difficult, even considering the nature of the

elements mentioned so far, to escape the temptation to view these units and scales as being in some way tied in to phonetic factors. Spirantisation, for instance, typically occurs intervocalically, whose articulation, with a stricture of open approximation, and whose typically voiced nature suggest a correlation between the environment in which spirantisation occurs and the articulatory properties of the affected segments. Voiced fricatives seem plausibly viewed as intermediate between voiced stops and vowels, in terms of degree of stricture, and the process whereby voiced stops spirantise looks overwhelmingly like a simple case of assimilation of degree of stricture. In fact, it seems rather over-apparent even to mention this. Articulatorily-based versions of Foley's scale have been proposed (cf Lass 1976, Anderson & Ewen 1980 for suggested phonetically-based interpretations of this scale) and are very feasible.

Foley, however, is obliged to treat this sort of scale as a purely formal scale demonstrating relations among entirely abstract phonological units, without any specific interpretation. On the question of the phonetic realisation of phonological units, he claims (48 - 49) that the same phonological elements receive different phonetic manifestations in different languages, but identical manifestations in a single language. However, to admit that this scale has a phonetic basis is not to commit the error of phonetic reductionism, but to allow for complex modes of interaction between phonological

and phonetic realities. Thus, while spirantisation effects are themselves phonetically motivated, the effect of spirantisation on an abstract phonological system will depend on factors peculiar to the system in question. To quote Saussure (1916/1965: 142), 'the system, in other words, contains the seeds of its own evolution'.

It is interesting to note, finally, that both Ohala and Foley consider that the adoption of their respective methodologies would amount to assuring that the enterprise of constructing phonological theories is a truly scientific one. In Ohala's case, consideration of the realist/instrumentalist debate and its relevance for linguistic theory construction casts doubt on his conception of generative phonology as a pre-theoretical taxonomy. In Foley's case, there is no explicit statement of an ontological framework in which purely abstract phonological units are to be interpreted. With the adoption of a realist and interactionist methodology, I think we go some way towards characterising the nature of phonological inquiry, both in its methods and object of inquiry. And I think there are interesting observations about the methodological status of syntactic inquiry that emerge from interactionism.

#### (ii) Syntax and discourse

In this section I want to make a few observations on the consequences of an interactionist methodology for the way we regard the relationship between syntax and discourse. Just as I

argued against the reduction of phonology to phonetics, so I will argue that linguistic realities of a syntactic sort must not be confused with, and cannot be reduced to, facts about discourse, or communication in general. This is a consequence of adopting a fairly strong version of the autonomy thesis; if one accepts that autonomous linguistics is a valid enterprise, one is bound to reject the sort of metatheoretical position adopted by (among others) Givón. Givón's approach to the study of linguistic objects 'views data of language use, variation, development, behaviour, discourse processing and experimental cognitive psychology as part and parcel of one empirical complex.' (Givón 1984: 10). Givón takes the separation of linguistic systems from facts about the speaker, hearer and communicative context to be only a necessary preliminary methodological step which if practised (in the way it has been in generative linguistics) is 'bound to yield unsatisfactory results' (loc cit).

That is, Givón is arguing, as I have, that one can evaluate a methodological framework in terms of the results of the theories it gives rise to. However, that is as far as we agree, since Givón assumes that the putting into practice of autonomism yields poor results in comparison with the adoption of what might be called a holistic methodology, where facts about, among other things, communicative function and general cognitive abilities are able to serve as explanatory devices. I argue that it is this holistic methodology which yields poor results. Far from assuming, as Givón does, that the division between



theoretical linguistics and the general study of human behaviour and psychology is artificial, I will argue that it is only such divisions that allow us to make any progress, and this, in accordance with the sort of realism I adopt, is sufficient reason for assuming that it is a reasonable guess at the way the object of inquiry actually is. Nor does this prevent my allowing that strictly linguistic states of affairs cannot be somehow tied in with facts about general cognitive abilities, communication, etc. What I want to maintain is that it is only through a non-holistic methodology, one where there are distinct but interacting domains, that we can make any headway; and if this is so, that is our justification for saying that things really are the way our theories (and their accompanying metatheory) say they are.

