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CHAPTER 3

Is Linguistics a Branch of Psychology?

Stephen Laurence

1 Introduction

According to Noam Chomsky's well-known and influential account, linguistics is properly conceived of as a branch of cognitive psychology. Linguistics studies one aspect of the mind, namely our 'competence' or knowledge of the natural language we speak. This view is widely endorsed by linguists, but has encountered considerable and sustained resistance among philosophers. Though the issue has been much debated, it seems far from settled. It would certainly be interesting if it could be shown, for example, that the Chomskian approach was fundamentally in error (perhaps embodying some deep confusion), or that linguistics could not be thought of as a scientific discipline, or that some different conception of linguistics was manifestly superior. However, the kinds of considerations philosophers have raised do not in fact show anything of this sort. Nearly all of the arguments purporting to raise particular problems for linguistics generalize in fairly immediate ways to other sciences. But, whereas the arguments may initially seem plausible when directed particularly against linguistics, I think that it becomes clear when they are considered in the light of these other sciences that they are based on rather dubious general principles. Accordingly, my strategy will in large part be to draw analogies to other sciences, and see how the arguments directed against linguistics play out there.

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In my view, a broadly Chomskian account of linguistics is the default hypothesis, for reasons having to do with the explanatory power that this interpretation confers on linguistic theory (as I explain in Section 2). I briefly sketch out some alternatives to the Chomskian account in Section 3. In the remainder of the chapter, I examine two of the main arguments that have been directed against the Chomskian account to see whether they offer good grounds for abandoning this default hypothesis in favour of one of the rival views.[1] Section 4 is devoted to what I call the *Methodological Argument* and Section 5 is concerned with what I call the *Martian Argument*. Versions of both arguments have been put forward by a number of different authors. I argue that these arguments do not provide us with any good reason for abandoning the Chomskian account and that the continued resistance the Chomskian view faces is not justified.

2 The Chomskian Account of Linguistics

According to Chomsky, linguistics is a 'branch of cognitive psychology' (1972: 1). He writes, 'I would like to think of linguistics as that part of psychology that focuses its attention on one specific cognitive domain and one faculty of mind, the language faculty' (1980: 4); or again, that 'the theory of language is simply that part of human psychology that is concerned with one particular "mental organ," human language' (1975: 36).

This psychological interpretation of linguistics naturally breaks down into two components, corresponding to the dual focus of linguistics on Universal Grammar and on the grammars of particular languages. Universal Grammar is essentially a theory of the knowledge embedded in the language-acquisition mechanism. According to recent linguistics, this knowledge is embodied in the system of parameters. And language acquisition is basically a matter of setting the parameters of that language, these being 'set' by 'triggering' information in the corpus that the child is exposed to. For each general way that languages can differ from one another, there corresponds a parameter. Having set all the parameters, a child will have

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[1] Other arguments against the Chomskian view have been discussed in more detail elsewhere. See e.g. Chomsky (1975; 1980; 1986); J. A. Fodor (1981); Laurence (1993); Antony (this volume); Matthews (this volume); and references in Barber (introduction to this volume).

learnt all the syntactic principles governing the language. Theories of particular languages' grammars are theories of the knowledge which results from this process, embodied in the acquired set of principles determined by the parameter settings. Chomsky interchangeably describes one's having this knowledge base as one's having acquired a 'competence', or one's having 'internalized' the grammar of the language, or as one's knowing the grammar of the language or internally representing the grammar of the language. And according to Chomsky, this knowledge base plays a central and essential role in language processing.

The Chomskian account ties the interpretation of linguistic theory to our psychological mechanisms for language acquisition and processing. The account thereby allows linguistics to contribute to the explanations of these abilities and increases the explanatory power of linguistic theory. At the same time, it opens the possibility of bringing a large number of alternative sources of data to bear on linguistic theory. I think that this is the main reason why most linguists adopt a Chomskian account of linguistics. It gives linguistic properties a central role in the explanation of the two chief language-related explananda—language processing and language acquisition—and it is supported by and explains a wide range of empirical data, including data concerning language change, language breakdown, processing errors, and the course of language development.

It will be useful for what follows to have a bit more detailed sense of the explanatory power of the Chomskian account. On the Chomskian view, linguistic properties play a crucial role in normal language acquisition and processing. They play this role in virtue of being identified with features of representational structures essentially involved in the exercise of these capacities. Accordingly, linguistic properties will play a substantial role in our explanations of patterns in utterances and commonalities among languages through their role in the mechanisms underlying the exercise of these capacities. Similarly, linguistic properties, through their role in these mechanisms, will play a vital role in explaining a whole host of results of psycholinguistic experiments concerning reaction times, priming effects, cognitive illusions, and the like, as well many ordinary phenomena concerning speech errors, processing errors, and the like. Given the Chomskian account, linguistic theory plays an important role in explaining these various phenomena, and at the same time the phenomena can be seen to provide new sources of evidence for linguistic theory.

Furthermore, taking the population of adult native speakers of a given language as a sort of core group, we may view more peripheral members of the linguistic community as deviating in different systematic ways in their linguistic capacities and behaviour from the capacities and behaviour of the core members.[2] The explanatory power of the Chomskian account is further displayed in its ability to extend to various peripheral groups, including young children still in the process of language acquisition, aphasiacs, and various others whose mastery of the target language is imperfect. Linguistic theory is proving increasingly useful in categorizing and making sense of data from neurological studies, developmental studies, studies of the historical course of language change, and many other data.[3] The Chomskian account will explain the various systematic deviations in linguistic behaviour and capacities of these groups from 'normal' speakers in terms of systematic differences in the representational base which subserves the relevant capacities (or operations defined over these representations), and which on the Chomskian account is tied to the interpretation of linguistic theories and properties themselves. And again, in each case, while linguistic theory explains the phenomena in these domains, the domains provide new sources of evidence for the Chomskian account.[4]

Much of the research I have alluded to is still in its early stages, but the results are quite suggestive. A brief discussion of a couple of examples will help make the attractions of the Chomskian account a bit more concrete. Consider, for example, an experiment by Swinney, Ford, Bresnan, and Frauenfelder (1988; reported in J. D. Fodor 1991) providing evidence for the existence of traces. Their experiment, using the cross-modal prim-

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[2] If we replace the assumption that there is such a thing as a 'normal' speaker with the more plausible assumption that each speaker speaks her own idiolect, we derive further explanatory potential in describing each of the variable idiolects within the framework of a particular linguistic theory.

[3] It is really a question of *producing* these data. The data which linguistic theory, on a Chomskian interpretation, 'makes sense of' simply would not be available otherwise (see Chomsky 1992).

[4] And of course the explanatory power of the Chomskian account might be yet further increased through the study of priming effects, cognitive illusions, speech errors, processing errors, and the like, in peripheral groups.

ing paradigm, tested for the existence of traces by looking for priming effects of words semantically related to the moved noun phrase at the trace position. Sentences like (1) were presented orally, and semantically related lexical items and controls were presented visually on a computer screen at selected intervals (see Figure 3.1).

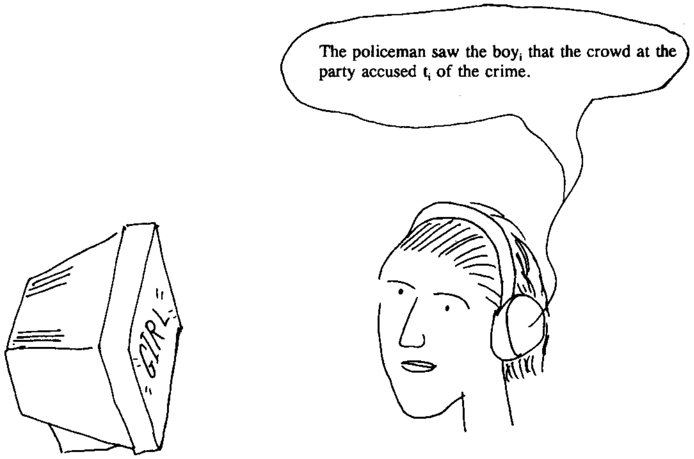

FIG. 3.1. Cross-Modal Priming Experiment

(1) The policeman saw the boy$_i$ that$_i$ the crowd at the
party accused t$_i$ of the crime.
    | | |
    1   2   3

So, as the subjects heard this sentence through their headphones, lexical items appeared on the computer screen at the times indicated by numerals. Subjects were given a lexical decision task in one experiment (that is, they were asked to decide whether the word on the screen was a word or not) and a naming task in another (that is, they were asked to read the word on the screen aloud). In both experiments Swinney *et al.* found a significant priming effect (that is, the reaction—deciding whether it is a word, or naming the lexical item—was faster) in positions 2 and 3 for lexical items semantically related to 'boy' (e.g. 'girl'), but not at position 1. At position 1, but not 2 or 3, lexical items semantically related to 'crowd' (e.g. 'group') showed a priming effect.

