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LINGUISTICS: WHAT'S WRONG WITH "THE RIGHT VIEW"

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1. Introduction

What is linguistics about? A simple enough question, you might think, but one that has generated surprising controversy, much literature, and no very convincing answer.

The focus of the controversy has been on the transformational approach to syntactic theory, a central subdomain of linguistics. That will be our concern.

Jerry Fodor, who should know about such matters, says that "there are...really only two schools of thought on this question" (1981: 197). With careful neutrality he names these "the Right View" and "the Wrong View". The Right View is the standard view of Noam Chomsky and his followers, including Fodor. Fodor attributes the Wrong View (pp. 198-9) to Stephen Stich (1972) and Jerrold Katz (1977).[1]

The Wrong View is a sort of instrumentalism. The task of linguists is to construct a grammar that simply captures its data base, the linguistic intuitions of speakers; intuitions about grammaticality, ambiguity, passives, and so on. Provided the linguist gets things right at the level of the intuitive judgments, he has done all that is required. The theory is just an instrument for predicting some such judgments on the basis of others. According to the Wrong View, then, linguistics is not about anything beyond the concerns of those judgments; it is not about any deeper reality.

Related to our opening question is another: What does linguistics explain? The Wrong View holds that linguistics explains nothing: linguistic theory systematizes intuitive judgments but it does not explain them. The Wrong View is thus typical of instrumentalism.

Classical instrumentalism was motivated by a positivist epistemology and semantics. The Wrong View is not. Indeed, it has a perfectly respectable motivation. It arises from troubles with the Right View, together with an inability to see any other alternative to that view. We think that there is another alternative. If we are right, then the motivation for the Wrong View evaporates. So we shall not discuss that view any further.

The Right View is popular and familiar. Its main claim is that linguistics describes a grammar that has been internalized by each speaker; the grammar is, as they say, *psychologically real*. The speaker's linguistic competence consists in the internalization of this grammar (J. A. Fodor 1981: 199).[2] So, according to the Right View, linguistics is about linguistic competence. "Linguistics is simply that part of psychology that is concerned with one specific class of steady states, the cognitive structures that are employed in speaking and understanding" (Chomsky 1975b: 160).[3]

What does linguistics explain? According to the Right View, it is, like the rest of psychology, part of the explanation of behaviour. Most obviously, a grammar explains linguistic behaviour, but it is also relevant to the explanation of non-linguistic behaviour. To follow someone's instructions, for example, one must first *understand* them; that understanding utilizes the internalized grammar.

Chomsky insists on a distinction between competence and performance. The theory of performance is concerned with all the psychological factors that bear on the production and understanding of linguistic symbols. So it is concerned not only with the competence that is, according to Chomsky, the subject matter of linguistics, but also with such factors as memory, attention and interest. The theory of competence is the core of the theory of performance.

How can we tell what linguistics is about? Though we must take note of what linguists say it is about, we cannot rest content with that. Doing linguistics is one thing, reflecting on it, another. Linguists may have incorrectly abstracted the goal of their activity. We have to examine their activity for ourselves. The 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 con-

struction of linguistic theory justify our thinking that the theory is *true* about *x* but not *y*. So we shall look to see which view of linguistics makes linguistic theory seem true.

Oversimplifying, our conclusions are as follows. The Right View is wrong; linguistics is not part of psychology. The right view is another one altogether, which we will call "Grandma's View". Our actual conclusions are much more complicated because of an important conflation. We will argue that the transformational linguists conflate two distinct theoretical tasks: one concerned with linguistic symbols and the other with linguistic competence.[4]

2. Two Versions of the Right View

The Right View holds that a grammar is an account of psychological reality. But what is it for a grammar to be internalized in the way this requires? There are two answers to this question.

A grammar is a set of rules generating all and only the sentences of a language. The set for any natural language is still largely undiscovered. Let us suppose, optimistically, that transformational linguists have discovered some members of the set for English. Call this fragment of a grammar, "G". Three ways we might describe a competent English speaker's relation to G are as follows:

1. She behaves as if she is governed by G
2. She is governed by G
3. She knows that G is part of the grammar for English and applies it.[5]

1 is not enough for the Right View because 1 can be true as the result of the speaker having internalized something other than G which yields the same behavioral output as G. The Right View requires either 2 or 3. Which? The most common expressions of the View suggest 3: the competence that linguistics describes is the speaker's tacit knowledge that the rules of the grammar are as they are; she has knowledge-that, or propositional knowledge, of G. We need a name for this version of the Right View. We shall call it "the Crazy Version". The Right View is sometimes presented in ways that seem to require only 2: there is no implication that the speaker knows about, or has any other propositional attitude toward, G. The only knowledge 2 might require is knowledge-how, a cognitive *skill*. We shall call this "the Sensible Version" of the Right View.

The differences between 1, 2 and 3 can be brought out by a simple analogy. Suppose that R is a rule for addition. So R is a mechanical procedure guaranteeing the right answer to problems of addition; it is an algorithm. Consider, now, a pocket calculator. We might describe it in any of the following ways:

1*. It behaves as if it is governed by R
2*. It is governed by R
3*. It knows that R is an algorithm for addition and applies it.

Clearly, 1* might be true without 2* or 3* being so. There are many ways to add and the calculator may be governed by a rule other than R; for example, R might be in the decimal notation and the governing rule in the binary notation. Next, 2* might be true without 3* being so. An object can be built so that it is governed by R, and thus have internalized R, and yet not explicitly *represent* R. And if it does not represent R it cannot have any propositional attitude toward R. Hence it cannot *know* about R. In the case of the calculator, we can be certain that it does not know about R, because it has been built in such a way that it could not know about anything.[6]

3. The Crazy Version of the Right View

Do transformational linguists believe the Crazy Version? The question has been aired at great length and yet, amazingly, no clear answer has emerged. They do not typically assert anything like 3, yet key figures believe that the speaker's relation to G is appropriately called knowledge (or something close).

Chomsky's favourite way of describing this knowledge is not helpful: he says that the speaker *tacitly knows the rules or principles* of her language. Unfortunately, the import of "knows the rules or principles" is not crystal clear. We are inclined to think it requires knowledge-that but there is room for doubt about this: perhaps mere knowledge-how is sufficient. The inclusion of 'tacitly' may seem to settle the question, for we ordinarily take a person's tacit knowledge to be propositions that he has not entertained but which he would acknowledge immediately in suitable circumstances. Thus, Ron tacitly knows that rabbits don't lay eggs; the thought has never crossed his mind, but he would deny that they laid eggs if the question were ever to arise. However, Chomsky knows better than anyone that

the ordinary speaker would not acknowledge the rules of G. So this can't be what he means by 'tacitly'. Further, on occasions Chomsky explicitly denies that the speaker's knowledge is knowledge-that (1969a: 86-7; 1969b: 153-4).