Consider Givón's comments on the status of the investigation of sentences. The theoretical construct 'sentence' is at the very heart of all theorising about syntax in the generative framework. It is part of the foundations upon which generative syntactic theories are built; if they have allowed us insight into the nature of human language, then the 'sentence' construct has been crucial in our achieving what we have achieved. Yet Givón allows only that sentence grammars are a necessary methodological preliminary, and that in practicing sentence-level description and analysis, one must beware the danger of ignoring the 'semantic-functional correlates of syntactic structure' (*loc cit*). This danger can only be avoided

if one proceeds onto 'the next stage of syntactic investigation - the study of texts, and the study of the functional distribution of various morpho-syntactic structures within the text.' (loc cit). Of this first stage, sentence-level analysis, Givón points out that it 'only tells the linguist that some structures are possible, may occur. It reveals nothing about the context and purpose of their occurrence, or how often they occur in comparison with other constructions that seemingly perform "the same" or similar function(s)' (11). Thus, the linguist, to establish these things, must engage in quantification and statistical or probabilistic analysis.

Several comments are in order here. Givón assumes that sentences (he uses the term interchangeably with 'structures': I do not know what the metatheoretical status of 'structures' may be in his overall conception of the nature of linguistic inquiry) may or may not occur. It is not at all clear that they may. If these linguistic objects can be said to be real (I have argued that they *are* to be taken to be real) then they cannot be viewed as spatiotemporally located events, as Itkonen rightly observes. But nor can they be argued to be acts either (one cannot perform a sentence). That is, sentences are not the sorts of thing that may *happen*. Rather, they are the sorts of thing that I have described as belonging to an ontic category of the Popperian 'objective knowledge' sort.

If this is so, then sentences cannot, by definition, occur in sequences in a text (this is simply a more restricted version of the more general claim that they cannot occur at all). And,

naturally, one cannot determine their frequency of occurrence. Nor does this mean that we take them to be Platonic objects (I have explicitly rejected such a view), as Givón (12-13) assumes\*. And this is the point (it is very much a metatheoretical one) at which autonomists and holists, such as Givón, depart. I am suggesting, therefore, that Givón is simply committing a category mistake here, which is unfortunate, as it is located within the very foundations of his metatheoretical position.

The autonomist is not committed to Platonism here since, in order to avoid Platonism, it is incumbent upon him only to show that sentences are not pre-existing objects, unavailable for causal interaction with mental processes. We do this by showing that in syntax there is something to be explicated; in order thus to understand and gain insight into our object, it turns out to be essential to make a distinction between sentences and the sorts of object that occur in texts, namely utterances. If this distinction is essential, that is our warrant in asserting the reality of the two entities postulated. That the first of these, the sentence, is not either a spatiotemporal event or an act is one of its defining properties: it is this fact that causes us to want to make the distinction in the first place.

\* One of Givón's more peculiar claims is that Chomsky's interpretation of syntactic categories is Platonic. Note that his error here is similar to that made by Botha, discussed in 3.1. One might say that the meaning assigned by both of these writers to this term is something like 'more abstract than my world view can tolerate'.

This point is, to say the least, of great methodological importance: it is at the heart of what the generative enterprise is all about (and it is Givón's rejection of the sentence/utterance distinction as more than an expedient preliminary that marks off his work as non-generative). It has recently been made, very explicitly, by Burton-Roberts (1985) in relation to the work of another non-generativist, Werth (1984), who shares many of Givón's methodological predilections.

Burton-Roberts (henceforth B-R) points out some of the confusions which arise if the sentence-utterance distinction is collapsed in the way that Givon and Werth have collapsed it. One ends up with conceptually confused notions such as 'incomplete utterance', where incompleteness is predicated of utterances. But such a predication is surely incoherent: every utterance is complete: it is by virtue of their having a temporal beginning and end that they count as spatio-temporal objects. As B-R points out, the locution 'incomplete utterance' can only make sense if we interpret it as 'the (completed) utterance of a sentence fragment rather than a complete sentence'. And if we have to adopt the sentence/utterance distinction to make sense of our object of inquiry, we are warranted in interpreting it realistically.

A parallel conclusion must be drawn from the attempt (on the part of Lyons 1971, for instance) to predicate grammaticality of utterances. B-R argues that it is only in virtue of the 'sentence' construct that we can make any sense of

utterances, and therefore of texts. I think that this point is parallel to the one I have been making about phonetic objects: speech sounds become an object of theoretical inquiry only in virtue of their relationship to phonological objects, which are not spatio-temporal. It is not mere 'artificial modularism', therefore, to draw a sharp ontological distinction between phonology and phonetics and between syntax and discourse; rather it is essential to the business of gaining insight into our object of inquiry. I think this becomes apparent when one considers modularist and holist issues when examining the relationship between theoretical linguistics and neighbouring disciplines.

