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A.B.

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CHAPTER 4

Linguistics is Not Psychology

Michael Devitt

1 Chomsky's View

The wonderfully successful research programme in linguistics initiated and sustained by Noam Chomsky starts from the assumption that a person competent in a language *knows* that language. The programme then defines the linguistic tasks in terms of this knowledge. Thus, at the beginning of a book called, appropriately enough, *Knowledge of Language* (1986), Chomsky claims that 'the three basic questions that arise' in the study of language are:

(i) What constitutes knowledge of language?
(ii) How is knowledge of language acquired?
(iii) How is knowledge of language put to use? (Chomsky 1986: 3)

In general, talk of 'knowledge' is very loose. This has led to some initial difficulty in interpreting (i)–(iii). However, there is a natural interpretation which takes Chomsky pretty much at his word.[1] On this interpretation, his answer to question (i) urges that competent speakers of a language

This paper is drawn from a book in progress, *Ignorance of Language*. It is similar to a paper delivered under that title at the Epistemology of Language conference in June 2000. The first version of the paper was delivered at King's College London in March 1997. Later versions were delivered at many universities. I am indebted to these audiences for comments. I also benefited from discussions in a seminar given by Stephen Crain, Juan Uriagereka, and myself in Fall 1997 at the University of Maryland. Most of all I am indebted to Georges Rey for many discussions of the topic over the last few years. Finally, my thanks to Gurpreet Rattan, Sara Bernal, and Philip Robbins for comments on the penultimate version.

[1] See esp. Chomsky (1986), 263–73, which includes the following: 'Knowledge of language involves (perhaps entails) standard examples of propositional knowledge' (p. 265);

have propositional knowledge of its rules.[2] This knowledge underlies the speakers' intuitive judgements about the syntax of expressions.

The key point concerning the rules of the language is that speakers stand in *a propositional attitude to representations of these rules*, albeit an unconscious or tacit one. Chomsky puts the point with characteristic firmness: 'there can be little doubt that knowing a language involves internal representation of a generative procedure' (1991*a*: 9; see also 1980*b*: 9). The term 'know' is mostly used for the propositional attitude in question but, when the chips are down, Chomsky is prepared to settle for the technical term 'cognize' (1980*a*: 69–70). The representations that are cognized are in a special faculty of the mind, 'the language faculty'. I shall call this the Representational Thesis.

The key point concerning the intuitions about particular syntactic matters is the strongly Cartesian view that speakers *derive their intuitive judgements from their representations of rules* by some process that is both causal and rational. The intuitions are, we might say, 'the voice of competence'. So, *simply* in virtue of being competent, speakers have propositional knowledge of syntactic facts; their competence gives them 'privileged access' to this reality. Because of this, these intuitions provide the main evidence about the nature of the rules.[3] This is not to say that the intuitions are infallible: performance error can lead to mistakes (Chomsky 1986: 36). Still, apart from this 'noise', intuitions reflect the underlying representations of the rules of the language.

To be competent in a language is to be able to produce and understand the expressions of that language. According to Chomsky, on our natural

'it is proper to say that a person knows that R, where R is a rule of his or her grammar' (p. 268).

[2] Recent versions of generative grammar talk of 'principles', not rules. These principles are overarching rules and their difference from what were previously called 'rules' is unimportant to my discussion (as I think Chomsky would agree: 1986: 243–4).

[3] Thus: 'It seems reasonably clear, both in principle and in many specific cases, how unconscious knowledge issues in conscious knowledge . . . it follows by computations similar to straight deduction' (Chomsky 1986: 270); 'we cognize the system of mentally-represented rules from which [linguistic] facts follow' (Chomsky 1980*b*: 9; the facts are expressed in intuitive judgments); 'We can use intuitions to confirm grammars because grammars are internally represented and actually contribute to the etiology of the speaker/hearer's intuititve judgments' (J. A. Fodor 1981: 200–1); 'Our ability to make linguistic judgments clearly follows from our knowing the languages that we know' (Larson and Segal 1995: 10; see also Baker 1995: 20).

interpretation, this competence involves representations of the rules of the language. So those representations determine what expressions the speaker produces and understands. According to the point about intuitions, those representations also determine what the speaker says *about* those expressions in her intuitive judgements.

On our interpretation, task (i) for a language comes down to the study of the system of rules that is the object of the speaker's knowledge. Chomsky calls this object an 'I-language'. Since the speaker's knowledge about this I-language constitutes her competence, task (i) is, in effect, the study of that competence. In attempting this task, the linguist produces a 'grammar', which is a theory of the I-language. That theory, hard-won by the linguist, is precisely what the speaker tacitly knows. Task (ii) is concerned with how the speaker acquires her competence. How much of her knowledge of the language is innate and how much learnt from experience? Task (iii) is concerned with the role played by this competence in performance. What role does her knowledge of the language play in understanding and producing expressions of the language?