The mystery deepens because the transformationalists so often write as if the speaker did have propositional knowledge of G and its consequences. Thus Chomsky describes the knowledge as a "system of beliefs" (1969a: 60-1; see also 1980: 225); as "a mental representation of a grammar" (1975a: 304). Learning a language is seen as learning a theory:

> It seems plain that language acquisition is based on the child's discovery of what from a formal point of view is a deep and abstract theory—a generative grammar of his language. (Chomsky 1965: 58)

Fodor describes Chomsky's view as propositional (1983: 4-10); it is the view that "your linguistic capacities...are...explained by reference to the *content of your beliefs*" (p.7). Finally, if the knowledge is not propositional, it must surely be knowledge-how. And, if we assume description 2, it is plausible to think that it is a particularly complicated piece of knowledge-how. However, Chomsky denies that it is knowledge-how (1969a: 87) or, at least, that it is knowledge-how without an "intellectual component" (1975a: 314-8).

Here and elsewhere (1975b: 162-4) Chomsky seems bent on undermining the distinction between knowledge-that and knowledge-how, between knowledge of facts and cognitive skills. Yet, as John Anderson says, this distinction "is fundamental to modern cognitive psychology" (1980: 223). Thus, long-term memory is taken to consist in the propositional (perhaps also imagistic representation of information (94-123). This is knowledge-that. On the other hand, cognitive skills like maze running, addition and chess playing, which may make use of representations of information, are not seen as consisting in such representations, but rather in, for example, "production systems" (222-92). These skills are knowledge-how.

We are left uncertain of the nature of the claim that the speaker has tacit knowledge of G.[7]

We set aside the exegetical issue for the substantive one. Is it reasonable to suppose that the ordinary speaker knows that G is part of the grammar for English and applies this knowledge in producing and understanding utterances? We think not. We think that 3, and

hence the Crazy Version, are grossly implausible.

First, no one has produced a single good reason for the Crazy Version; the considerations adduced for the Right View are only ones for the Sensible Version; they are for 2 not 3.[8] In the face of twenty years of apparently overwhelming objections to the Crazy Version transformationalists have hardly conceded a point. When they do respond to the criticisms, they do so with obscurities. But mostly they continue on as if it is sheer pedantry to insist on the distinction between the two versions.[9]

Second, Gilbert Harman (1967) has raised the following problem for the Crazy Version. According to that Version, understanding a language requires representing its grammar. That representation must itself be in a language. What is it to understand that more basic language? If we suppose the more basic language is the same as the original language then we are caught in a vicious circle. If we suppose that it is some other language ("Mentalese" perhaps), then its grammar also has to be represented. This requires a still more basic language. And so on. The only way to avoid a vicious circle or an infinite regress is to allow that at least one language is understood directly, without representing its grammar. Why not then allow this of the original language, the one spoken?

Some linguists, particularly Fodor (1975; see also Chomsky 1969a: 87-8; 1969b: 155-6), have an answer to this question. They think that there are good reasons for supposing that in order to *learn* a language you have to understand another one already. However, we can understand that one directly, without representing its grammar, because it is not learned; it is *innate*. So, the answer to Harman requires a strong innateness thesis.

Third, Stich (1971, 1978) brings out the implausibility of the Crazy Version nicely by contrasting the speaker's relation to G with unproblematic cases of propositional knowledge. If a person knows that *p*, we expect him to be aware of *p*, or at least to be able to become aware of it when given a suitable prompt; and we expect him to understand expressions of *p*. The ordinary speaker quite clearly lacks this awareness and understanding for most of G. If a person knows that *p*, his knowledge should join up with other knowledge and beliefs to generate more beliefs. If a speaker has knowledge of G it is clearly not inferentially integrated in this way. Consider an example. Without tuition, a speaker is unlikely to have the conceptual recourses to understand even the relatively simple claim that 'NP –> Det + Adj + N' is a rule of English. If she tacitly knows that this is a rule,

her knowledge is largely inferentially isolated from her other beliefs.

Fourth, Stich's point is strengthened by an important aspect of the transformationalists' views. At its most explicit and extreme, this aspect sees the area of the mind that employs G, the sentence-parsing area, as *modular*. Fodor is the main exponent of modularity (1983), but Chomsky also speaks favourably of the idea (1980: 40-7).

In the functional organization of the mind, modular systems lie between transducers and the central processor. Transducers are the familiar sense organs which convert incoming stimuli into neural code. The central processor is the site of higher mental functions; what goes on there is what we would ordinarily think of as thinking. Modular systems analyze the raw data received from transducers and pass on some of the results to the central processor. These systems are "domain specific", specializing in a particular sort of stimulus; for example, sentences or faces. They are "innately specified" and "hardwired", "associated with specific, localized, and elaborately structured neural systems" (J.A. Fodor 1983: 36-7). They are "mandatory": there is no decision to operate; they are cognitive reflexes (pp. 52-3). Finally, and most importantly for Stich's point, they are "autonomous" (pp. 36-7). On the one hand, "there is only limited central access to the mental representations" that modular systems compute (p. 57). On the other hand, the systems have no access to information held elsewhere; they are "informationally encapsulated" (p. 64). Hence the inferential segregation that Stich points to.

The mere internalization of G in a module cannot involve the speaker in any propositional attitudes towards G, for the module is inaccessible to the central processor which is the site of her propositional attitudes. If the modularity thesis is right, the speaker no more knows G in virtue of being able to talk than she knows the principles of depth perception in virtue of being able to see. If the thesis is right, the only thing that could "know" G is the sentence-parsing module itself.

The modularity thesis is highly speculative and controversial. But even if it is false, Stich's point is still supported. If Chomsky is anywhere near right about the sentence-parser, it has many of the properties of a modular system. He thinks of it as a relatively encapsulated "mental organ" inaccessible to our general cognitive capacities. G is in a relatively autonomous system. The contrast between a speaker's propositional attitudes and her relation to G remains.

We conclude that description 3, and hence the Crazy Version of the Right View, are highly implausible, perhaps even crazy. It is time to consider the Sensible Version.

4. The Sensible Version of the Right View: Criticisms

The Sensible Version of the Right View does not require that the speaker represents G, a fragment of the grammar for English that linguists are on their way to discovering; she need have no propositional attitude to G. It accepts that knowledge of a language is a cognitive *skill*, and hence may be mere knowledge-how not knowledge-that. The Sensible Version does not require description 3, but it does require 2. If linguistic theory is to be true, G must be psychologically real in that it governs the behavior of each speaker and is descriptive of her competence. We doubt even this.[10]

The internalization of G requires the internalization of all the levels of analysis hypothesized by G: deep structure, intermediate structures, surface structure, and the rules.[11] This is what we doubt and what we shall be discussing in this section and the next. We do not doubt that *some* aspects of G are psychologically real, in particular those aspects that go into determining meaning. The nature and point of this qualification will become clearer in sections 6 to 8.

Why should we suppose that we are entitled to claim anything stronger than description 1? After all, grammars are like other algorithms. If there is one set of rules that generates a set of English sentences and assigns them appropriate meaning-relevant structures, there will be many; there will be a mathematical explosion of algorithms. Suppose G′ is one of those alternative fragments of a grammar. Given that G′ has the same output as G, why suppose that it is G rather than G′ that is psychologically real? The evidence and considerations that have guided transformational grammarians provide insufficient reason for thinking that it *is* G.