(iii) Modularity and holism

A few comments are necessary, at this stage, on the sort of overall picture of the organisation of linguistic models which follows from the adoption of interactionism. I have said that its adoption supports the notion of an autonomous theoretical linguistics, and that this is crucially linked to the idea of autonomism within theoretical linguistics, to the modular picture of the organisation of a grammatical model containing descriptive devices for syntactic, semantic and phonological phenomena.

It is clear that this kind of modular approach is most commonly countered by a different sort of metatheoretical approach in which modularity is abandoned, and some version of

what might be called 'holism' is adopted. These two differing metaphysical research programmes might be summed up, rather superficially, as follows: modularism, interpreted realistically (in the way I want to interpret it) amounts to the claim that the object of inquiry is best characterised in terms of a set of discrete but interacting sub-parts, whereas holism typically incorporates the assumption that the notion of 'interaction between modules' is not a fruitful one. Just what a given holist wants to replace this idea with varies from one writer to another (and very often, it is not clear precisely what conception of the organisation of linguistic objects and their structure the holist is proposing).

An interesting example of such an approach is that taken by Russell (1987 a), who describes the notion of 'interaction' as 'promissory and pseudo-scientific' (224). I try to show, here and in Carr (1987 b), that the version of holism that Russell proposes leads to hypotheses that, at least as far as the explication of linguistic phenomena goes, are unfalsifiable (and therefore non-scientific). Thus, although it would be not quite accurate to say that holism itself (or at least Russell's version of it) is unscientific (it is a metaphysical research programme, not a theory, and is not in itself falsifiable), it would be accurate to say that, as a metatheoretical position, it does not lead to falsifiable hypotheses, and is therefore to be less highly valued than one which does. Of course, nor can one say that modularism is falsifiable itself, but one can point to the successes of falsifiable theories based upon the modular

approach as evidence that it is to be more highly valued than holism.

Notice that modularism has tended to be embedded within what has come to be called the 'computational theory of thought' or the 'computation theory of the mind', in the sense in which Fodor (1976, 1983, and elsewhere) uses the expression. And it is the computational theory of thought, including modularism, that Russell is attacking.

A couple of points are in order here. The first is that one should really be speaking of the computational *meta*-theory of thought, since this view of mental representations and processes is an overall picture, a metaphysical research programme, from which particular theories may be evolved. The second is that there is no *necessary* link between a broadly 'computational' metatheory and modularity. While it is evident that computational models can be devised on a modular basis, non-modular computational models are entirely feasible (cf the work of Schank for this sort of non-modular computational approach to models of natural language understanding). It would be mistaken, therefore, to assume that arguments against modularism are necessarily arguments against a broadly computational theory of cognitive processes.

My response to the version of holism that Russell outlines might be termed the 'what else?' defence of modularism (to use a phrase of Dennett's). An example of a problem that Russell wants to deal with from a holistic point of view, or rather the

syntactic problem which Russell claims is not a problem, syntactic or otherwise, is that dealt with in transformational work by means of the NP-Aux Inversion rule. Consider Russell's holistic response to the fact that the following is ungrammatical (Russell will not use the expressions 'grammatical' and 'ungrammatical', since he does not recognise either these or the construct 'sentence' to which they relate):

\*Is the car which late will leave first?

Russell's comment on this is that 'action is hierarchical' and that one cannot 'lift the first occurrence of 'is' out of a wh-clause to form an interrogative for the same reason that one cannot release one's hand from a cup of hot coffee in order to position a saucer whilst returning the cup from the lips' (227). The former creates 'gibberish' while the latter creates a nasty accident. He wants to say that there is some form of 'mental scaffolding' which ensures that the hand stays on the cup and the verb BE stays in the wh-clause; to quote Russell: 'things that belong together stay together' (loc cit).

Thus the explanation Russell gives us for why verbs in some clauses (main clauses) can be inverted around their subjects and why others cannot is that the subordinate clause verbs 'belong' in the subordinate clause. Why he restricts his attentions to wh subordinate clauses is unclear; the fact that the subordinate clause in question is of the 'wh' sort is, of course, irrelevant. Russell's 'explanation' amounts to little more than asserting that one cannot invert verbs in some clauses because they cannot be inverted, but that we can invert verbs in



others because they can be. If a verb in a subordinate clause 'belongs there', so does a main clause verb 'belong' there, assuming that 'belonging' is to be explicated in any sort of coherent way (in terms of constituency, for instance).