So what, according to Chomsky, *is* a language? What is the reality that is the concern of a grammar? A language is a system of rules represented in the language faculty. Those represented rules are the reality that a grammar is theorizing about.

Chomsky is naturally interpreted as urging the Representational Thesis because doing so takes his talk of 'knowing that', 'propositional attitudes', and 'representation' at face value. The thesis is the core of what Jerry Fodor (1981) calls 'the Right View' of what a grammar is about. The thesis is certainly widespread among linguists.[4] Still, the interpretation of Chomsky may not be right. And many sympathetic to his research programme think the thesis is obviously mistaken (as I have discovered when proposing it as an interpretation).[5]

So, suppose that the interpretation is wrong. What then is Chomsky's view of the reality of a language? A language is a system of rules *embodied somehow* in the mind without being represented (just as, say, arithmetical

[4] '[M]y linguist friends tell me that learning how to talk a first language requires quite a lot of learning that the language has the grammar that it does' (J. A. Fodor 1998: 125).

[5] On this and other problems of interpretation, see Rey (2003) and his contribution to the present volume.

rules are embodied in a simple mechanical calculator without being represented). Those embodied rules are the reality that a grammar is theorizing about.

It can be seen that, according to Chomsky, the reality of a language is in the mind, whether as represented rules or as otherwise embodied rules: the reality is psychological. He has persuaded many others of this. As Robert Matthews says: 'It is a measure of the depth of the conceptual revolution wrought by Noam Chomsky in linguistics that few linguists would quarrel with his notion that theoretical linguistics is a subfield of psychology' (1991: 182).[6] So it is not surprising that Chomsky is irritated by the oft-raised question: 'Are the rules described by a grammar "psychologically real"?' (see e.g. 1980*a*: 189–201). He points out that a grammar is a scientific theory and so should be treated just like any other scientific theory. And a scientific theory should be treated realistically, for the alternative of treating it instrumentally has surely been discredited. This yields a very fast argument for the psychological reality of the rules decribed by the grammar. We have good, though not of course conclusive, evidence for a grammar's truth and so we have good evidence for the reality it concerns. And, in Chomsky's view, that reality is psychological.

Yet, on the face of it, this view of linguistics seems implausible. In any case, Kim Sterelny and I (1987; 1989) have argued against it. Jerry Katz (1981; 1984) and Scott Soames (1984) have argued independently along similar lines.[7] Our point seems simple, even rather obvious. Chomsky (1986: 34–6; 1991*b*: 31; 1995: 33–4) has responded to it briefly and dismissively. Louise Antony (2003) has responded critically to Soames in a 1991 talk that is now published in this volume. Stephen Laurence (2003) has mounted a lengthy attack in this volume.[8] Some people stopped talking to us. Beyond this, there is no evidence that our arguments have had any effect.

My aim in this paper is to argue the matter somewhat differently and,

[6] Some do quarrel, however: 'We make no claims, naturally enough, that our grammatical theory is *eo ipso* a psychological theory' (Gazdar *et al.* 1985: 5).

[7] See also related views in Cummins and Harnish (1980), Stabler (1983), and George (1989*b*).

[8] I had written this paper before receiving Laurence's. I have made several responses to his paper in the notes.

I hope, better.[9] I claim that there is something other than psychological reality for a grammar to be true of: it can be true of a *linguistic* reality.[10] One might think that this claim was uncontroversial and yet Chomsky and others seem to resist it. So I shall start by arguing for the claim carefully with the help of three quite general distinctions. I shall then argue that it is plausible to think that the grammar is indeed more or less true of that linguistic reality. Furthermore, this reality is worthy of theoretical study in its own right, whatever the case may be with psychological reality. The grammar might *also* be true of a psychological reality, of course, but to show that it is requires an explicitly *psychological* assumption. I think, but will not attempt to argue here, that it is hard to establish a psychological assumption that will do the trick. In particular, I think that there is no evidence for the Representational Thesis. If this is right, the very fast argument that we have good evidence for the psychological reality of linguistic rules because psychological reality is what grammars are about is revealed as not only fast but dirty.

How important is all this? What hangs on it? One indubitable triumph of 'generative grammar' does *not* hang on it: the extraordinary progress in providing explicit statements of the linguistic rules with the aim of deriving complete structural descriptions of all the possible sentences of a language. So I suspect that the vast majority of the day-to-day work of linguists and psycholinguists in Chomsky's research programme does not hang on it. However, the view does have great importance for the issue of the psychological reality of a language. If the view is right, the research programme is revealing a lot about language but, contrary to advertisements, rather little about the place of language in the mind (beyond the idea that, whatever that place, it may be largely innate).[11] So I think that the extent to which the rules of the language are in the mind is a fairly open question.