(1) The early evidence for G was almost entirely about linguistic symbols: about which strings of words are grammatical; about the ambiguity of certain sentences; about the statement forms and question forms; about the synonymy of sentences that are superficially different; about the difference between sentences that are superficially similar; and so on. This evidence is linguistic, not psychological, and so does not seem to be the sort to throw immediate light on

what is in the head; on the precise nature of a speaker's competence, the mental state that plays the key role in her understanding and production of sentences. There is no basis in this evidence for thinking that the speaker has internalized G rather than the meaning-equivalent G′.

At this point, a protest is likely from old hands: "What you are referring to as linguistic evidence is really the evidence of the native speaker's intuitions. These intuitions reflect her underlying competence and are psychological. The role that they play as evidence shows that the grammar is indeed about competence." The protest raises difficult issues which we will take up later (section 9). We can set it aside now because it does not affect our claim that this evidence, even if the expression of intuitions, gives no reason for preferring G to G′.

(2) Transformationalists seek grammars that not only meet the linguistic evidence but are also simple and elegant.[12] So we can assume that G is simpler and more elegant than G′. But why is that a reason for thinking that G is psychologically real? Suppose that R is the simplest and most elegant algorithm for addition. On that basis alone we are not justified in ascribing R to any calculator. Perhaps the calculator is a child who adds by counting marbles. What is needed before ascribing R or G to an object is evidence about *its design*, about how it achieves its effect of producing additions or sentences. In the case of G, what is needed is psycholinguistic evidence.

These remarks against the bearing of the transformationalists' criteria of simplicity and elegance on psychological reality can be strengthened. First, the fact that G is more simple and elegant than its rivals is rather more evidence *against*, than evidence for, G being part of the grammar our brain is built to use. If innateness claims are right, our brains are specifically adapted to a certain class of grammars. Stephen Jay Gould (1980: 19-31) has used examples like the panda's thumb to show that adaptations are typically *not* maximally efficient engineering solutions to the problems they solve. For, adaptations are of pre-existing structures and this constrains the solutions possible. Second, as David Lewis has pointed out to us, the fact that G is maximally efficient and elegant from the grammarians' point of view does not entitle us to suppose it is optimal from the brain's point of view.[13]

(3) A dominant concern of transformational grammar has been language acquisition, which should provide psycholinguist

evidence.[14] However, until recently, attention to language acquisition has been more honorific than substantive:

> Many generativists assert that they aim to account for how children master their native languages, but the vast majority of their analyses do not contribute to that aim. (Hornstein and Lightfoot 1981b: 7)

Nevertheless, facts about language acquisition have more and more been playing a role in the construction of transformational grammars. We must see their bearing on the claim that G is psychologically real.

A language like English is a very complex system of rules. Yet, according to Chomsky and his followers, children learn this system quickly on the basis of meagre and misleading data. In particular, transformationalists emphasize the unavailability of negative data (that certain syntactic constructions do *not* occur), and the absence of systematic instruction about ambiguity.[15] There are certain sorts of errors that children do not make despite never having been warned against them. They end up making correct judgments about ambiguity despite the patchy and unsystematic evidence about ambiguity in their data. Given the lack of evidence available to the child, the transformationalists claim that it is highly implausible that language learning uses only the devices of empiricist learning theories. Rather, they claim, learning involves an innate language-acquisition device. This device constrains the grammar we can acquire. The system of constraints is called "universal grammar".

Recent versions of transformational grammar have presented a very rich picture of universal grammar: it is a set of principles that are incomplete schematizations of phrase-structure and transformational rules. They are completed, in learning, by fixing certain "parameters"; for example, fixing the ordering surface structure of basic constituents. The popular "government and binding" view exemplifies this approach.[16] So transformationalists claim that the only grammars that can be learnt are completions of the specified schematizations.

How might these considerations support G over G′ as an account of psychological reality? G might fit better than G′ into a theory of language acquisition: the decomposition of G into learned parameters and innately fixed principles might yield a more plausible account of language acquisition than the decomposition of G′. For learned parameters to be plausible, they should be simple, and robustly

manifested in the child's linguistic experience. For innate principles to be plausible they must, at least, be ones that seem to play a role in the acquisition of other languages. We would also want to see evidence for the principles from the development of language in a child and from language processing.

We think that arguments along these lines do help to restrict the grammars that are candidates for psychological reality. In particular, we think that the transformationalists are on strong ground in their claims about the data available to children.[17] Nevertheless, we doubt that these arguments come close to establishing that G is psychologically real.

First, the evidence for the innate principles is not strong. Since these principles must play a role in the acquisition of all languages, we look to other languages for evidence of them. The trouble is that very few languages have been studied in sufficient depth to provide evidence. Worse, where in-depth studies seem to agree on an innate principle, there is a high risk that this agreement is imposed by the method of study rather than discovered in nature. Among the many alternatives to G equally compatible with the linguistic evidence, how many are tested for their ability to account for the acquisition of English? Very few: most are ruled out as candidates by the psychologically irrelevant consideration of simplicity; see (2) above. *And the same goes for all the other languages that have been studied in depth.* If the language-acquisition test for each language was applied to a much wider range of grammars, all with the same prima facie claim to being psychologically real, perhaps we would get agreement on different innate principles. More likely, we would get no agreement at all.

In sum, to the extent that the language-acquisition test depends on an appeal to independently motivated innate principles, it assumes that certain grammars for other languages are psychologically real. But their psychological reality is as much in question as that of G. What we need is a principled reason for ruling out the members of the mathematical explosion of grammars for each language. The theory of language acquisition does not supply this reason. The theory starts from—indeed, probably must start from—fairly detailed assumptions about the nature of the psychological states that are the end product of language acquisition.[18] And the nature of those states are precisely what is in question.

Interesting recent work on language development and processing provides some evidence for the innate principles, but cannot close the above evidential gap. Facts of language development have been shown to be consistent with the principles.[19] The worry is that the facts would be consistent with many other principles with equally good claims to being considered. The evidence from processing is not extensive and is controversial. It depends on substantial assumptions about the forms of rules and the nature of the processor.[20]

We have claimed that assumptions about innate principles do not deal with the mathematical explosion of alternative grammars in a principled way. Suppose that we were to set that aside, taking the apparent agreement between languages to be discovered not imposed. Our second doubt about the arguments from language acquisition is that we may still have a mathematical explosion to deal with. If there is one set of rules that can be plausibly decomposed into learned parameters and agreed innate principles, then there may be many.

Considerations of language learning certainly restrict the range of candidates to be part of the psychologically real grammar, but they do not establish that G is part of it. What is needed to establish this is much more direct psycholinguistic evidence.

(4) If G is psychologically real then it plays a role in performance, in the production and understanding of sentences. We can look to psycholinguistics for evidence that G and not G′ does indeed play this role. In principle, such evidence could settle the matter: evidence about reaction times,[21] the types of errors we make, the relative ease of understanding sentences, the order in which sentences are learned, and so on. Thus, suppose that according to G the active (e.g. 'Max bit Sam') is the basic form and the passive (e.g. 'Sam was bitten by Max') the derived form, but that according to G′ the reverse is the case. Now suppose that we discovered that actives are learned before passives; that they are easier to understand and remember; that fewer errors are made with them. Evidence of this kind would favour G over G′ as an account of psychological reality.