How does such an experiment support the Chomskian account? Since there is no significant priming effect for 'boy' at position 1, but there is at 2 and 3, we cannot simply explain the outcome in terms of a priming effect from the overt occurrence of 'boy', since this effect has evidently 'worn off' by position 1. We can make sense of the data, however, on the assumption that the internalized grammar is one that incorporates traces and, in particular, posits the existence of a trace of the NP 'the boy' as the object of the verb 'accused'. This trace will explain the renewed priming effect. Grammars that are in some sense extensionally equivalent to such grammars, but do not incorporate traces or posit this particular trace in this location, will face a prima facie difficulty explaining these data.[5] What is more, it is only on the Chomskian account that such data can be used to determine which linguistic theory is correct and it is only on the Chomskian account that linguistic theory will have the added explanatory power involved in explaining such data.

Turning to a much older piece of data, Fodor, Bever, and Garrett in a series of early experiments discovered that subjects, when asked to determine the temporal location of 'clicks' made during the oral presentation of sentences, tended to displace these clicks to major phrasal boundaries (see Fodor, Bever, and Garrett, 1974). Again, such data can be made sense of on the assumption that the internalized grammar is one that assigns phrasal boundaries, in a way consistent with the data from these experiments. And grammars that are in some sense extensionally equivalent to such grammars, but do not make comparable assignments of phrasal boundaries will face a prima facie difficulty explaining these data. And again, it is only on the Chomskian account that such data can be used to determine which linguistic theory is correct and it is only on the Chomskian account that linguistic theory will have the added explanatory power involved in explaining such data.

Much the same point can be made by considering data from cases involving more 'peripheral' members of the linguistic community. Yosef Grodzinsky (1990), analysing behavioural data concerning the phenomenon of 'agrammaticism', suggests an analysis of this phenomenon in terms

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[5] Of course a Chomskian account of linguistic theory can be correct even if current linguistic theory is not. But evidence for the psychological reality of a particular grammar is *ipso facto* evidence for the psychological reality of some grammar, and therefore, in the context of the argument here, evidence for the Chomskian account of linguistic theory.

of the Principles and Parameters theory. In particular, he suggests that the peculiar pattern of behaviour associated with this aphasia can be accounted for by supposing that patients suffering from it delete traces at S-structure, and assign theta roles using a default heuristic. If these data held up, and could be extended, they would offer strong support for grammars incorporating the basic features Grodzinsky claims are manipulated in this aphasia.

Parameters, in addition to their roles in explaining cross-linguistic variation and the fact of language acquisition, may plausibly also be invoked in the explanation of the *course* of language acquisition and development. Lightfoot (1991) discusses several cases of historical changes in languages, attributing them to the effects of changes of parameter settings. Regarding the course of language acquisition, one much-discussed hypothesis (due to Hyams 1986) attributes early subjectless sentences in English to the fact that the default setting of a parameter is to allow a 'phonologically null' (i.e. unpronounced) element called 'pro'. The details are not essential for my purposes here. Hyams's idea, which she uses to explain various data concerning language acquisition, is that whereas adult English does not allow pro, children learning English use pro for a time, until the parameter is reset (see Rizzi 1992 for discussion). The crucial point here is that, as above, these various phenomena all get *explained* largely in terms of linguistic properties through their roles in language-processing and acquisition mechanisms. These few examples illustrate the explanatory power of the Chomskian account. In each case, on the Chomskian account and given a particular linguistic theory, we can explain the data—importantly, all extensionally equivalent grammars will not be equally illuminating of the data.[6] And in each case, we can only bring the data to bear on our choice of grammar under the assumption that the Chomskian account is correct.

Much of this research (like that in much of psychology in general) is still in the early stages, and undoubtedly further refinements of research methods, and more powerful future methods, will necessitate theoretical changes. But this is only natural and to be expected. The Chomskian account of the nature of linguistics is not tied to any particular linguistic theory, and certainly does not require the truth of current linguistic theory in any detail. In fact, quite to the contrary, changes in current linguistic theory occasioned by present

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[6] This will be important in connection with some of the alternative accounts of linguistics discussed below.

and future results of this sort will rather serve to support the Chomskian picture. Thus, the Chomskian interpretation both gives linguistic theory vast explanatory power and gives us a very large base of potential sources of confirmation (or disconfirmation) for linguistic theories. In my view such considerations as these not only make the Chomskian account the default hypothesis, but also provide considerable grounds for supposing that it is actually the right account.

3 A Look at the Alternatives

Before getting into the objections that have been raised against the Chomskian account, it will be useful to have a brief look at some of the alternatives to that view that have been proposed. Often enough much the same argument is presented by different authors, but the argument takes on a slightly different shape depending on the background perspective of the author in question. And, of course, ultimately we will need to evaluate the strength of the Chomskian view relative to its rivals.

One of the earliest philosophical alternatives to the Chomskian interpretation of linguistics was suggested by Stephen Stich in a series of papers in the early 1970s. According to Stich, linguistic theory is best thought of as a theory of our capacity for linguistic intuition. An integral part of the methodology of linguistic theory involves the production of linguistic intuitions—of grammaticality, synonymy, ambiguity, and the like—these constituting the bulk of the data informing our linguistic theories. Stich argued (largely on the grounds that this interpretation was the most charitable one, giving linguistic theory the best chance of being *true*—see below) that linguistic theory should be thought of as a theory of this psychological capacity of ours to generate such linguistic intuitions (1972: 142–5; 1975: 94–5).[7]

A rather different interpretation has been offered by Jerrold Katz in a series of publications (see e.g. Katz 1977; 1981; 1985*b*; Katz and Postal 1991; also Langedeon and Postal 1985). The view which emerges from these

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[7] Note, however, that Stich treats this capacity as analogous to a perceptual capacity. It delivers knowledge in the form of linguistic intuitions when presented with given sentences and is equally well described by a host of extensionally equivalent grammars, none being internally represented (see esp. Stich 1971: 494–6).

publications, which Katz has called 'Realism' or 'Platonism' (I shall use the latter, less ambiguous, term), is that linguistic theory is a theory of a particular sort of abstract object, languages. In this, linguistics is analogous to mathematics and other formal disciplines.

Jerry Fodor in an interesting and influential article 'Some Notes on What Linguistics is About' (1981) lumps together the positions of Katz and Stich under the heading 'the Wrong View', which he characterizes as a sort of instrumentalism. It may be that from a methodological point of view Stich and Katz should be thought of as instrumentalists,[8] but from a metaphysical point of view neither seems to fit the bill. Stich and Katz both believe that there is an independent object of linguistic enquiry, and differ about the character of this domain.

Interestingly though, two more recent critics of 'the Right View' (Fodor's name for the Chomskian position) seem far closer to the description Fodor gives of the Wrong View. In 'Language and Languages' David Lewis says that he can find no way to 'make objective sense of the assertion that a grammar G is used by a population P whereas another grammar G′ which generates the same language as G, is not' (1983: 177). Scott Soames (1984) does not explicitly say that there is no objective basis for linguistic claims, but does say that what makes a grammar correct is simply the fact that it provides (one of) the simplest overall account(s) of a specific set of data.

Finally, there is the position of Michael Devitt and Kim Sterelny (1987; 1989), put forward in their popular philosophy-of-language textbook *Language and Reality* and in a follow up paper 'Linguistics: What's Wrong with "the Right View"'. Taking up the terms of the debate that Fodor sets out, Devitt and Sterelny claim to be offering an alternative to both the Right View and the Wrong View. According to Devitt and Sterelny, linguistics is a 'theory of symbols' as opposed to a theory of competence. However, they do not say much about what symbols have their linguistic properties in virtue of, stating merely that such properties are most likely complexes of psychological and environmental factors and drawing an analogy with the properties of being a pawn, or being worth a dollar. Thus it is not clear what

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[8] Fodor's charges are best seen as challenging methodological assumptions of anti-Chomskians, in my view. As such they will be relevant to what I take to be the two main arguments to be offered against the Chomskian, the Methodological Argument and the Martian Argument (see below).

their view really amounts to or even if they are in fact offering *an alternative* to the Chomskian view, as they claim (see below).