A person's I-language, according to Chomsky, supervenes on intrinsic properties of the person's brain: it is, as philosophers would say, 'narrow'

[9] The earlier argument now seems to me to have many errors. A preview of the present argument is to be found in Devitt and Sterelny (1999), ch. 8.

[10] Earlier (Devitt 1981: 92–5) I argued an analogous point against philosophers who identify semantics with the explanation of linguistic competence or understanding.

[11] Cowie (1998) takes a sceptical view of the innateness claim.

and individualistic. It does not involve language-world connections and so does not involve semantics proper. In effect, an I-language has only syntactic properties, in a broad sense of the term. This restriction reflects Chomsky's doubts about the scientific study of reference.[12] I do not share these doubts and so think that the object of study should not be an I-language but rather something 'wide', a 'wide-I(ed)-language': the study should include the referential properties which, together with syntactic properties, determine the truth-referential meanings of expressions. But this difference of opinion is largely beside the present issue and so, for the sake of argument, I shall go along with Chomsky's restriction to an I-language and to syntax.

2 Competence vs. Outputs

A competence is a competence to produce a certain sort of output/product; or it is a competence to process a certain sort of input; or it is both.

> *Distinction 1*
>
> Distinguish the theory of a competence from the theory of its outputs/products or inputs.

For convenience, I shall focus on competences to produce certain sorts of outputs.

I shall draw the distinction first with a simple example, distant from the concerns of linguistics: the competence of a blacksmith and the horseshoes he produces. Horseshoes are obvious parts of the physical world. A study of them will quickly conclude that they are made of iron, have a certain shape, have holes for nails, and so on. The blacksmith's competence is some state of his mind or body that plays the central role in explaining his behaviour in producing horseshoes. Goodness knows what a study of it would conclude. The key point is that the 'theory' of the horseshoes is one thing, the theory of the competence another, because horseshoes are very different from the competence to produce them. Of course, given the causal relation between the competence and the horseshoes it produces, we can expect a theory of the one to bear on a theory of the other. But manifestly this does not make the two theories the same.

[12] Related doubts are expressed in Pietroski (this volume).

With an eye to two important features of constructing a grammar, we note that there are two respects in which a theory of the outputs of a competence is not simply about the *actual* outputs of that competence. First, there can be performance errors in the exercise of a competence. Thus sometimes what a blacksmith produces is not a good horseshoe. The theory is only concerned with the nature of the outputs of a competence when it performs as it should; the theory idealizes by abstracting from error. Second, the theory is concerned with any *possible* output of the competence (when working well). Thus, the theory of horseshoes is concerned not only with the actual outputs of competent blacksmiths but with any of an indefinitely large number of outputs that they might produce.

The discussion to follow provides several other illustrations of Distinction 1.

3 Structure Rules vs. Processing Rules

The theory of a competence explains what it is about an object that makes it competent. Part of the explanation must be that the object embodies rules that govern the process of producing the appropriate output when the competence is exercised. Call these rules 'processing rules'. Sometimes the *outputs* of a competence are also rule-governed, but in a different way: their natures are constituted by their place in a 'structure' defined by a system of rules. Call these rules 'structure rules'.

> *Distinction 2*
>
> Distinguish the structure rules governing the outputs of a competence from the processing rules governing the exercise of the competence.

In characterizing the output of the blacksmith we shall not appeal to rules, but in characterizing other outputs we shall. Thus, consider the output of a chess player: chess moves. The characterization of chess moves must appeal to a rather elaborate system of rules: a bishop may only move diagonally; the king may only move one square; no piece except a knight may move through an occupied square; and so on. Chess moves are rule-governed in that something counts as a chess move at all only if it has a place in the structure defined by the rules of chess. Something counts as a particular chess move in virtue of the particular rules that govern it, in

virtue of its particular place in the structure. (This was an insight of the structuralists, of course.) A 'theory' of the nature of chess describes these structure rules.[13] In doing so it describes constraints on the appropriate output of a chess player. A chess player should only make moves that have a place in the system the structure rules describe. That is, a chess player should make only legal moves. The structure rules *may* also be among the rules governing the psychological process by which she produces chess moves. They may be among the processing rules activated in the exercise of her chess competence. However, this is not necessary and may be unlikely. In figuring out a move, the player may not actually go through processes like that of inferring '*x* moves diagonally' from '*x* is a bishop'. In any case, the key point is that *being a structure rule*—a rule governing outputs—is a very different property from *being a processing rule*, a rule governing the psychological production of outputs.