In practice, however, psycholinguistic evidence of this sort has not supported the view that G rather that G′ has been internalized by the speaker. 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 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.[22] Even Robert Berwick and Amy Weinberg, who defend the psychological significance of transformational grammar, emphasize the great difficulty in getting performance data to bear on the psychological reality of grammars (1984: 35-45). The most that they hope to do is show the functional plausibility of the general principles of current transformational grammar.

We can sum up the discussion in this section as follows. To establish that G is psychologically real, hard psychological evidence is needed. Yet, as David Lightfoot points out, "the overwhelming mass of crucial evidence bearing on the correctness of grammar" has been linguistic (1982: 28).[23] This linguistic data cannot distinguish between many grammars generating the same sentences with the same meaning-relevant structures.

5. The Sensible Version of the Right View: Responses

What responses have been made to criticisms like ours? We shall start with one from David Marr.

Marr (1982) has used his well-known distinction between levels to defend Chomsky. Marr distinguishes three levels of understanding an information-processing device. The first is the *computational* level: it specifies *what* is computed and *why*. The second is the *algorithmic* level: it specifies *how* the computation is done. The third is the level of *implementation*: it specifies the way the computation is realized physically (pp. 22-5). Marr claims that the distinction between the computational and the algorithmic levels is "roughly [Chomsky's] distinction between competence and performance". He thinks that critics of Chomsky have overlooked the distinction between levels, and have criticized the grammar as if it were at the algorithmic level when it is actually at the computational level (p. 28).[24] The problem with this defense is that everything said at the computational level does still have to be true of the device. So all aspects of the grammar do have to be psychologically real; the speaker really must perform that computational task in all its detail. That is precisely what is in doubt. It is true, of course, that there may be many different ways of carrying out that task at the algorithmic level, differences which are not the concern of the grammar. But that is beside the point of our criticisms.

Nevertheless, we think that Marr's distinction helps to bring out what is right and wrong about the Right View. Briefly, we think that transformational grammar conflates Marr's computational and algorithmic levels. We shall discuss this in section 8.

Chomsky and his followers have a standard response to criticisms like ours.[25] They point out that their claim about grammatical rules is a typical scientific hypothesis based on *inference to the best explanation*. An account is needed of how we produce and understand English sentences. The best explanation of this is that we have built into us the very same rules that a variety of evidence suggests define English sentencehood. So we are entitled to infer that we do have those rules built in. The grammarians go on to draw attention to two aspects of this inference which it shares with *any* inference to the best explanation.

(i) It is irrelevant to argue that the conclusion of the inference may be wrong. *Of course* the evidence does not prove the hypothesis conclusively: it is *underdetermined* by the evidence like any other scientific hypothesis. Perhaps other hypotheses would provide better explanations. But again that is true of any hypothesis and is therefore beside the point. Until other hypotheses are produced we are entitled to accept this one.

(ii) When we accept a theoretical hypothesis we should interpret it *realistically*: it purports to describe an area of reality underlying our observations. The alternative view is instrumentalism: the view that an hypothesis is simply an instrument for predicting observations on the basis of past observations; it does not describe an underlying reality. Instrumentalism as a general doctrine is discredited and it would be unjustified discrimination to apply it to linguistics in particular.

In sum, the grammarians conclude that they are entitled to believe that their grammatical rules really do describe the reality underlying linguistic behaviour. That reality is psychological, the speaker's competence.

Chomsky offers a nice analogy (1980: 189-92). Suppose that physicists are unable to get any direct evidence about the inside of the sun. The best they can do is construct a theory of the inside that, if it were true, would explain the observed behaviour of the sun. Any such theory may be wrong of course, but they are entitled to believe the best one they can come up with.

We agree with these general observations. It is pointless to object

that the conclusion of an inference to the best explanation might be wrong. And, certainly the conclusion should be construed realistically. (Sydney realism is the most virulent known strain.) However, we do not think that those general views yield the desired result in this particular case: they are insufficient to justify 2 and hence the Sensible Version.

The key question is whether the inference to the psychological reality of G is an *appropriate* one. Nobody knows how to codify inferences to the best explanation, but two conditions on appropriate ones are obvious enough. The proposed explanation must be both good and better than its rivals. It is no canon of science to accept a bad explanation because others are worse, nor to draw explanations out of a hat when we have a range of good ones.

Is G—say a Trace Theoretic version of transformational grammar—on the way to a good explanation of competence? Surely we must be agnostic here. So little is known of the computational mechanisms of the mind that we have no idea whether G is even a candidate for psychological explanation. Is G better than G′? Again, suspension of judgment is appropriate. What is the reason for preferring G to the many possible alternatives?

These remarks can be strengthened. Chomsky asks us to imagine that physicists have constructed a theory correctly predicting the sun's behaviour. If there was general agreement that this was a good theory and the best available, then its tentative endorsement would be appropriate. But transformational grammar is not yet in anything like that position. Our earlier supposition about G is indeed optimistic: there never has been a stable consensus about even the rough details of the form and structure of a grammar. We certainly wouldn't attribute solar reality to a theory that was supported largely by indirect evidence, was rejected by many experts, and which past experience suggested would be abandoned within five years.

In sum, inference to the best explanation does not warrant the conclusion that G is psychologically real. We are not justified in accepting description 2.

Where does our discussion leave the Sensible Version of the Right View? According to that version, linguistics is about competence—an internalized grammar—and is part of the explanation of behaviour. If that were the case, then the lack of justification for 2 would leave linguistic theory itself unjustified. This is a good reason for thinking that the Sensible Version is not right.[26] We think that it is indeed

not right. But it is not entirely wrong either. A final verdict must wait on our discussion of the theory of symbols, and transformationalists' conflation of that theory with the theory of competence.

6. The Conflation of Symbol with Competence

Suppose we were to ask Grandma what linguistics is about. She would be likely to say: "language". If we pressed, we would hear about words and the way they fit together into sentences; about nouns and verbs, actives and passives, ambiguity, and so on. In brief, Grandma sees linguistics as being about the properties and relations of *linguistic symbols*. We doubt that we would hear a word from her about competence; not a glimmer of the Right View. This is a little embarrassing for Fodor, because he likes to think that Grandma, in her rough and ready way, is rather wise. We agree and only wish that he and Chomsky took more notice of her.

The problem is not that Chomsky and his followers deny what Grandma says. Rather, they conflate the idea that linguistics is about symbols with the idea that it is about competence. Further, they give priority to the latter.

In the opening pages of *Syntactic Structures*, the work that began transformational grammar, Chomsky describes linguistics as follows:

> The fundamental aim in the linguistic analysis of a language L is to separate the *grammatical* sequences which are sentences of L from the *ungrammatical* sequences which are not sentences of L and to study the structure of the grammatical sequences. (1957: 13)

The stated concern here is with linguistic symbols. Much discussion in transformational grammar gives the impression that this is indeed the concern. The discussion is about such matters as ambiguity, word order, and synonymy.