What is objectionable about Russell's metatheoretical position is the fact that he does not give us any indication of what exactly 'belonging' amounts to, and with such vague and undefined notions, one cannot come up with falsifiable hypotheses. One must tackle hierarchicality, as a fairly clearly defined construct in linguistic theory, in a much more coherent way if one is to counter the claims made with it and about it in theoretical linguistics (or in its associated philosophical claims). My response to Russell's holism is therefore to say that if this is holistic explanation, it leaves much to be desired, and in the absence of a better articulated holism, we are as well to stick with modularism\*.

If Russell's holism constitutes no real challenge to the idea of interaction between ontologically distinct domains, there are serious coherent accounts of hierarchicality which have been developed, and which might be considered to threaten the sort of interactionism I propose. The work of Simon (1962, 1981, for example) springs to mind; it is clear that in Simon's

\*The remainder of Russell's comments, on the realist assumptions inherent in my version of interactionism, are unworrying. This is because, in replying (in his 1987 b) to the comments I make on realism in AL (in Carr 1987a) he mistakenly equates all versions of linguistic realism with Platonism, which I overtly reject both there and in 5.1 above.

case we are dealing with a means of shedding light on the nature and origin of hierarchicality rather than constructing a vague 'things that belong together stay together' holism. Simon's proposals are best not described as holistic, and do not necessarily build a case for holism against modularism, but they may seem to undermine interactionism nonetheless. If, as he suggests, hierarchicality is likely to turn out to be a property of the internal structure (and, presumably, internal representations) of any complex organism, then it is seen not to be solely a property of specifically linguistic objects, and the idea of a linguistic, as opposed to a non-linguistic, capacity is in jeopardy.

It is not surprising, therefore, that Simon's work has been used to tackle the philosophical underpinnings of the Chomskyan innateness hypothesis, notably in Sampson (1978). While I have little to add to the rationalist/empiricist debate, I should say that I do not think interactionism commits me to a version of the innateness hypothesis. Even if there is a good case for denying that humans are possessed of a specifically linguistic cognitive capacity, I am not claiming that the object of theoretical linguistic inquiry is of a mental (speaker-internal) sort, and the interaction I visualise is between speaker-internal states of affairs (innately endowed or otherwise) and specifically linguistic, speaker-external, objects such as sentences.

However, it remains to be seen whether such purely

linguistic constructs as sentences will remain a valuable tool in linguistic research; I imagine that they will, since it is difficult to imagine a coherent linguistics without them. Moore and Carling's (1987) attempt at explicating what they call the 'problem of correspondence', for example, suffers precisely because it does not take the construct 'sentence' seriously as a notion that may be interpreted realistically. In an attempt to say what the correspondence there may be between sentences as objects generated by the grammar and 'our everyday notion of sentence', they construct a sentoid/sentence distinction to distinguish between the former and the latter. But they give no idea of what our everyday notion of sentence is, and one cannot elevate this notion to the status of theoretical construct, as they do, simply because *it itself is in need of explication*. And this, of course, is precisely what the sentence/utterance distinction is designed to do (and will do if we take it seriously: cf the remarks on Burton-Roberts and Lyons in (ii) above).

I am relatively confident, therefore, that my linguistic objects in and of themselves will have to be recognised, and that a picture of the relationship between them and non-linguistic, sociolinguistic and psycholinguistic states of affairs will have to be formulated which recognises their existence. The principal threats I see to this sort of autonomism-plus-interaction come from work in AI on the validity of modularism, but it is interesting to note, as a closing comment, that work in AI does not collapse cognitive psychology,

theoretical linguistics and computational modelling of cognitive processing (as the surveys in Thompson 1983, Biggs 1987 and Wilks 1987 show). There are clearly distinct AI-type problems, cognitive psychological problems, and theoretical linguistic problems, and much of the argumentation between AI, AL and psycholinguistics researchers (as reported by Biggs and by Wilks in Modgil & Modgil 1987, for instance) centres around questions of the inter-relationship between these disciplines. I think that an awareness that these fields are concerned with distinct, but interacting, domains, in the way that I suggest they are, can only shed light on what each discipline can and cannot achieve. As Biggs (1987: 194) states, many of the problems one faces in understanding the relationships between these fields are metatheoretical; this is an observation I wholeheartedly agree with.

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