A nice example of this distinction is provided by the distinction between the *formation* and *transformation* rules of a formal logic (the latter are not to be confused with the very different transformation rules of grammar). The *formation* rules are structure rules characterizing the *wff*s (well-formed formulae) of the system: nothing counts as a *wff* unless it accords with those rules. In this way, *wff*s are rule-governed. Each *wff* has its particular syntactic structure in virtue of the particular formation rules that govern it, in virtue of its particular place in the structure defined by the system of rules. The *transformation* rules are processing rules governing the move from one *wff* to another; they govern a process of valid derivation (if the rules are good). Nothing is both a formation and a transformation rule.

Think of the formal logic as embodied in a 'logic machine'. The machine takes *wff*s as inputs, processes them according to the transformation rules, yielding *wff*s as outputs (so it embodies a proof procedure). The outputs of this machine are all in accord with the formation rules, but those rules are not the ones that govern the process of producing them.

Bees provide another good example of the distinction between structure rules and processing rules. A bee returning from a distant food source produces a 'waggle dance' on the vertical face of the honeycomb. The positioning of this dance and its pattern indicate the direction and distance

[13] An *interesting* theory of chess will describe good strategies, of course. But that is a different matter.

of the food source. These dances form a very effective symbol system governed by a surprising set of structure rules. It is the task of a theory of the dance symbols to describe these structure rules. Scientists completed this task some time ago. In contrast, the processing rules by which the bee performs this rather remarkable feat remain a mystery.[14]

Here is a description of one of the structure rules of the bee's dance:

> To convey the direction of a food source, the bee varies the angle the waggling run makes with an imaginary line running straight up and down . . . If you draw a line connecting the beehive and the food source, and another line connecting the hive and the spot on the horizon just beneath the sun, the angle formed by the two lines is the same as the angle of the waggling run to the imaginary vertical line. (Frank 1997: 82)

How *might* the bee manage this? To start with it must 'remember where the food source is' when it gets back to the hive. How? Two popular ideas are that the bee uses variations in Earth's magnetic field or in the polarization of the sun's light. A wilder idea is that the bee is sensitive to quantum fields (Frank 1997: 84). Whatever the truth of this matter, the real mystery remains: what process does the bee go through to turn this memory into an appropriate dance, a dance governed by the structure rule? We should not rush to the judgement that the structure rule itself must govern this unknown process. It may be the *wrong sort* of rule to play this role. Nature faced the design problem of adapting the pre-existing structures of an insect to produce (and respond to) the message of the bee's dance. We have no reason to suppose a priori that nature solved this problem by making the bee go through the structure rule 'calculation'. Indeed, it is not at all clear that the bee could plausibly be seen as performing this calculation: can the bee even manage the necessary representations of the food source, of the spot on the horizon, and of the angles?

With an eye to important features of grammar construction, we have noted, first, that our theory of outputs idealizes by abstracting from performance errors. So we are not concerned with the chess player's moves when he is drunk, with any 'noise' produced by the logic machine, or with the

[14] 'Scientists have known of the bee's dance for more than 70 years, and they have assembled a remarkably complete dictionary of its terms, but one fundamental question has stubbornly remained unanswered: How do they do it?' (Frank 1997: 80).

bee's dance when it is shaken off course. We have noted, second, that we are concerned not only with any actual output but with any possible output. So we are concerned with any of an indefinitely large number of *wff*s that the logic machine might produce and of dances that the bee might perform.[15] We now note, third, that we also abstract from properties of the outputs that are irrelevant to our concerns. For example, consider a collection of logic machines each embodying the same formal logic. One machine may produce a 'written' *wff* in one script, another in another script; one may produce a fast high-pitched spoken *wff*, another a slow low-pitched one. We might be interested in these differences and so distinguish these *wff*s and the competences that produce them. But we might well not be. We may be simply interested in the rule-governed syntactic structures of the *wff*s, structures shared by the outputs of all these machines. So in our theorizing we abstract from these differences.

Still with an eye to important features of grammar construction, we note, fourth, that although our theory is of the idealized output we can use it to make distinctions among the non-ideal. Moves that are not chess moves, formulae that are not well-formed, and manœuvres that are not proper bee dances can differ in their *degree* of failure. For they can differ in the sort and number of structure rules of chess, *wff*s, and bee dances that they fail, respectively, to satisfy.

4 Respecting Structure Rules

Although processing rules need not include any of the structure rules, they must, I shall say, 'respect' them.

Distinction 3

Distinguish the respecting of structure rules by processing rules from the inclusion of structure rules among processing rules.