Nevertheless, as we have seen, the favoured way of describing the linguistic task is quite different. Thus, in another classical work, *Aspects of the Theory of Syntax*, almost in the same breath as some remarks like those quoted above, Chomsky says:

> The problem for the linguist...is to determine...the underlying system of rules that has been mastered by the speaker-hearer...Hence, in a technical sense, linguistic theory is

> mentalistic, since it is concerned with discovering a mental reality underlying actual behavior. (1965: 4)

Here the concern is not with symbols, a human product, but with competence, a characteristic of the human mind. This is the Right View.[27]

Signs of the conflation are to be found in our recent discussion of the Sensible Version. We saw then that transformationalists use two different sorts of evidence in constructing a grammar. These bear directly on two quite different theories. The linguistic evidence bears directly on the theory of symbols and the psycholinguistic evidence bears directly on the theory of competence.

The properties of symbols that concern a grammar are *syntactic* properties. Full semantic properties—the properties that determine meaning—include reference. Reference involves word-world connections on which a grammar casts no light. Similarly, the competence that concerns a grammar is *syntactic* competence. Full semantic competence requires getting reference right. Reference is not fully determined by anything in the head. So, the conflation that we have been describing is that of the theory of the syntax of symbols with the theory of syntactic competence.[28]

These theories are very different. Linguistic competence is a mental state of a person that explains her linguistic behaviour; it plays a key role in *the production of* that behaviour. Linguistic symbols are the result of that behaviour; they are *the products of* that behaviour. They are datable placeable parts of the physical world: sounds in the air, marks on the page, and so on. They are not mental entities at all. A theory of a part of the production of linguistic symbols is not a theory of the products, the symbols themselves. Of course, given the causal relation between competence and symbol, we can expect a theory of the one to bear on a theory of the other. We consider that relation in section 8. But the relation does not make the two theories identical.

The theory of symbols is concerned with the properties of symbols that make them good for certain purposes. What is it about them that leads people to produce and respond to them as they do? The simple answer is that symbols have meaning. But then what is meaning? It has been discovered that syntactic properties are part of the answer.

Analogously, we might be interested in what makes a certain move-

ment of a ball a good tennis shot. The answer would be in terms of such properties as speed, direction and height. Or we might be interested in what makes a certain chess move good. The answer would be in terms of the myriad possible game continuations; perhaps, in each of these the move gives white an advantage and no other move guarantees this. In all of these cases we are concerned with objects or events in the physical world "outside the head".

However, in each case we might have another concern which is very much with something "inside the head" (or, at least, "inside the body"). What is the explanation of the behaviour—certain movements of hand and arm, perhaps—producing good sentences, tennis shots or chess moves? To answer this, we need a psychological (perhaps, physiological) theory, a theory of competence; we need a theory that explains, for example, how white knew that that particular chess move was good. Such a theory is different from a theory of the objects produced by the behavioural output of a competence: different from a theory of linguistic symbols, tennis shots or chess moves.

The difference between the theory of symbols and the theory of competence can be made vivid by considering the many ways in which a person could be competent. 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.[29]

In sum, linguistic competence, together with various other aspects of the speaker's psychology, produces linguistic behaviour. That behaviour, together with the external environment, produces linguistic symbols. A theory of symbols is not a theory of competence.

The conflation of these two theories is bewildering. Why do transformationalists conflate them? We shall conclude this paper with a tentative diagnosis (section 10).

7. Grandma's View

Having distinguished symbols from competence, we urge Grandma's View of what linguistics is about: it is about symbols and explains the properties in virtue of which symbols have their roles in our lives; more cautiously, that is what it ought to be about and do. We shall argue for Grandma's View by saying more about the theory of syntax and, in the next section, more about its relation to the theory of competence. Some of these matters are discussed in more detail in our *Language and Reality* (1987: parts I-III).

(1) What are the purposes for which symbols are good? The purposes, as almost everyone has seen, are communicative ones. People use symbols to communicate information to each other about the human and nonhuman environment. People use them to greet, question, command, joke, offend, abuse, intimidate, and so on. These are social purposes. If our public language is also our main medium of thought, as we think it is, then linguistic symbols have another important, but not social, role in our lives.

(2) Symbols serve those purposes in virtue of their meaning. We mentioned above some properties that we think contribute to the explanation of the meaning. Those properties were syntactic, reflecting the concerns of this paper. In our view, syntax fits into the total explanation as follows. The core notion in the explanation of meaning is that of truth conditions; it is largely because a sentence has certain truth conditions that it has its role. The sentence's property of having those truth conditions is to be explained in terms of its syntactic structure and the reference of its parts. Reference is to be explained partly by a description theory but, ultimately, by a causal theory taking into account links to nonlinguistic reality. So syntax has its place in the explanation of truth conditions.

(3) The version of Grandma's View that we are urging takes linguistics to be about symbol *tokens*, datable, placeable, parts of the physical world. This version of the View should be distinguished from another which takes linguistics to be about symbol *types*. Talk of types is often just a convenient shorthand for talk of tokens of those types. If that were all that talk of linguistic types amounted to, the second version of Grandma's View would collapse into the first. However, at least one linguist, Katz (1977, 1981, 1984a), seems to take linguistics to be irreducibly about types. It is about Platonic objects just as mathematics is often thought to be. Fodor treats this as

if it has a fair chance of being the Worst View. His objection, which strikes us as sound, is that the Platonic view makes linguistics unempirical (1981: 205-6).

The view that linguistics is about tokens seems to have become discredited through its association with the anti-theoretical practices of pre-Chomskian structural linguistics. Clearly, this misguided past should not be allowed to count against the view.[30] Aside from that, there is a surprisingly popular objection to the view.[31] Crudely, linguistics can't be about tokens because there aren't enough of them. The grammar for English applies to a potentially infinite number of symbols. Yet there can be only a finite number of tokens. So, linguistics can't be about tokens.

One might as well object that a theory of tigers can't be about the beasts that stalk India and excite interest in zoos. Tiger theory is open-ended. It follows from the theory that if something led to the existence of a tiger token, then that token would be, say, carnivorous. Similarly, the grammar for English describes the characteristics of anything qualifying as a sentence of English. Its application is not limited to tokens actually produced. It follows from the theory that if something led to the existence of an English sentence token, then that token would be, say, tensed.

(4) In distinguishing the theory of symbols from the theory of competence, we may seem to have made it mysterious. We do think that linguistics, as the theory of symbols, has a certain autonomy from other theories, including psychology. However, we are physicalists and so must see this autonomy as only relative: in some sense, linguistics must ultimately be explained in physical terms. But this requirement does not remove the autonomy of linguistics any more than it removes that of, say, biology or economics.

Linguistics is a social science. Like all social sciences, it 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. Yet each social science proceeds largely undisturbed by the lack of a complete description. And so it should.

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. Yet people quite properly feel free to theorize about chess, money, elections and the law. Similarly, we should feel free to theorize about linguistic objects and events.

It would be interesting to consider the way in which the syntactic properties that have concerned us in this paper are dependent on psychological fact. We must leave that to another place.[32]

8. Symbols and Competence

It is time for a verdict on the Right View. To give this we must first say a brief word on the relationship between the two theories that the transformationalists conflate: the theory of the syntax of a language and the theory of syntactic competence in the language.