From such various positions as these, over the years, there have been quite a number of criticisms of the Chomskian view. It is not possible, nor would it be particularly beneficial, to consider all of them here. Instead, I focus on two arguments which recur in one form or another throughout the literature, which I call, respectively, the 'Methodological Argument' and the 'Martian Argument'. Both have broader methodological implications, and my main interest will be in these broader implications.

4 The Methodological Argument

The Methodological Argument originates with Stich. As Stich presents it, the basic idea is that, given the methods of linguistic theory, we should not adopt a Chomskian interpretation of linguistics, because on such an interpretation, linguistics is most likely false (Stich 1972; 1975). The burden of Stich's argument, then, is establishing that current linguistic methods are rather unlikely to uncover psychologically real grammars (or UG). It is worth noting at the outset that the argument, so formulated, looks to be inferring a metaphysical conclusion (that linguistic theory is *not about* language competence) from epistemological premisses (that on the basis of current linguistic methods, we could not come to *know* or *determine* or be *justified* in believing facts about what is responsible for our capacity to process language).

Refining Stich's principle, Devitt and Sterelny (1989) state a general methodological principle for determining the subject matter of linguistics:

> *Devitt and Sterelny's Methodological Principle*
>
> [T]he best reason that we can expect to find for thinking that linguistics is about *x* rather than *y* is that the considerations and evidence that have guided the construction of linguistic theory justify our thinking that the theory is *true* about *x* but not *y*. (1989: 498–9)

Though Devitt and Sterelny do not say so, we may assume that this principle is intended to be an instance of a more general methodological principle for determining the subject matter of a science, since the principle would not be worth much if it applied only to linguistics.

Why, then, is linguistics most likely false if it is interpreted as a theory of competence? For Devitt and Sterelny, as for Stich, the principal reason stems from the fact that there exist many alternative grammars which are extensionally equivalent to any given grammar: the same set of acceptable strings can be generated by many different formal systems. This is unquestionably true. However, Devitt and Sterelny (following Stich) assert that this provides us with an argument that the grammar, G, of current linguistic theory has no more claim to psychological reality than any of a whole host of extensionally equivalent grammars. In particular they claim that 'the evidence and considerations that have guided transformational grammarians provide insufficient reason for thinking that it *is* G [rather than some extensionally equivalent grammar, *G′*]', that is psychologically real (Devitt and Sterelny, 1989: 504).[9]

Devitt and Sterelny offer four points in support of their claim that the evidence and considerations which have guided linguists do not justify us in taking G to be psychologically real. The four are closely related to claims made by Stich.

The first point, made by both Stich and Devitt and Sterelny, concerns the fact that the primary evidence offered in favour of a particular grammar such as G is evidence concerning linguistic intuitions. Such evidence, they argue, provides at best indirect evidence about what is or is not psychologically real. As Devitt and Sterelny say, 'there is no basis in this evidence that the speaker has internalized G rather than the meaning-equivalent *G″*' (1989: 505).[10]

Second, following Stich, Devitt and Sterelny argue that considerations of simplicity and elegance of G over *G′* do not provide a basis for taking G to be psychologically real. As Devitt and Sterelny put it, such considerations of simplicity and elegance are 'psychologically irrelevant' (1989: 507). There is no reason to suppose that the brain uses the formally most simple and ele-

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[9] I should note, however, that Devitt and Sterelny hold that '*some* aspects of G are psychologically real, in particular those aspects that go into determining meaning' (1989: 504). Stich, in the papers under discussion, is rather sceptical about the coherence of the notion of mental representation, and thus of the availability of a robust sense of 'psychological reality' for grammars.

[10] The idea of a 'meaning-equivalent' grammar is never fully explained by Devitt and Sterelny. The rough idea is that two grammars are meaning-equivalent if the differences between them do not affect the meanings assigned to utterances.

gant grammar. In fact, there is reason to suppose that from an evolutionary or a neurobiological point of view, the formally most simple and elegant grammar is *less likely* to be the one actually used by the speaker.[11] Therefore, we are not justified in believing G rather than G′ to be psychologically real on grounds of simplicity.

The third point concerns evidence drawn from research and experiments on language acquisition. Study of UG offers a constraint on particular grammars because grammars might be rejected by theorists if they serve to complicate UG unecessarily. The simplicity and elegance of particular grammars must be balanced against the simplicity and elegance of the general theory, UG. However, as Devitt and Sterelny point out, if the arguments above are sound they will carry over to the case of UG. If the considerations of simplicity and elegance are 'psychologically irrelevant' (1989: 507), then given that there are indefinitely many extensionally equivalent grammars, there will likewise be indefinitely many extensionally equivalent theories of UG and, so the argument goes, no way to choose between them.

Stich develops the point in a slightly different way. He imagines a logical space defined by the class of all extensionally equivalent grammars for all natural languages, and notes that there will be many ways to build theories of UG which develop different commonalities among these. The initial choices we make in theorizing about grammars for particular grammars and the initial commonalities we emphasize start us off in some particular corner of this logical space. But we are there, Stich argues, only by reason of historical accident. We might have started elsewhere, and arrived at different particular grammars and a different account of UG. Nothing but historical accident sets our theories apart.

Finally, Devitt and Sterelny consider evidence from studies of language processing: 'evidence about reaction times, the types of errors we make, the relative ease of understanding sentences, the order in which sentences are learned, and so on' (1989: 508). They suggest that in principle such evidence could settle a claim of psychological reality, but that in practice the evidence has failed to support currently popular linguistic theories. As they put it:

> We need to show that the deep structure, intermediate structures, surface structure, and the rules of G, have all been internalized. It is generally agreed that there

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[11] Devitt and Sterelny credit Stephen Jay Gould (1983), David Lewis, and Robert Berwick and Amy Weinberg (1984).

> is little evidence of this. There are problems even finding the transformationalist's levels and rules at all, let alone the particular details specified by G. (1989: 509)

Stich, writing earlier, notes that such evidence, though potentially relevant, is not presented by linguists. And at one point he suggests that evidence concerning brain structure is available and renders evidence from linguistic intuitions for the psychological reality of grammars otiose (1975: 99–100).

In sum, Stich and Devitt and Sterelny argue that the sorts of evidence and considerations in these four categories do not justify us in taking G, essentially current linguistic theory, to be psychologically real. But then, if we adopt the Chomskian view, we are not justified in believing that current linguistic theory is likely to be *true*. By Stich's general methodological principle, then, linguistics is not about competence as the Chomskian would have it. And the same is true for Devitt and Sterelny's general Methodological Principle, provided there is an alternative interpretation on which linguistic theory *is* likely to be true (which Devitt and Sterelny claim to provide).

What should we make of this argument? In my view, the trouble with the Methodological Argument goes right to its core: the methodological principles of Stich and Devitt and Sterelny are false. I think that even if we grant that much of the evidence that linguists appeal to is in a sense 'indirect', that simplicity and elegance are not guides to psychological reality, and finally that what direct evidence there is has tended not to support current theory in detail, this does not give us reason enough to suppose that linguistics is not a theory of competence.[12] This becomes clear when we consider theory construction in psychology, for much the same case that has been made against attributing psychological reality to linguistic theory can be made, for example, against attributing psychological reality to the constructs of *psycholinguistics*.