[15] This talk may appear to commit theories of outputs to the existence of unactualized possibilia, but the talk can be, and in my view should be, a mere manner of speaking. It is a convenient way of capturing the fact that these theories, like all interesting ones, are lawlike. Strictly speaking, the theories quantify only over actual entities but the theories are, in some sense, necessary. So the talk captures the modal fact that if something *were* a horseshoe, a chess move, a *wff*, a bee's dance, or whatever, then it *would have* the properties specified by the appropriate theory of outputs. (How are we to explain modal facts? I do not know but, *pace* David Lewis, surely not in terms of unactualized possibilia.)

I have mentioned that there is a causal relation between a competence and its output. There is also a 'constitutive' relation. This arises from the fact that the *very nature* of the competence is to produce its outputs: producing them is what makes it the competence it is. Thus, the blacksmith's competence is (partly) the ability to produce horseshoes; the chess player's, to produce chess moves, things governed by the structure rules of chess; the logic machine's, to produce *wff*s, things governed by the formation rules; the bee's, to produce dances, things governed by the dance rules. So a theory of the outputs of a competence is automatically, to that extent, a contribution to the theory of the competence, for it tells us about the outputs the production of which is definitive of the competence. And we can say that a competence and its processing rules must 'respect' the nature of the appropriate output in that, performance errors aside, the processing rules must produce outputs that have that nature. Where we have to appeal to structure rules to characterize that nature, as we do with the outputs of the chess player, the logic machine, and the bee, these structure rules must be respected by the processing rules. Thus, whether or not the chess player actually goes through a process of inferring '*x* moves diagonally' from '*x* is a bishop', whatever processes she does go through must respect the structure rule that a bishop moves diagonally; any moves she makes must be in accord with that rule. And even if I am right in suggesting that the processing rules governing the bee's dancing cannot plausibly be seen as including the previously described structure rule for the direction of the food source, the processing rules must respect that structure rule in that they produce dances that are governed by it.

On the strength of the fact that these structure rules must be thus respected, it may be appropriate to say that the competent object behaves *as if* those rules were embodied in the object, but it is surely not appropriate to say *solely* on those grounds that the rules *are* embodied in it. The respecting might, of course, be the *result* of the rules being embodied; for example, the rules might also be processing rules. But the respecting alone does not require that the rules be actually realized in the speaker; for example, it does not require that they be processing rules. For there may be many other possible ways that a competence might respect the rules. So the claim that a competence and its processing rules respect the structure rules is the minimal claim on the internal-reality issue. In a sense, this claim tells

us little about the competence because it tells us nothing about *the way in which* the competence respects the structure rules. Still, we should not minimize the minimal claim. We know something quite substantial about a bee when we know that there is something-we-know-not-what within the bee that respects the structure rules of its dance. And were the respected rules richer and more complicated than those of the bee's dance, we would know something more substantial.

It follows from the minimal claim that a theory of a competence must posit processing rules that respect the structure rules of the outputs. Similarly, a theory of the outputs must posit structure rules that are respected by the competence and its processing rules. Let us capture this by saying that both theories must meet the 'Respect Constraint'.

I have remarked that a theory of the outputs of a competence must be a contribution to the theory of the competence. I think that we should go further: it seems plausible to think that the theory of a competence must *begin* with a theory of its outputs. A competence is a competence to produce certain outputs. How could we make any significant progress studying the nature of a competence until we knew a good deal about the outputs that it is supposed to produce? How could we start trying to solve the mystery of the bee's competence to dance until we knew the previously described structure rule for the direction of the food source? In brief, the theory of outputs has a certain epistemic and explanatory priority over the theory of competence.

5 Application to Linguistics

I shall now apply this discussion to linguistics, arguing that we should see grammars as primarily theories of linguistic reality, not psychological reality. In the discussion I have had an eye to certain important features of grammar construction. This was in anticipation of a certain objection to the view of linguistics I am urging. The objection is that this view cannot be right because it cannot account for those features. We shall see that it can and does.

Observing Distinction 1, we distinguish the theory of a speaker's competence in a language, a psychological state, from the theory of the outputs of that competence, sentences in the language. The construction of the former theory is Chomsky's task (i). The construction of the latter theory is a *different* task, one that I wish to promote. What can we say about it?