The theory of syntax assigns to sentences structures that are relevant to the sentences' meanings. The theory of syntactic competence assigns to sentences structures that play a psychological role in the sentences' production and comprehension. The latter structures must include the former. For, the production and comprehension of sentences by the competent speaker requires the matching of those sentences with thoughts *having the same meaning-relevant structures*. To be competent is just to have the skill of matching sentences and thought that are, in this way, alike.

So, the meaning-relevant structure that the theory of syntax assigns to sentences must be the same as the meaning-relevant structure the theory of syntactic competence assigns to matching thoughts. But the theory of competence is interested in more than this structure: it is interested in *how* the speaker matches the meaning-relevant structures of sentences and thoughts. There are many different ways of matching involving many different structures. These differences are irrelevant to the theory of syntax but vital to the theory of competence because of its concern with the psychological processes that competence contributes to performance.

An earlier example helps to bring out the difference between the theory of syntax and the theory of competence. According to G the active (e.g. 'Max bit Sam') is the basic form and the passive (e.g. 'Sam was bitten by Max') the derived form, but according to G′ the reverse is the case. Since each sentence is assigned the same meaning-relevant structure by G and G′, this difference does not matter at all to the theory of syntax. Yet it is crucial to the theory of com-

petence. It is to settle precisely this sort of question that psycholinguistic evidence about acquisition, reaction times, and the like are sought. This evidence has no immediate relevance to meaning.

Marr's earlier-mentioned distinction between the computational and algorithmic levels (section 5) is helpful here. The computational theory characterizes the nature of the task; "a mapping from one kind of information to another" (1982: 24). In Marr's example of a cash register, the mapping is from pairs of numbers into single numbers; and, of course, that mapping has to get the addition right (p. 22). So the characterization of the computational task that must begin a theory of the register involves a piece of arithmetical theory. In the case of a syntactically competent speaker, the mapping is between syntactic structures and sentences; and that mapping *has to get the meaning right*. So the characterization of the computational task that must begin a theory of competence involves a theory of meaning-relevant structure. The problem with transformational grammar is that it goes beyond this linguistic task to questions of *how* the speaker performs this task. It introduces analyses and psycholinguistic data that are irrelevant to the linguistic task. These analyses and data bear on the algorithmic level. The conflation we have complained of is roughly that of this algorithmic level with the computational level.

Marr claims that Chomsky's theory "is a true computational theory": it "is concerned solely with specifying what the syntactic decomposition of an English sentence should be, and not at all with how that decomposition should be achieved" (1982: 28). But what is involved in Chomsky's syntactic decomposition? It goes way beyond the structure that is necessary to determine meaning. So it goes way beyond the characterization of task required by a computational theory of the competent speaker. Though Chomsky is not concerned with the algorithm for performance, his psychological approach to grammars seems clearly concerned with the algorithm for competence. There are levels within the algorithmic level.

Consider now the verdict on the Right View. The View seemed wrong because it made linguistic theory depend on a dubious inference to the best explanation: the psycholinguistic reality of G (section 5). (Perhaps this inference was made attractive to transformationalists by their conflation of theories; section 6.)

In contrast, Grandma's View makes linguistic theory seem probably true. It makes the truth of the theory depend on an inference to the

best explanation that is not dubious: the linguistic evidence warrants acceptance of the linguistic reality of G. We have good reason to believe that symbols have the meaning-relevant properties that G assigns to them. The existence of alternative grammars like G′ is no problem. G′ is meaning-equivalent to G and so assigns the same meaning-relevant properties. It is therefore as linguistically real as G. The differences between G and G′ are linguistically irrelevant.

It would be nice to say simply that Grandma is right and Right is wrong. However, this would be far too simple.

First, we have seen that the meaning-relevant structure of thoughts that competence matches with symbols must be the same as that of the symbols. The theory of symbols supplies the characterization required for the computational level of the theory of competence. So, Grandma's View implies that there is some truth in the Right View.

Second, the psycholinguistic evidence that has gone into the construction of G is directly relevant to a theory of competence but not to a theory of symbols. This shows that linguistics, as currently practiced, is partly about competence not symbols. To this extent, the transformationalists' belief in the Right View has *made* it right and Grandma's View wrong.

These remarks are concerned with what *is* the case with linguistics. What *ought* to be the case? There are two tasks which, though related, ought to be kept distinct: one concerns symbols, the other competence. That is what we insist on. Both tasks are, of course, worthwhile and there is no point in arguing about which task we should *call* "linguistic". However, we suggest that it is natural to call the former, which is about language, "linguistic", and the latter, which is about a psychological state of people, "psycholinguistic". Adopting this usage, we conclude that linguistics ought to be about linguistic symbols. Grandma's View, which is largely right, ought to be entirely right.

Finally, our discussion has revealed a certain priority of linguistics over psycholinguistics. Just as a theory of competence at tennis or chess depends on a prior grasp of the nature of tennis or chess, so also does the psycholinguistic theory of competence at a language depend on a prior grasp of the nature of the language. This accords with the priority that Marr seems to imply (1982: 27-8) for the computational level over the algorithmic: understanding what is computed has some sort of precedence over understanding how it is computed.[33]

9. Linguistic Intuitions

We have mentioned (section 4) a likely protest against our argument: we have failed to take proper account of linguistic intuitions which play such a prominent role in the practice of transformationalists. These intuitions are speakers' linguistic beliefs: beliefs that a certain sequence of words is a sentence; that another is ambiguous; that two others are paraphrases, and so on. When we talk of the "linguistic evidence" for the theory of symbols, what we are really referring to are these intuitions. The evidence these intuitions provide is psychological (Fodor, Fodor, and Garrett 1975: 244). How then can the theory based on them not be psychological? So, how can it be so sharply distinguished from the theory of competence? Even though our intuitions do not support G over G′ as an account of psychological reality, our rejection of the Right View seems too hasty.

We have said that, according to the Right View, linguistics is part of the explanation of behaviour. The important evidential role that transformationalists give to linguistic intuitions often leads them to claim that linguistics also, perhaps primarily, explains those intuitions.[34]

There are two quite different things that might be meant by a claim that intuitions are evidence. One is: *the fact that speakers have the intuitions* is evidence. The having of an intuition is a psychological phenomenon and so would be evidence for a psychological theory. In particular, to the extent that the speaker's competence is causally responsible for her having linguistic intuitions, they are indeed psychological evidence for the theory of competence. And to that extent, linguistics, if identified with the theory of competence, does explain the having of intuitions.

The having of an intuition can be evidence for a psychological theory even though the intuition is thought to be false. Thus, an atheist can take the having of religious intuitions to be evidence for or against a theory of, say, irrationality.

The alternative interpretation of the claim is importantly different in this respect. For the alternative is to suppose that the *content of the intuitions* is evidence. And that can be so if, but only if, the intuitions are likely to be true. The contents of true intuitions are evidence for a theory of the phenomena they are about. Indeed, strictly speaking, it is the phenomena, not our intuitions about them, that are the evidence. In this way, intuitions may be physical evidence, biological

evidence, economic evidence, or whatever. They are psychological evidence only if they are about psychological phenomena.[35]

Insofar as the theory of symbols is supported by linguistic intuitions, it is supported by their contents. These intuitions are about linguistic symbols. So, to the extent that they are likely to be true, their contents are indeed evidence for a theory of symbols. They are not psychological evidence. Linguistics, if identified with the theory of symbols, explains what intuitions are about, not the having of those intuitions. Our distinction between tasks stands. The Right View should still be rejected.