Contemporary psycholinguistic theory generally divides the task of language comprehension into a number of component processes. One of these components is the process of recovering the syntactic structure of a given sentence. And one much-discussed hypothesis concerning this process is that it is governed by the principle of Minimal Attachment. The hypoth-

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[12] As should be clear from the discussion in sect. 1 above, however, it is not at all clear to me that we should accept any of these points either.

esis has been advocated by Lyn Frazier and others. Frazier characterizes Minimal Attachment as follows:

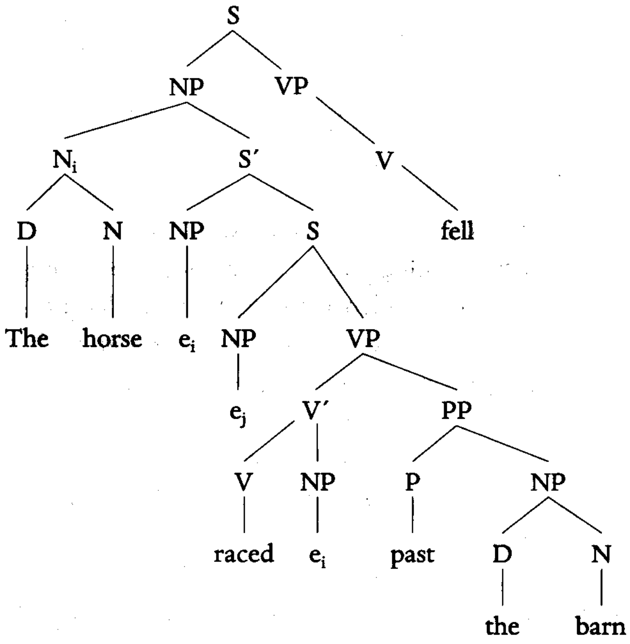
> *Minimal Attachment*
>
> Attach each new item into the current phrase marker postulating only as many syntactic phrase nodes as is required by the grammar. (1987: 520)

A significant component of the evidence adduced in favour of this principle and the model of language processing incorporating it comes from linguistic intuitions. Consider, for example, the classic sentence (2):

(2) The horse raced past the barn fell.

which is very difficult to process. The structure of (2) is something like (3):[13]

(3)



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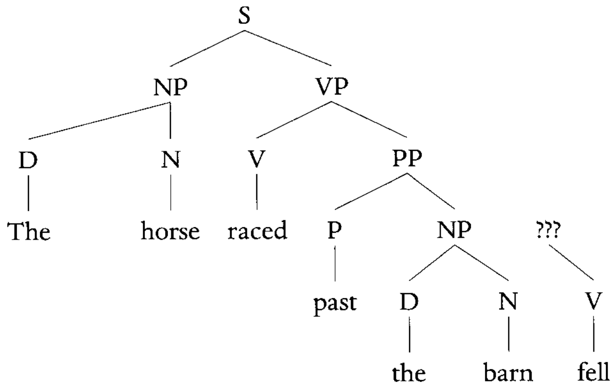
[13] Arguments for Minimal Attachment clearly interact with particular accounts of syntactic structure, and alternative accounts of structure may well affect the arguments for Minimal Attachment (as is briefly noted below). However, since I am not concerned to argue for or against Minimal Attachment, the structure in the text will suffice.

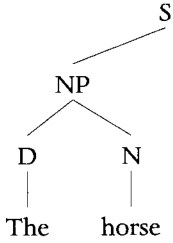
The sentence says that a certain horse, namely the one that was raced past the barn, happened to fall. Though the sentence is grammatical on this reading, this is not how the sentence is typically read. The dominant reading of the sentence assigns it a structure on which the sentence is *ungrammatical*. This structure is something like (4):

(4)

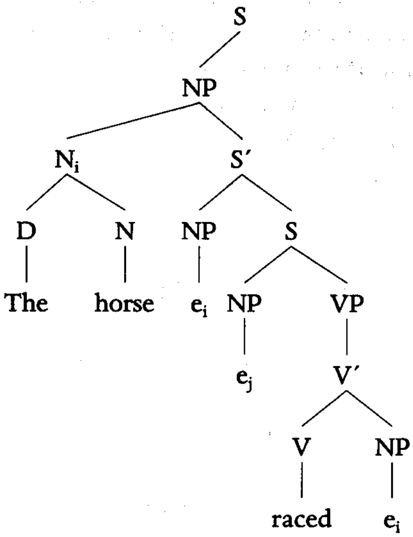


where the initial segment 'the horse raced past the barn' means that a certain horse went racing past a certain barn. The sentence provides evidence for a model of language processing incorporating the principle of Minimal Attachment since attaching 'raced' as in (4) rather than as in (3) is less costly in terms of the number of nodes, and thus is the attachment predicted by Minimal Attachment. The important point to notice is that when 'raced' is added to the tree, more new nodes need to be added in (3) than in (4). To see this, notice that we start with (5), and when we add 'raced' in building (3) we get (6), whereas when we add 'raced' in building (4) we get (7). Since (7) requires that fewer new nodes be added to (5) than (6) requires (to say the least), it is the reading predicted by Minimal Attachment.

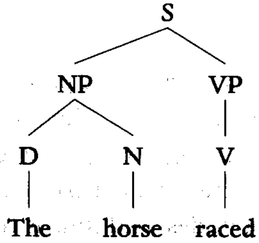
(5)



(6)



(7)



Now although a model of language processing incorporating the principle of Minimal Attachment explains these intuitive data, there are many alternative models of language processing which also explain the data. Indeed, given any computational model of parsing, we might generate indefinitely many alternative models, simply by adding in a computationally superfluous step or two. Provided the steps we add have an equal effect on minimal assignments and non-minimal assignments, the alternative models so generated will all be compatible with these same data. But given the existence of indefinitely many alternative possible parsing mechanisms, we might argue, by analogy with the Methodological Argument, that the intuitive evidence adduced does not justify any one of these theories over the others. Furthermore, if formal simplicity and elegance are 'psychologically irrelevant', they cannot help us narrow the field.

Of course, there is also more direct evidence in the form of reaction time experiments and the like (see e.g. Frazier 1987; Rayner, Carlson, and Frazier 1983; Ferreira and Clifton 1986). And it is true that psychologists in general stay closer to chronometric data than purely intuitive data. However, continuing with the analogy to the Methodological Argument, we might argue further that though reaction-time experiments and the like might in principle decide between the competing alternative parsing models, most of the relevant experiments have not been done and the evidence we do have is at best equivocal—certainly no model of language processing is supported in detail. There are many theories in the field and they are undergoing a constant process of revision. And certainly, Minimal Attachment has significant difficulties facing it. To cite just one sort of problem for the theory, Edward Gibson (1991) notes that the interpretation of experiments favouring Minimal Attachment may rely on controversial assumptions about phrase structure that may not be independently justified. Indeed, it seems that in general most of the difficulties Devitt and Sterelny allege in the case of linguistics are inherited by a linguistically based psycholinguistics, simply in virtue of the fact that such theories in psycholinguistics will be informed and inspired by the theories in linguistics.

So what are we to make of all this? If we are not justified in accepting as psychologically real theories which tend not to be supported in detail by 'direct' psychological evidence, then perhaps we should conclude that we are not, by and large, justified in accepting current psycholinguistic theories. Perhaps. But we most definitely should *not* conclude that by and large current psycholinguistic theories *are not theories of psychological capacities*. But this, of course, is *exactly* the conclusion the analogue of the Methodological Argument would urge on us. The argument in psychology would be just as it was in linguistics: since current theories are not justified by these sorts of evidence, we should not, given their general methodological principle, consider these theories to be *about the psychological domain*, since so construing them makes them probably false. Clearly something has gone wrong. Certainly, constructing a theory of the mind should not be *easy*, but neither should it be conceptually impossible. What has gone wrong, I think, is that the Methodological Argument's central principle, the Methodological Principle, is false. This principle should be rejected.

Returning to linguistics, it seems clear that the problem is that the Methodological Principle essentially conflates the issue of the likely truth of current theory with the issue of the subject matter of that theory. Of course much of current theory—in linguistics as in almost any science—is likely to be false. A cursory glance at the history of science will demonstrate that. And of course current theories in newly developing sciences such as linguistics are especially vulnerable. But equally clearly, *this should not deprive these sciences of their natural subject matters*. It is perhaps true that crucial evidence has not yet been brought to bear. And it is further true that current theory is likely to be lacking in various ways and inaccurate in detail. But the reason why crucial evidence has not been brought to bear is that science is not easy. Theories in linguistics are still developing in what look to be very productive ways, and neurological and psycholinguistic evidence, though growing, is constrained by limitations of experimental paradigms, testing methods—and crucially, the paucity of well-defined theoretical alternatives to test among.[14] Neurological and psycholinguistic evidence is not independent of linguistic theory. Indeed, linguistic theory can be seen as directly responsible for such evidence, since we can only make sense of (and therefore only have access to) the data because of the theory. And again, though current linguistic theory is likely to be inaccurate in many ways, what choice do we have in seeking out psychologically real grammars but to go with those judged best using whatever data we have available at any given time? And certainly, theorists should adopt as their working hypothesis the simplest, most elegant one available. What else are they to begin with—the least simple hypothesis? Similarly, the fact that in constructing psycholinguistic models of processing and acquisition we naturally (and unavoidably) make many assumptions about our general cognitive architecture, other cognitive mechanisms, the brain, computation, and mental representation, among other things, makes our models only as good as those assumptions. These sorts of considerations apply to *any* scientific theory, though. Linguistics, being a relatively new science, is just more vulnerable. The fact that a theory of competence is tied to further theory or that, given the current state of evidence, it is not likely to be accurate in detail should not, however, make it any less a theory of competence.[15]

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[14] This last point is one that Chomsky has frequently emphasized.