Like the theory of the outputs of the blacksmith, chess player, logic machine, and bee, the theory of the outputs of linguistic competence is not concerned simply with the actual outputs. It abstracts from performance error to consider outputs when the competence is working well. Thus we account for the first important feature of grammar construction. And our theory of outputs is concerned with any of an indefinitely large number of these idealized outputs that the competence might produce, with any possible output.[16] Thus we account for a second important feature. Like the theory of the outputs of the logic machine, our theory can abstract also from a range of properties of the outputs—for example, form of script and pitch of sound—focusing simply on the syntactic properties that we are interested in. Thus we account for a third important feature. The outputs of a linguistic competence, physical linguistic symbols, are governed by a system of rules, just like the outputs of the chess player, the logic machine, and the bee. Something counts as a sentence only if it has a place in the linguistic structure defined by these structure rules. Something counts as a particular sentence, has its particular syntactic structure, in virtue of the particular structure rules that govern it, in virtue of its particular place in the linguistic structure. Like the theory of the idealized outputs of the chess player, logic machine, and bee, our theory can be used to make distinctions among the non-ideal. Strings that are not sentences can differ in their *degree* of failure. For they can differ in the sort and number of linguistic structure rules that they fail to satisfy. Thus we account for a fourth important feature.

Observing Distinction 2, we distinguish these structure rules from processing rules involved in the exercise of linguistic competence. These two sorts of rules have very different roles. The processing rules produce sentences of the language in the exercise of linguistic competence. It is because those sentences are governed by the structure rules that they are indeed sentences of the language. It may be possible that a structure rule will also be a processing rule, but it is not necessary that it be.

[16] And, as with the earlier theories (n. 15), such talk need not be construed as a commitment to unactualized possibilia but rather as a way of capturing the fact that the linguistic theory is lawlike. So if something *were* a sentence, a wh-question, a passive, or whatever, it *would have* the properties specified for such items by the theory.

Finally, observing Distinction 3, we note that although the structure rules governing sentences may not be among the processing rules that govern the exercise of linguistic competence, they must be respected by the competence and its processing rules: performance errors aside, the outputs of the process must be sentences of the language and hence must be governed by the rules of the language. For, it is the very nature of the competence to produce such sentences. The claim that the structure rules of the language must be respected by the competence and its processing rules is the minimal claim on the issue of the psychological reality of language. In this sense, at least, we might say that the grammar describes 'what the competent speaker knows'. And on the strength of this minimal claim we might say that the speaker behaves *as if* those linguistic structure rules were psychologically real in her, *as if* she embodied them. But it is surely not appropriate to say *solely* on the strength of that minimal claim that those rules *are* psychologically real in her, *are* embodied, for the claim does not require that the rules be actually realized in her. In a sense, the claim tells us little about linguistic competence because it tells us nothing about *the way in which* the competence respects the linguistic rules. Still, we do know something substantial about a person when we know that there is something-we-know-not-what within her that respects the rich and complicated structure rules of a certain natural language.

Both a theory of a person's linguistic competence, of her knowledge of her language, and a theory of her linguistic outputs must meet the Respect Constraint. A theory of the competence must posit a psychological state that respects the rules governing the linguistic outputs. And a theory of the linguistic outputs must posit rules that are respected by the competence and its processing rules.

On my view, a language is composed of the outputs of a linguistic competence, symbols that are governed by a system of linguistic structure rules. That is the reality of a language. And the task we have been contemplating, and that I wish to promote, is the study of the nature of this reality. This is not Chomsky's task (i), the study of the nature of the competence itself. Indeed, at first sight the contemplated study may seem to be alien to Chomsky's enterprise. It may even seem to smack of studying an 'E-language', of which Chomsky takes a dim view: 'the concept [of an E-language] appears to play no role in the theory of language' (1986: 26); an E-language has 'no

corresponding real-world object' (1986: 27). But I rather doubt that the outputs of linguistic competence fit Chomsky's description of an E-language. According to him, an E-language is 'externalized . . . in the sense that the construct is understood independently of the properties of the mind/brain' (1986: 20). An E-language for Chomsky seems to be essentially Platonic. The outputs I have identified, physical sentence tokens governed by a system of linguistic rules, are certainly not divorced from the mind/brain since they are the symbolic outputs of the mind/brain. In studying them our *object* of study is not the mind/brain, of course, but it is likely to turn out that their properties are largely *determined* by the mind/brain. Finally, the theory of them is as much concerned with real-world objects as the theories of horseshoes, chess moves, bees' dances, and *wff*s. It is often convenient to talk of the objects posited by these theories as if they were *types* not tokens, abstract Platonic objects, but this need be nothing more than a manner of speaking: when the chips are down the objects are parts of the spatio-temporal physical world.