There is a more subtle objection based on the role of intuitions.[36] The objection accepts that there are two tasks to be performed, but claims that their conflation is harmless, even appropriate, because the only way to throw light on symbols is by examining competence. For, our linguistic intuitions provide the only evidence for the theory of symbols; and it is plausible to see these intuitions as reflections of our underlying linguistic competence ("the voice of competence"). The Right View's failure to mention symbols is excusable because the theory of symbols is completely derivative from the theory of competence.

Something has already been conceded to this objection. We pointed out that the Right View is not entirely wrong because the same meaning-relevant structure is to be found in symbol and matching thought. So the theory of the structure of the one must also be a theory of the structure of the other.

Of course, the theory of competence is concerned with more than this structure of matching thoughts. It is concerned with other structures involved in matching. This concern is irrelevant to the theory of symbols. However, this does not undermine the objection. According to the objection, the theory of competence includes the theory of symbols but is wider than it.

It is the objection's claim of epistemic priority for the theory of competence that seems damaging to our position: the claim that the theory of symbols is derivative. This claim is based on assumptions about linguistic intuitions: (i) that they are the only evidence about symbols; (ii) that they are reflections of competence.

(i) must be an exaggeration. People produce and react to linguistic symbols. We can use information about such phenomena, and any other information we have about humans and their social life, in theorizing about symbols.

(ii) is more difficult to deal with. Suppose that description 3 were true of the speaker: her competence consists in tacit propositional knowledge of the grammar, as the Crazy Version requires. (ii) would then seem very plausible.

If the speaker has this knowledge of the grammar, it is plausible to see her intuitions as stemming from the knowledge in some way. Most simply, the intuitions might be seen as a straightforward inference from the knowledge, in which case her intuitions would be the very best evidence of the nature of her competence. However, linguists have generally preferred to see the intuitions as related less directly than this to the tacit knowledge: that knowledge, together with other factors, explain the having of intuitions (J. A. Fodor 1981: 200-1). Still, the intuitions would be largely reflections of the underlying competence, as (ii) claims. Since they largely reflect *knowledge*, they should be true, and hence good evidence of the nature of symbols.

However, the speaker's competence does not consist in tacit knowledge of the grammar. So (ii) must be reassessed.

We think it likely that a speaker's competence resides in a sentence-parsing module, or in something similarly inaccessible to the central processor (section 3). The central processor is the home of intuitions. Given the inaccessibility of the sentence-parser, it is an open question what role the module has in producing the intuitions. It is certainly unlikely that there would be anything close to the above straightforward inference. The module may not even "speak the same language" as the central processor.

What else but competence might have a role in producing linguistic intuitions? The central processor is the obvious candidate. 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.

Suppose that linguistic intuitions were entirely the result of central-

processor activity, past and present. They would not then be reflections of competence and (ii) would be false. As a result, linguistics would not explain our having of the intuitions we have. Furthermore, the claim that the theory of competence is epistemically prior to that of symbols would be groundless.

Which of our two possible sources is mainly responsible for linguistic intuitions? The sentence-parser's main role is obviously to *do things with* language not to *provide us with thoughts about* language. Nevertheless, we think it likely that the sentence-parser does have a significant role with the most basic intuitions—for example, judgments of similarity and difference. Similarly, it is likely that the perception modules have a significant role with the most basic perceptual intuitions. However, it is unlikely that the perception modules are the sole source of those intuitions; recognition and classification depends partly on central memory. It is also unlikely that the sentence-parser is the sole source of basic linguistic intuitions. Many have argued that a person's linguistic intuitions are sensitive to her context.[37] If so they are sensitive to information represented in the central processor. Furthermore, to the extent that intuitions depend on the acquisition of theoretical concepts like *grammatical*, *ambiguous*, *passive*, and *noun phrase*, central processing will play a role.[38]

To sustain (ii), the central processor must have hardly any role in forming linguistic intuitions. It would have to be the case that our intuitions are largely uninfluenced by any thinking about linguistic phenomena, and by anything we learn about language at home and at school. Linguistic phenomena would have to differ from all other—physical, biological, economic, and so on—in failing to make us think.

In sum, we accept that there is a close relation between the theory of symbols and the theory of competence. However we do not accept that the role of linguistic intuitions shows that the conflation of the two theories is appropriate, nor that the former theory is epistemically prior to the latter. The intuitions are not simply the reflection of competence; we doubt that they are largely so. To the extent that they are not reflections, they are not, strictly speaking, evidence for the theory of symbols. The evidence is rather the linguistic phenomena that give rise to the intuitions. In any case, the intuitions are not the sole evidence for the theory. We see no reason to revise our verdict on the Right View.

10. Diagnosis of the Conflation

A major theme of this paper has been that linguists conflate the theory of the syntactic properties of symbols with the theory of syntactic competence. Why do they do this? We shall finish by hazarding a few guesses.

First, equivocation between taking 'knowledge' to refer to the *content* of knowledge and taking it to refer to the *state* of knowledge may lead to a slide from the theory of symbols to the theory of competence.[39] The initial conception of the task is to describe a language L. L is what L-speakers know; it is their knowledge. So the task is to describe L-speakers' knowledge. This follows, however, only if 'knowledge' refers to content. The slide comes if we move from this, and the fact that L-speakers' knowledge is their competence in L, to the conclusion that the task is to describe their competence. For this move holds only if 'knowledge' refers to the state.

Second, the fact that the grammarians tend to think of competence as propositional *knowledge* of syntax facilitates the conflation. For, a theory of competence would then be a theory of the syntactical rules known by the speaker. And if the speaker *knows* that a certain rule applies to a linguistic symbol then, of course, it does apply; knowledge implies truth. So, once the speaker's knowledge had been described, there would be nothing further to say about linguistic symbols.

Third, confusion over the nature and role of linguistic intuitions may be significant. We have seen (section 9) that if these intuitions are taken to be reflections of competence and also the only evidence for the theory of symbols, then it is plausible to see that theory as derivative from the theory of competence.

Fourth, the strange objection that linguistics can't be about symbol tokens because there aren't enough of them (section 7) has discredited the idea that linguistics can literally be about symbols at all. For that idea is then thought to be that linguistics is about types. If it were about such Platonic entities it could not be empirical. It may then be thought that linguistics can be made empirical, and able to accommodate the infinite number of sentences in a language, only by taking it to be about our *capacity* to produce and understand any of those sentences.

Fifth, the theory of symbols, like other social theories, is partly dependent on psychological facts. We think, though we have not

argued, that this dependency is particularly striking in the case of the syntactic properties of the symbols. Perhaps this dependency prevents people from seeing the relative autonomy of the theory of symbols from psychology.

These points are derived from our discussion. The next two are not.