[15] Robert Cummins, illustrating a different point, discusses a possible explanation for molecular bonding, where atoms are held together in molecules by 'hook-and-eyes' (1983:

I have not offered any alternative interpretation on which psycholinguistic theory is likely to be true, as Devitt and Sterelny's Methodological Principle requires. So before turning to the Martian Argument, I should make a few brief points about this.

First, it is not difficult to come up with alternative accounts of what psycholinguistics might be about by looking for them closer to the actual data that have been used thus far in psycholinguistic theorizing. One possibility is that psycholinguistics is simply an account of the intuitive data which it has attempted to account for, or of some posited psychological faculty responsible for such data. Alternatively, we might take it to be about some realm of abstracta isomorphic to the theory. Arguably, these alternatives are more likely to be true, given the considerations that have largely guided psycholinguistic theorizing. However, they should not make what otherwise *seems* to be a theory directed at explaining language processing fail to be about this subject matter at all.

Second, we should also note that Devitt and Sterelny do not really have an alternative conception of linguistics to offer. They create the appearance of offering an alternative conception of linguistics by drawing a contrast between 'symbols' and 'competence'. As they note in a different context, however, the natural Chomskian reply is that 'the Right View's failure to mention symbols is excusable because the theory of symbols is completely derivative from the theory of competence' (1989: 521). This may not be Chomsky's own view, but someone with broadly Chomskian sympathies might well argue as follows:

> In a sense it is true that linguistics *is* about symbols. However, the important properties of these symbols—the properties in virtue of which symbols have their linguistic properties—are properties pertaining to our linguistic competence, and perhaps aspects of how these symbols are processed in language comprehension and production. So it is clearer to say that linguistics is about competence, since it is only because native speakers internally represent the grammar of their language that symbols have the linguistic properties they do. Thus, although linguistics could be said to be about symbols, it is, in the first instance, about

8–9). This theory, by current lights, seems to be a false theory of molecular bonding: we do not think molecules are held together in this fashion. But it does seem to be *a theory of molecular bonding*, none the less. The Methodological Principle seems to rule out the possibility of such false, or poorly justified, accounts as even being *about* their purported subject matter.

competence. The linguistic properties of symbols are completely derivative from properties concerning competence.

The important issue is not over whether linguistics is about symbols, but rather over the nature of the facts which determine the linguistic properties of symbols. Here the Chomskian has a more or less concrete suggestion. Devitt and Sterelny say only that these properties are clearly not intrinsic properties of the symbols, but rather involve some combination of 'psychological facts, and facts about the natural environment' (1989: 516–17). However, this does not even serve to *distinguish* their position from the Chomskian's, who also holds that linguistic properties are not intrinsic properties of symbols, and that they involve some combination of psychological facts and environmental facts. Thus the apparent stark contrast between their view of linguistics as 'the theory of symbols' and the Chomskian's, according to which linguistics is 'the theory of competence', amounts to no more than an issue about which properties of the environment, and local psychological agents, symbols inherit their linguistic properties from. And Devitt and Sterelny offer no definite suggestion concerning this issue (see 1989: 517). The only suggestion that they make that may serve to distinguish their position from the Chomskian one consists in a somewhat vague analogy to properties in other 'social sciences':

> Like all social sciences, [linguistics] seems to be immediately dependent on psychological facts and facts about the natural environment. The nature of this sort of dependency is complex and hard to describe. . . . Consider some examples of this sort of dependency. What makes a physical object a pawn or a dollar? What makes a physical event a vote or unlawful? Nothing intrinsic to the objects and events in question; rather, it is the psychological states, within certain environments, of people involved with such objects and events. Exactly what states, what involvement, and what environment, is hard to say. (1989: 517)

One might imagine that what Devitt and Sterelny have in mind here is some complex of propositional attitudes concerning the purposes or intentions of inventors or users of objects (such as chess pieces) or which otherwise establish the legitimacy of certain practices (voting, law). The idea would then be that linguistic objects, properties, relations, and so on can be treated similarly.

It is not clear that there is any definite suggestion here at all. None the less, to the extent that there is a discernible position, it seems that the view faces serious difficulties. I will mention only one problem here. It stems from the fact that linguistic properties are hierarchically structured and do not typically have direct physical realizations—linguistic properties do not typically attach to physical properties directly, but rather do so only by way of further layers of linguistic properties. So, for example, morphosyntactic properties attach to phonological strings as opposed to mere sequences of sound. Even the sound properties in language are abstract, not directly 'there' in the physical realization of language. Similarly, to the extent that there is 'movement' involved in the analysis of sentence structure (or multiple layers of structure), these features of sentences will not be present in the physical realization of language. These 'layers' of linguistic properties pose a difficulty for Devitt and Sterelny because they seem to require layers of propositional-attitude complexes of a very implausible sort: for example, attitude complexes to establish the existence of the phonological properties of a string, which could then figure in attitude complexes to establish morphological properties of a string, which could then figure in . . . and so on. To get an alternative along the general lines alluded to by Devitt and Sterelny, it seems that a very complex (and as yet wholly unspecified) account would seem to be necessary. And we have no reason to believe that intricate complexes of attitudes of this general sort exist.[16]

Finally, it is perhaps worth noting that, given their Methodological Principle, linguistic theory for Devitt and Sterelny would probably be a theory of our 'folk' theory of linguistics, not a theory of symbols at all. In order to explain how linguistic intuitions could count as evidence for linguistics, Devitt and Sterelny draw an analogy with our physical intuitions and biological intuitions and suggest that our linguistic intuitions might be generated by a 'folk linguistics', just as our physical intuitions and biological intuitions

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[16] At the same time, we should bear in mind that regardless of what one says about the nature of linguistic properties, an account of language processing is still needed, and any remotely plausible model is going to be built around representations of the sort articulated by working linguists. The result is that whether we call the knowledge base involved in language processing 'the language', as Chomskians would, or whether we think of language as some sort of public phenomenon, as Devitt and Sterelny suggest, we are going to be committed to the existence of the knowledge base that Chomskians take linguistic theory to be about. Given this, though, there is no need for the complex attitude-based alternative Devitt and Sterelny allude to.

are perhaps guided by folk theories of physics and biology. In particular, they say:

> Just as physical intuitions, biological intuitions, and economic intuitions can be produced by central-processor responses to the appropriate phenomena, so also can linguistic intuitions. These linguistic phenomena are not to be discovered by looking inward at our own competence but by looking outward at the social role that symbols play in our lives. When linguists do this now, they do not start from scratch. People have been thinking about these matters for millennia. The result of this central-processor activity is folk, or otherwise primitive, theory or opinion: the linguistic wisdom of the ages. The wisdom will be a good, albeit fallible and incomplete, guide to the nature of linguistic symbols. (1989: 522)

But if linguistic intuitions are simply a reflection of folk linguistics, why should they count as evidence for linguistic reality? When we are looking for evidence regarding the constitution of the physical or biological world we do not consult folk theories, we investigate the physical or biological domain directly. Furthermore, given the track record of folk theories, we can probably conclude that folk linguistics, if it exists, is a false theory. Investigations of folk physics have revealed that our naive (folk physics) intuitions about a wide range of physical phenomena, and their apparent theoretical basis, turn out to be radically mistaken (see e.g. McCloskey 1983). Indeed, the 'evidence' of intuition about physical reality seems far more a reflection of our folk theory than of physical reality. It is not at all clear, then, why in the case of language the evidence of folk linguistics should justify our belief in any theory constructed on its basis regarding linguistic reality. Thus, linguistics seems unlikely to be true construed as a theory of linguistic reality if the predominant source of evidence for the theory really is, as Devitt and Sterelny suggest, linguistic intuition, and such intuitions derive from our folk theory of linguistics. However, as Stich (1972) suggested in much the same context, if we take grammars to be part of the story about how our capacity for making folk-linguistic judgements works, it seems we are far more likely to have the theory come out true—after all, the evidence from intuitions is, by hypothesis, much more directly related to this capacity than it is to 'linguistic reality'. But, then, according to Devitt's and Sterelny's own Methodological Principle, we should interpret linguistics to be about *x* rather than *y* if 'the considerations and evidence that have guided the construction of linguistic theory justify our thinking that the theory is *true* about *x* but not *y*' (1989: 498–9). Since they claim that the predominant type of evidence for linguistics comes in the form of linguistic intuitions, and these considerations are more likely to justify our thinking that linguistics is a true theory of our capacity for folk linguistics than a true theory of 'linguistic reality', it seems that Devitt and Sterelny's own Methodological Principle suggests that linguistic theory should be taken to be a theory *about our folk theory of linguistics, not some independent 'linguistic reality'*.