Here I part company with Jerrold Katz (1981; 1984; 1996). He also favours a linguistic task that is quite different from Chomsky's task (i), but the one he favours is the study of a system of Platonic objects. For him talk of sentence *types* is not a mere manner of speaking but essential to the task. He calls Chomsky's view 'conceptualism' and my sort of view 'nominalism'. He takes nominalism to have been refuted by Chomsky's criticisms of Bloomfieldian structuralism. Yet, so far as I can see, these criticisms are not of the *nominalism* of the structuralists but rather of their *taxonomic methodology*, a methodology in the spirit of positivism. According to Chomsky, this methodology imposed 'arbitrary and unwarranted' limitations on linguistics: it insisted on defining 'lower levels' before 'higher levels'; it was inductive instead of explanatory (abductive); its epistemology was localist instead of Quinean holist. Indeed, despite the explicit nominalism of the structuralists, Chomsky is prepared to take the structuralists as implicitly concerned with the psychological reality of language and hence not really nominalist at all (Chomsky 1975: 30–6).[17] Yet he still thinks his methodological criticisms stand. In any case, Chomsky's methodological criticisms

[17] In taking this line, Chomsky follows a common and effective pattern in realist philosophy of science: arguing that scientists who claim to be instrumentalists follow practices that are implicitly realist.

can be and, in my view, should be embraced by the nominalist. In particular, we should not demand that the linguistic properties of tokens be reduced to 'brute-physical' intrinsic properties of the tokens. The linguistic properties that concern us are 'high-level' relational properties.[18]

There are likely to be lingering doubts about my contemplated task. One doubt is about how the domain of study is to be determined: how do we select the tokens to be studied from all the other behavioural outputs of speakers? And the answer is: In the way science usually determines domains. That is, guided by folk linguistics, we start with an intuitive idea of the domain of grammatical tokens to be studied. We do not include many items that seem 'unacceptable' to speakers. As our linguistics goes scientific, we modify our view of the domain, accepting some strings that we had thought ungrammatical because they were, say, too hard to parse or 'meaningless'. We may even reject some strings previously thought to be grammatical. Linguistics, like other sciences, largely determines its own domain.

Another doubt arises out of attitudes to Bloomfieldian linguistics. From the generative perspective, the Bloomfieldian approach often appeared to be somewhat superficial and instrumentalist, concerned merely with describing regularities in the corpus of observed utterances rather than with the language's underlying generalizations. The generative focus on the psychological reality of language is seen as the way to avoid this instrumentalism and be a realist about linguistic theory.[19] So there may be doubts about

[18] Katz has another objection to nominalism: grammars are about an infinite number of sentences but there cannot be an infinite number of tokens. If there is a problem for my sort of nominalism, it lies in its apparent commitment to non-actual possible sentences, a problem that would arise even if we were dealing with a finite language (e.g. English with a limit of one million words to a sentence). The only significance of any apparent commitment to an infinite number of sentences is that it would *guarantee* that some were non-actual. But talk of there *being* non-actual possible outputs of a competence can be a mere manner of speaking (nn. 15 and 16). So too can talk of there *being* an infinite number of such outputs. The truth behind the talk of the non-actual can be simply that the grammar is lawlike. And the truth behind the talk of the infinite can be simply that there is no limit to the number of different sentence tokens that might be governed by the rules the grammar describes.

[19] For example, consider the following quotes and the text that surrounds them: 'On other grounds, it is difficult to explain why investigators continually found it necessary to revise and modify their procedures in the light of results that were, in some unexplained sense, "unacceptable" though in no way inconsistent with the corpus of data' (Chomsky 1975: 36); 'we are interested in linguistic analyses primarily insofar as they may be claimed to represent the knowledge speaker-hearers have of the structure of their language' (J. A. Fodor, Bever, and Garrett 1974: 40); 'The shift of focus from language itself to the native speaker's knowledge of language is the major feature of the Chomskian tradition' (Haegeman 1994: 7).

how my contemplated task can be realist about language. But, as I have emphasized, the study of linguistic tokens is not concerned only with actually observed tokens: like any other scientific theory, it is modal, concerned with any possible token. And the approach should indeed be realist, concerned with the underlying generalizations of the language. Linking language to the mind is important, of course—and I do plenty of it—but it does not require that we collapse the contemplated task into task (i). And the link to the mind is not needed for realism. We should be realist in linguistics as everywhere else in science,[20] as Chomsky has frequently insisted. But we can be realist in linguistics without taking the grammar to be true of psychological reality, but rather taking it to be true of linguistic reality: all being well, linguistic symbols really do have the properties ascribed to them by the grammar; some really are c-commanded, some really are coreferential, and so on.

Here is a more disturbing doubt. I have talked of studying the nature of a sentence token, a nature that we reach by abstracting from properties that are irrelevant to our concerns. But what are these concerns? *What is our theoretical interest in the token?* It would not be enough to argue for what Soames (1984) calls the 'conceptual distinctness' of this task from the study of competence. We have to show that the task is worthwhile. I suspect that the presupposition, often the conviction, that there is no such worthwhile task is the main reason for thinking that the linguistic task is Chomsky's (i). The view is that we need to take the task to be about competence for it to be worthwhile.[21]

Here are four reasons for thinking that my contemplated task is worthwhile. First, it must be worthwhile *if Chomsky's task (i) is*.[22] For, although we have distinguished the two tasks, we have also related them in a way that makes completing the contemplated task *necessary* for completing task (i).