Sixth, it seems that it is hard to notice the difference between linguistic behaviour and linguistic symbols. Yet they obviously are different. The behaviour is a series of bodily movements, usually of vocal chords or hands. The symbol is an object in the world produced by the behaviour with the help of the environment, usually a sound or an inscription. The explanation of linguistic behaviour that is the concern of the theory of competence is a psychological description of its *cause*. The explanation of a linguistic symbol that is the concern of the theory of symbols is a semantic description of its *nature*. (In some sign languages the behaviour is the symbol, but that does not alter the point: we have a different explanatory interest in it *qua* symbol from *qua* behaviour.) Perhaps the failure to distinguish behaviour from symbol encourages the conflation of a theory that partly explains the former with a theory that explains the latter.

Seventh, we have found a place for syntax within a truth-conditional theory of meaning (section 7). Yet it is very difficult to see why we need such a theory. What does it explain?[40] We think that the distinction we have just made between linguistic behaviour and linguistic symbols is vital in answering this question (1987: sec. 9.4). The absence of a clear and agreed answer may lead people to miss the task of explaining symbols altogether.

In conclusion, the conflation of a theory of symbols with a theory of competence has caused confusion about the subject matter of linguistics. We think that the subject matter is largely, and ought to be entirely, symbols. Oversimplifying, Grandma is right, Right and Wrong are both wrong.[41]

Notes

1. The attribution to Katz is probably mistaken; see Katz 1984a.
2. Linguists of the Right persuasion thus use 'grammar' with a systematic ambiguity: both as the theorist's construction and as the possession of each native speaker (Chomsky 1965: 25).
3. Hornstein and Lightfoot claim that "a psychological interpretation for grammatical claims has often been adopted by modern writers and by some traditional grammarians" (1981c: 28n). Indeed, such an interpreta-

tion seems to have a long history; see Saussure 1916: 77,90.

4. After we had delivered an earlier version of this paper, our attention was drawn to an excellent paper, Soames 1984a. This takes a similar view to ours on the conflation and on what linguistics is about.
5. In an earlier work (1987: 134-42), we used the verb 'follow' instead of 'govern'. We have changed because 'following a rule' suggests to many that the rule is represented and so undermines the very distinction we are emphasizing.
6. Our three-way distinction between descriptions is analogous to Stabler's three-way distinction between levels of computational theory (1983: 391-2).
7. In recent years Chomsky has preferred the technical term 'cognize' to 'tacitly know' (1975b: 164-5; 1980: 69-70). However, the interpretative difficulty remains. Does cognizing G require standing in a "propositional attitude" to G or is it a mere skill?
8. Stabler's arguments (1983) are very persuasive to this effect, given his plausible interpretation of the Crazy Version using the computer analogy.
9. The most revealing exchange is that between Chomsky and Harman: Harman 1967; Chomsky 1969a; Harman 1969; Chomsky 1969b. See also Chomsky 1975a.
10. We are not the first doubters, of course; see, for example, Stich 1972, Pylyshyn 1972, and Katz 1977.
11. Berwick and Weinberg call this the view that the relationship between a grammar and the parser that embodies it is *transparent*. They seem, obscurely, to hold that the relationship could be less direct, yet the parser would still realize the grammar (1984: 75-82).
12. Baker puts the requirement, somewhat naively, as follows: "The rules should be as general as possible, so that they do not make the language appear to be more complicated than it actually is" (1978: 8).
13. The same point is made by Berwick and Weinberg 1984: 94-5.
14. We are indebted to a Maryland Mafia of Norbert Hornstein, David Lightfoot and Amy Weinberg for criticisms and suggestions that led to this discussion of language acquisition.
15. See particularly, Baker 1979; Hornstein and Lightfoot 1981c; Lightfoot 1981; 1982, especially: 15-21; in press.
16. Chomsky 1981a and 1982 are detailed expositions of this approach; Chomsky 1981b is an overview. See also Wexler 1982; Berwick and Weinberg 1984, chs 1 and 5; Van Reimsdijk and Williams 1986.
17. Even here some caution is appropriate: it is not always obvious what can be gleaned from data. For example, "the child may have intonational access to major phrase boundaries" (Gleitman and Wanner 1982: 37).
18. See, e.g., Gleitman and Wanner 1982: 27, 35.
19. See, e.g., Lightfoot in press.
20. Evidence for the innate principle of subjacency is provided by Berwick and Weinberg 1984: 153-71. J. D. Fodor 1985 is a detailed criticism, to which Berwick and Weinberg 1985 is a reply.

21. Pylyshyn has pointed out some problems in using reaction times as evidence in psychology (1980: 116-19).
22. For discussions based on the psycholinguistic evidence that go mostly against the case for the psychological reality of G, see Bresnan 1978: 1-3; Anderson 1980: 382-3; Lyons 1981: 259-60; Johnson-Laird 1983: 276-95.
23. An examination of actual arguments for a grammar confirms this. See, for example, Baker 1978: 3-27; Hornstein and Lightfoot 1981c: 17-24.
24. Support for this interpretation of Chomsky comes from his explicit rejection of the view that a grammar is a model for a speaker or hearer; see, e.g., 1965: 9, 139-40.
25. See, e.g., Chomsky 1980: 189-201.
26. "If linguistics is truly a branch of psychology ..., as is often unilaterally asserted by linguists, it is so far the branch with the greatest pretensions and the fewest reliable results" (Gazdar et al 1985: 5).
27. For some other examples of the conflation, see Chomsky 1966, and Katz 1971, ch. 4.
28. Philosophers typically make a similar mistake, conflating the theory of the full semantic properties of symbols—particularly of truth conditions—with the theory of full semantic competence. Dummett is the most explicit: "a theory of meaning is a theory of understanding" (1975: 99). Philosophers often compound this mistake by taking a propositional view of competence (cf. the Crazy Version). For references and criticisms, see Devitt 1981: 92-110; 1984: 205-11; Soames 1984b.
29. Katz makes a similar point; 1977: 266.
30. Katz remarks that the structuralist approach to grammars was "insufficiently abstract" (1984a: 18). But a theory of tokens *can be* as abstract as you like.
31. We have been confronted with this objection many times in conversation.
32. See Devitt in preparation.
33. Compare: "it is possible, and arguably proper, for a linguist (*qua* linguist) to ignore matters of psychology. But it is hardly possible for a psycholinguist to ignore language" (Gazdar et al 1985: 5).
34. See, for example, Lees 1957: 36; Chomsky 1969a: 81-2; Baker 1978: 4-5.
35. Katz makes a similar distinction between the "source" and "import" of intuitions; 1977: 258.
36. We are indebted to Jerry Fodor for an objection along these lines in response to an earlier version of this paper.
37. See, for example, Labov 1972: 192-201.
38. Cf Baker, who seems to regard all intuitive judgments as simply manifestations of competence (1978: 4-5).
39. For a passage suggestive of such a slide see Baker 1978: 3.
40. On this see Leeds 1978, Field 1978, Churchland 1979, and Stich 1983.
41. We are grateful for comments received when earlier versions of this paper were read in 1985 at the University of Sydney, La Trobe University and the University of New South Wales; and in 1986 at the University of Maryland. We are also indebted to Fiona Cowie and Stephen Stich.

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