5 The Martian Argument

We may now turn to what I call the Martian Argument. The argument is very simple. Basically, the argument is just that since different speakers of English, for example, *could* have different grammars, the linguistic properties of English cannot be determined by the nature of the internalized grammar. Devitt and Sterelny put it as follows:

> According to the transformationalists, English competence consists in internalizing a grammar. They go further: all English speakers have internalized near enough the one grammar; competence has a uniform structure across the linguistic community. Even if this is so, it is not necessarily so. Many other grammars could agree on the meaning-relevant structures they assign to the sentences of English. Suppose that Martians became competent in English by internalizing one of these other grammars. The theory of Martian competence would have to be different from the theory of ours. Yet the theory of symbols would be the same, for it would still be English that they spoke. Returning to Earth, it would not matter a jot to the theory of symbols if competence among actual English speakers was entirely idiosyncratic. (1989: 514)

Versions of this argument can also be found in, among other places, Soames (1984), Katz (1977), and Lewis (1983).

I must say that I find the prevalence of this argument quite puzzling. As far as I can see, the argument owes whatever plausibility it has to the fact that linguistics is not yet a well established science. The argument itself merely begs the question against the Chomskian account. Thus, the Chomskian might respond that the language that the Martians speak, despite sounding an awful lot like English, *is not English*. It is after all an open empirical question what constitutes a language, to be settled in part by empirical research and in part by general theoretical considerations. Presumably the general theoretical considerations tie the nature of a kind like 'natural language' or 'English' to various explanatory goals. We can see the Chomskian to be taking the goal of explaining the fact that humans acquire and process natural language as among the central explanatory goals of linguistics. Whatever properties languages have which account for our ability to acquire and use them will thus characterize the nature of languages and linguistic properties. But, according to Chomskians, what is really essential about language, what accounts for our ability to acquire and use language, is something about *us*—namely, the psychological capacities and representational resources we have underlying these abilities.

Certainly the Chomskian must grant that it is logically possible, perhaps nomologically possible (and, for all we know, even actual), that there are beings with a different competence that otherwise resemble English-speakers as much as you like. But this is just as the philosopher of chemistry must grant that it is logically possible, perhaps nomologically possible (and, for all we know, even actual), that there exists a substance which is not H_2O, but otherwise resembles water (in appearance and behaviour) as much as you like. The logical possibility of such a substance, however, should not require the philosopher of chemistry to hold that water is not (identical to) H_2O.[17]

It is logically possible that there exists a substance that resembles water as much as you like, just as the language the Martian speaks resembles English. But in both cases it is an open theoretical question whether the nature of the kind in question must take this logical possibility into account. Similarly, it is logically possible that there be a substance that obeys all the same laws that water obeys (apart from those which presuppose that water $=H_2O$) just as it is logically possible that there be something that obeys all the laws governing, for example, a sentence of English (apart from those which presuppose that the language is determined by competence). But again it is an open theoretical question whether the nature of the kind in question must take this logical possibility into account.

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[17] Much the same argument could be made for other natural kinds, including those whose nature is broadly functional—such as species kinds, for example—where the crucial functional properties turn out to be non-superficial ones.

Given any substantive independent account of the nature of a kind, it is not hard to come up with a logically possible 'counter-example' of the sort the Martian Argument offers. Ned Block (1990) describes a logically possible being that encodes plausible continuations to all possible English conversations, and in 'conversing' with you, chooses a plausible continuation at random, given as input the initial segment of the conversation to that point. Such a being is likely to be a 'counter-example' to any reasonable psychologically based account of the nature of linguistic kinds (including, for example, Devitt and Sterelny's account).[18] This example is a more sophisticated version of the age-old example of parrots which mimic English. Is the parrot, mimicking an English-speaker—or the valley echoing your voice, or the tape recorder replaying your voice, or the computerized telephone operator or machine at the supermarket checkout counter that 'tells' you the prices of your groceries—are these things speaking English? Is the system that Block imagines?

Presumably these sorts of cases are not enough to rule out every substantive account of linguistic properties. What, then, should we say about them? In the case of the valley, or the parrot, or the tape recorder I assume that we want to say something like: 'We should rule these cases out because they do not involve full command of the language.' As Chomsky says, they lack the 'creative aspect of language use': they do not have the capacity for potentially infinite use of language in an innovative way that is free from stimulus control, and yet coherent and appropriate to context. But why should this be relevant? Essentially, we are appealing to theoretical considerations about what aspects of speech are theoretically important. We are deciding *on theoretical grounds* that despite the fact that these things produce 'utterances' that seem to be English sentences, they are not in fact speakers of English. Some of the other cases introduce further complications.[19]

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[18] As noted above, it is not clear that Devitt and Sterelny actually offer an alternative substantive account, despite their claims to the contrary. The sketchy remarks they do make suggest that they perhaps have in mind an account based in a set of beliefs, intentions, and other attitudes, in which case the Block example would presumably be a 'counter-example' to their view inasmuch as the Martian case is a 'counter-example' to Chomsky's.

[19] In the Block case we may actually have a system which satisfies Chomsky's criteria for having the creative aspect of language use. It will regularly produce novel 'utterances' (and has a boundless capacity for doing so), they will be appropriate to context and

The main point I want to make here, though, is that we must use theoretical considerations of some sort to rule out these potential 'counter-examples'.[20] Why should systems that lack the creative aspect of language use not be counted speakers of natural languages if they produce 'utterances' phenomenologically indistinguishable from utterances of mine? Why should beings of the sort Block imagines not be counted as speakers of natural languages? Broadly theoretical considerations must be brought to bear. But if such considerations can be brought to bear in these cases they can in principle be brought to bear against the imagined Martians as well. And in that case as in these we might decide that the imagined beings just do not speak English rather than reject the account of the nature of linguistic properties which such creatures are allegedly a counter-example to.[21]

Given that it is a theoretical issue, the question is what sorts of theoretical consideration should be brought to bear? Arguably, exactly the sorts of consideration that have been offered in favour of the Chomskian view: considerations pertaining to the explanatory power of the theory. Given that the central explanatory roles of linguistic kinds are in explaining our ability to use language (language processing) and our ability to acquire the ability to speak a language (language acquisition), the sorts of possible evidence discussed in connection with the Chomskian view above seem directly relevant to the evaluation of which grammar is the correct theory of a given language. Taking into account data pertaining to language processing, change, breakdown, and acquisition provides us with a rich and diverse source of potentially confirming or refuting evidence for linguistic theories. And such data, being directly tied in with the central explanatory roles of linguistic kinds, seem directly relevant to the evaluation of linguistic theories.

In Section 2 above I tried to illustrate the explanatory power of linguistic theory on the Chomskian account, suggesting that there was actually a fair

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coherent, and they seem as free from stimulus control as our own utterances are (since in neither case is a particular response determined by the stimulus alone).

[20] Of course, in any of these cases we might accept that the system *really is* an English-speaker. Block's being might be taken to speak English. The parrot or the tape recorder or the valley might be taken to speak English as well. See below for further discussion.

[21] J. A. Fodor (1981) argues in much the same way against the claim that there is no way to decide between grammars which are equally simple on 'linguistic grounds' alone (see esp. pp. 153–4).

amount of data which could be used to support various aspects of current linguistic theory given the Chomskian account. It is important to bear in mind, however, that it is not really necessary for our present purposes to establish that any of these data will stand up to further empirical scrutiny. The point here is a conceptual one. The burden of the Martian Argument is not to show that the sorts of evidence discussed above will not *in fact* be substantiated. The point is rather to question whether *in principle* such data are even *relevant* to the evaluation of linguistic theories. Thus, for the purposes of this discussion, we can take the evidence discussed above as given. In fact, we can suppose that some particular linguistic theory is corroborated in full detail along the lines suggested above. The point, then, is that the Chomskian account, in allowing such data to be relevant to the choice between linguistic theories, and construing linguistic kinds as intimately tied to the nature of language processing and acquisition, greatly increases the range of evidence that can be brought to bear in evaluating linguistic theories and confers enormous explanatory power on linguistic theory. And it does these things in ways that are very natural given that language processing and acquisition are perhaps *the* central linguistic explananda. So the Chomskian interpretation provides us with a satisfying account of what theoretical considerations might be brought to bear in determining whether or not a parrot, a tape recorder, or a Martian is in fact an English-speaker.

The main reply to considerations of this sort by opponents of the Chomskian account is that this account makes linguistic theory *insufficiently general*. This sort of reply is offered by philosophers attempting to provide alternative substantive accounts and those offering more or less instrumentalist accounts, where any number of grammars turn out to be 'equally true' of a given speaker, provided they are in some sense extensionally equivalent (and on some views equally simple or elegant).[22] These philosophers believe that it is a problem for the Chomskian account that it does not take Martians and human English-speakers to speak the same language. There

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[22] Among the non-substantive alternatives, I count Lewis (1983), Soames (1984), and perhaps Katz (1977). I should note that to the extent that these authors are being non-realist, it is not part of a general anti-realism with regard to all science. In each case the claim is that there is something special about linguistics which makes it the case that many alternative accounts are all 'equally true', in a sense that would not apply to alternative physical or biological theories.

are a number of points to consider in developing and responding to this argument.

The first is that it is not at all clear why we should stop here in the argument. Why stop with *Martians*? Why not extend the generality to cover, for example, beings of the sort Block imagines? Or parrots? Or valleys? Notice that we would then be including objects that do not make use of *any* internalized grammar. Of course we *could* say that all of these things *do* speak the same language, that normal human English-speakers, Martians, Block's beings, even parrots and valleys, are all equally well described by the whole host of 'extensionally equivalent' grammars that are said to describe our linguistic abilities. The problem with this move, I think, is that it robs linguistic theory of explanatory power. In seeking 'greater generality' we actually *lose* explanatory power.

Suppose that the data from acquisition, processing, and breakdown patterns continue to corroborate a particular grammar as the grammar internally represented in human English-speakers (and likewise for human French-speakers, German-speakers, Hindi-speakers, and so on), along the lines discussed in Section 2. Under the Chomskian account such data can be brought to bear in determining the correct grammars for the languages spoken by these various groups, and linguistic kinds will play an important role in explaining language acquisition, processing, and breakdown in these groups. As noted above, this is not true of all the extensionally equivalent grammars. Presumably most of these grammars will be completely unilluminating when it comes to describing and explaining these data from acquisition, breakdown, processing, etc.

But then I think we have to ask what exactly *the point* is of construing linguistic kinds in the way these opponents of the Chomskian view suggest. What is *the point* of construing linguistic kinds so that they apply equally to normal human English-speakers, Martians, Block's beings, even parrots and valleys? Now it seems *possible* to me that a conception of linguistic properties which construes linguistic kinds so that they apply equally to normal human English-speakers, Martians, Block's beings, even parrots and valleys, should be in some way valuable from an explanatory point of view. But at the same time, it is far from *obvious* that such a conception is in fact required.

What might defenders of the Martian Argument suppose needs to be explained? One possibility is that they simply feel that what look like the same phenomena from a superficial behavioural point of view must receive the same explanation, and the Chomskian account precludes this. Another possibility is that they might point to the possibility of 'communication' between human speakers of English and Martians. The second suggestion is perhaps more interesting, so let us look at it first.

Presumably the implicit argument here is that the ability of human speakers of English and Martians to 'communicate' with each other should be explained in terms of linguistic properties, and that linguistic properties construed along Chomskian lines are either unable to explain such 'communication' or cannot provide as good an explanation of such 'communication' as rival accounts can. I see no reason why this argument should be accepted, since it seems clear that there is in fact an explanation of all of this in Chomskian terms. The Martian language, like English, can be characterized relative to the linguistic competence of its speakers—in this case the grammar that the Martians have internalized. Communicative exchanges between human speakers of English and Martians would then be thought of (charitably) as essentially unintentional translations from one language to the other (or less charitably as involving relatively harmless systematic misinterpretations of each other's speech).

It is not at all clear that the defender of the Martian Argument can provide even an equally good explanation here, much less a better one. It will take some work to see why this is so. The details of the alternative explanation will vary somewhat depending on which alternative account we adopt. But the various alternatives would all presumably claim that there was a whole class of explanations (corresponding to the class of extensionally equivalent grammars describing the common language). These explanations will differ from the Chomskian one in that they will not get bogged down in 'irrelevant processing details' and therefore, perhaps, allow for some simpler, more general characterization of the linguistic properties involved in the communicative exchange. Supposing that this is true, what I want to call into question is whether this 'simplicity' and 'generality' should be thought of as a virtue. The considerations here are essentially the same as those involved in the question put aside earlier, of whether there is good reason to suppose that what look like the same phenomena from a superficial behavioural point of view must receive the same explanation. To put these issues in perspective, I think that it is once again valuable to consider the case of other sciences, such as chemistry or genetics / molecular biology.

Imagine someone making the analogue of the Martian Argument in chemistry or genetics/molecular biology, and charging that those who want to identify water with H_2O, or identify the things responsible for transmitting given traits in a population with chains of polynucleotides of a certain sort, were illegitimately limiting chemistry or genetics in taking these kinds to be insufficiently general. After all, the advocate of the Martian Argument analogy might go on, these reductionists would be excluding certain logically possible[23] substances which resemble water in appearance and behaviour as much as you like, or obey the same laws of trait transmission as genes do, and yet are distinct from H_2O or the proposed polynucleotide basis for the trait transmitter. Surely the response in this case would be that we *might* use the more general kinds, but what is *the point*, given that the 'more restricted' kinds the reductionist advocates vastly increase the explanatory power of the kinds and link these kinds to new and powerful sources of evidence? I am arguing that the same should be said in the case of linguistics.

Now suppose that our Martians look just like humans do, and to all appearances, are able to mate with humans in the normal way, and produce what seem to be normal human children. But suppose it turns out that in fact Martians have no DNA, and so do not transmit their traits to offspring in the same way that we do. Perhaps instead their cells contain a much more complex mechanism for controlling development, which when it interacts with a normal human sex cell 'reads' the genetic information off from this cell, and alters its development-controlling mechanism appropriately. Perhaps half the offspring produced in this way turn out to have human mechanisms of trait transmission and half have Martian ones. The details are unimportant since presumably there are a great many logically possible mechanisms which might do the job and whose superficial behaviour might resemble that of the human genetic system as much as you like. *The question then is whether such logical possibilities require us to give up our belief that genes are chains of polynucleotides of a certain sort as an insufficiently general account of the nature of genes.*

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[23] Advocates of the Martian Argument do not want to rest their argument on the empirical claim that such beings as Martians in fact exist, only on the fact that such beings are *possible*.

I think it is clear that they should not. But just as the defender of the Martian Argument asked 'But how are we to explain human/Martian communication?', the defender of the analogue of the Martian Argument might ask 'But how are we to explain human/Martian procreation?' Following the logic of the linguistic case we should presumably look for a (perhaps simpler) more general explanation in terms of 'genetic properties' *without regard for biochemical reality*. I think it is clear, however, that this would result in massive loss of explanatory power. The explanations gain much of their explanatory power by their link to the underlying biochemical properties. But this, of course, is just as it is in the case of the Chomskian account of linguistic properties which ties their nature directly to the nature of our abilities to acquire and process language.

I think it is also clear that the natural way we would go about explaining human/Martian procreation under the imagined circumstances would be in terms of the interaction of different kinds of 'trait transmitters'. Despite the superficial behavioural appearances, humans and Martians (in the imagined circumstances) have fundamentally different trait-transmission properties. Not only does this not preclude an explanation in terms of properties linked to particular sorts of 'processing' mechanisms, but such an explanation, in bringing out the real complexity of human/Martian procreation (not to mention the increased explanatory power of the kinds, and the wider evidentiary basis allowed), is actually preferable. Exactly the same considerations seem to apply to the Chomskian account of linguistic kinds.

The moral, I think, is that we do not want to leave the kinds of a science open to any logically possible 'counter-example' of the sort raised by the Martian Argument. The analogies to chemistry and biology make it clear that we cannot just assume that more generality—especially generality over various merely possible sorts of entities—is necessarily a good thing. There is a real danger of draining the explanatory power of kinds by trying to get them to explain too much. As the examples from chemistry and biology make clear, we often want our understanding of the nature of a kind to explain relatively specific properties associated with the kind, not just properties of the greatest generality.

Perhaps some future science of language or communication will be interested in more general properties than properties connected to our ability to process and acquire natural languages. Perhaps the science will be interested only in semantic properties of utterances and cover us, and Martians, and many other sorts of being. Perhaps. But I see no reason to let the nature of natural-language linguistic properties turn on this possibility. The kinds in linguistics, like the kinds in other sciences, can legitimately exclude, on essentially theoretical grounds, certain objects that might pre-theoretically be taken to be instances of those kinds, thereby increasing the explanatory power of linguistic theory in the ways suggested above. And assuming that the data concerning language processing, acquisition, and breakdown continue to be systematically describable on the assumption of an internal representation of particular grammars, I think it would be perverse to ignore these rich sources of evidence and to deny linguistic theory the generality and explanatory power involved here.

6 Conclusion

My argument for the Chomskian account of linguistics comes down to this. Because of the great explanatory power the Chomskian account offers linguistic theory in explaining a vast range of phenomena from language acquisition, processing, breakdown, and historical change, in terms of linguistic kinds (and the correlative prospect of extending the base of evidence for linguistic theory), the Chomskian account should be our default hypothesis concerning the nature of linguistic kinds. These considerations provide us with a powerful prima facie reason for accepting the Chomskian account. Of course, prima facie reasons are not conclusive reasons, and the Chomskian account may yet be dislodged. The data may not work out in the end. Perhaps we will eventually conclude that, contrary to our present expectations, *no grammar at all* is in fact internally represented. We will just have to wait and see about this. The question then becomes whether there are any powerful theoretical or conceptual (or further empirical) reasons for rejecting the Chomskian account in favour of one of its rivals. Here I have considered two influential arguments that have been raised against the Chomskian account, and I have argued that neither poses any real difficulty for the Chomskian account. Moreover, as the last section makes clear, apart from the question of whether there are effective arguments against the Chomskian view, the alternatives to that view themselves face serious empirical and methodological difficulties. The main issue facing the alternatives is that they all serve to reduce the usefulness of linguistics by restricting its explanatory power. They restrict the explanatory power of linguistics by putting indefinitely many extensionally equivalent grammars essentially on equal footing, though the vast majority of these grammars will not be psychologically real. At the moment, then, there is every reason to believe that linguistics *is*, as Chomsky claims, a branch of psychology.

Appendix

Scott Soames (1984) presents a curious blend of the Methodological and Martian Arguments that is worth considering briefly. Soames wants to argue that:

> (i) There is a theoretically sound, empirically significant conception of linguistics in which its subject matter is the structure of natural language, considered in abstraction from the cognitive mechanisms causally responsible for language acquisition and mastery.
>
> (ii) This conception of linguistics fits the ways in which practising linguists formulate, defend, and criticize their theories better than the received view does. (1984: 157)

Soames characterizes the domain of enquiry for linguistics by means of a set of 'leading questions'. He then argues that this domain of enquiry is conceptually distinct and empirically divergent from the study of psychological reality. In arguing for empirical divergence, Soames is in effect arguing that further evidence from neurology or psycholinguistics will not end up supporting current linguistic theory as a theory of what is psychologically real. In arguing for conceptual distinctness, he is in effect arguing that this evidence is not relevant, given his characterization of the autonomous domain of linguistics in terms of the 'leading questions' (and his interpretation of them).

'Leading questions' are central questions which work in a given field attempts to answer. According to Soames, among the leading questions of linguistics we find (Q1)–(Q3).

> (Q1) In what ways are . . . alike and in what ways do they differ from one another? Instances are obtained by filling in the blank with a list or description of some (or all) natural languages.
>
> (Q2) What (if anything) distinguishes natural languages from . . .? Instances are obtained by filling in the blank with a description of some set of artifi-

cial languages—e.g. 'finite state languages'—or animal communication systems—e.g. 'bee language'.

(Q3) In what ways have (has) . . . changed and in what ways have (has) . . . remained the same? Here, instances are obtained by filling in both blanks by the same description, or list, of one or more natural languages. (1984: 158)

In answering questions of this sort, Soames suggests, we will appeal to our pre-theoretic sense of what constitutes a linguistically significant property or relation, and this in turn will tell us that considerations such as (8)–(10) are not '*directly* relevant' to determining whether or not two groups, the Xs and the Ys, speak the same language or different languages:

(3) Xs process sentences of type A faster than they process sentences of type B; whereas Ys process Bs faster than As.

(4) Xs make fewer mistakes comprehending As than they do comprehending Bs, whereas Ys make fewer mistakes with Bs than As.

(5) Xs learn A-constructions as children before they learn B-constructions, whereas Ys learn them in reverse order. (1984: 159, original numbering)

Of course if such evidence is excluded, we may well end up with a number of equally simple and elegant theories, and these may well diverge from whatever grammar, if any, is psychologically real. It is hard to see, though, why such evidence should not be relevant, harder still to see how we could rule it irrelevant a priori.[24] An answer to any of the leading questions may well appeal to linguistic properties of any sort. If utterances have linguistic properties in virtue of facts concerning our capacities for language processing and acquisition, as the Chomskian holds, then evidence of the sort cited in (6)–(8) is quite relevant. Background assumptions about the nature of linguistic properties are thus being used to exclude these as relevant data.

Again it is useful to consider an analogy to another science. A Soamesian geneticist might argue as follows.[25] The domain of enquiry for the science of genetics is given by a set of leading questions:

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[24] I see no issue here between a piece of evidence's being relevant and its being '*directly* relevant'. Provided it is relevant evidence, I do not care whether or not it is '*directly* relevant' evidence. For a forceful statement of the view that what counts as relevant evidence is determined a posteriori, see J. A. Fodor (1981), 150–2.

[25] J. A. Fodor (1981) uses this argument against 'the Wrong View' (using the example of astronomy). As noted above, Soames and Lewis (both writing after Fodor's article) seem especially clear instances of the Wrong View.

(Q4) In what ways are . . . alike and in what ways do they differ from one another? Instances are obtained by filling in the blank with a list or description of some, or all, types of organism.

(Q5) What (if anything) distinguishes naturally replicating organisms from . . .? Instances are obtained by filling in the blank with a description of some set of artificial systems—e.g. self-duplicating programs—or non-organic natural systems of property transmission—e.g. in chemical reactions.

(Q6) In what ways have (has) . . . changed and in what ways have (has) . . . remained the same? Here, instances are obtained by filling in both blanks by the same description, or list, of one or more type of organism.

Appealing to our pre-theoretic sense of what constitutes a genetically significant property or relation tells us that while data from cross breeding are '*directly* relevant,' considerations such as (6)–(8) are not '*directly* relevant' to determining whether or not two groups, the Xs and the Ys, are both members of the same type of organism, or members of different types:

(6) Xs reproduce faster in situations of type A than situations of type B; whereas Ys reproduce faster in situations of type B than type A.

(7) Xs produce fewer mutations in situations of type A than they do in situations of type B, whereas Ys produce fewer mutations in situations of type B than type A.

(8) Xs exhibit trait A before they exhibit trait B, whereas Ys acquire these traits in reverse order.

The issue, as J. A. Fodor (1981) makes clear, is that one cannot specify a priori what counts as evidence in a science, and to all appearances (3)–(5), as (6)–(8), are relevant to the sciences of languages and genetics. If leading questions do set the domain for sciences, they do not thereby determine what counts as relevant evidence.[26] The main conclusion here is the same as that of the Martian Argument: one does not determine the subject matter of linguistics on the basis of pre-theoretic intuitions about whether or not two populations are or are not

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[26] Enough has been said now about Soames's view. I should add for completeness, however, that the argument for empirical divergence builds on the alleged conceptual distinctness. Noting the multitude of extensionally equivalent grammars, Soames argues that the best theory from the point of view of psychology will not be the best theory from the point of view of linguistics, since the two theories will have to answer to different sets of data, each appropriate to their domain as characterized by a set of leading questions. This argument can never get off the ground, though, if Soames does not have an argument for restricting the base of evidence for linguistics, as I have argued he does not.

speaking the same language—rather this is an issue that gets settled only by doing linguistic theory.

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