[20] See Devitt (1997). I often do missionary work for realism.

[21] See Laurence (2003), sect. 5, for a vigorous argument to that effect.

[22] I owe this reason to Roblin Meeks.

For, the nature of the speaker's competence studied by task (i) *involves* the nature of the symbols studied by the contemplated task: those symbols are what the competence produces. Indeed, the contemplated task has a certain epistemic and explanatory priority over task (i). How could we make any significant progress studying the nature of competence in a language unless we already knew a good deal about that language? Just as explaining the bee's dances is a prerequisite for discovering how the bee manages to produce those dances, so also explaining the syntax of sentences is a prerequisite for explaining how speakers manage to produce those sentences.

A second reason for thinking that my contemplated task is worthwhile is that analogous ones are. This may not seem so obvious with the horseshoe, chess, and the logic machine, but it is surely obvious with the bee's dance. A serious researcher spent years 'cracking the code' of this dance, working out how it indicates the direction and distance of the food source. And his findings were certainly interesting to scientists.[23] The study of human language must surely be more worthwhile and interesting than the study of the bee's.

A third reason for thinking the task worthwhile would be that substantial and interesting theories are fulfilling the task. In the next section I shall argue that generative grammars are such theories.

The fourth and most important reason starts from the intuition that our concern with sentence tokens, as with bees' dances, is with their *meanings*. This is a widely held view[24] but it is unsatisfactorily vague. I have argued elsewhere that we should be concerned with the properties of sentence tokens that enable them to play certain striking roles in our lives, including the role of informing us about reality; these are the 'meanings' of tokens (1996: 2.3–2.8). Analogously, the properties of bees' dances that concern us are the ones that enable them to play their role of indicating food sources. Sentence tokens have their meanings partly in virtue of their syntactic

[23] 'Von Frisch's *Dance Language and Orientation of Bees* was some four decades in the making. By the time his papers on the bee dance were collected and published in 1965, there was scarcely an entomologist in the world who hadn't been both intrigued and frustrated by his findings. Intrigued because the phenomenon Von Frisch described was so startlingly complex; frustrated because no one had a clue as to how bees managed the trick' (Frank 1997: 82).

[24] Randy Harris calls the definition of linguistics as 'the study of the links between sound and meaning' 'one that virtually all linguists would agree to' (1993: 5).

properties and partly in virtue of the meanings of their words. So, accepting the restriction to syntax for the sake of argument, the nature of the sentence token that we need to explain is made up of the syntactic properties in virtue of which the token can play those striking roles.

Our first reason seemed to make our theoretical interest in the contemplated task dependent on our theoretical interest in Chomsky's task (i). On the basis of our fourth reason, I shall soon argue for the opposite dependency (Section 7.4).

Much more needs to be said about the theoretical interest of studying linguistic symbols than I can attempt to say here. I think that this interest does indeed arise out of our interest in the mind, in particular from our interest in thoughts and their role in explaining behaviour (1996: 2.5).[25] But, once again, this does not make our study psychological: in particular, it does not turn it into task (i), the study of competence.

Is my contemplated task appropriately characterized as nominalistic? It takes all the objects that linguistics is about to be concrete tokens, and so to that extent it is nominalistic. Where it stands ultimately on the nominalism issue depends, of course, on what we make of its ascription of meaning *properties* to those objects. However, it seems unlikely that the nominalist would have any *special* difficulty paraphrasing away this property talk. My contemplated task for linguistics is likely to be as nominalistic as tasks in physics, biology, or economics.

6 The Contemplated Task and the Linguistic Enterprise

Whether or not this study of the outputs of competence is the study of an E-language in Chomsky's sense, and whatever the case about the psychological reality of languages, I want to argue that there is nothing alien to the linguist's enterprise in the contemplated task.

First, these actual and possible idealized outputs, governed by a system

[25] It is this theoretical interest that is likely to make a grammarian of English as concerned with the outputs of Laurence's Martians as with our own outputs. And it will prevent her concern from spreading to the outputs of parrots, tape recorders, and the like (Devitt and Sterelny 1999: 145), a spread that Laurence argues is a likely consequence of not taking the Chomskian view (2003: sect. 5).

of rules and fitting into a structure, *are* what we would normally call a language. Indeed, wherever there is a linguistic competence there *has* to be such a language, for the language is what the competence produces: the language is what the speaker is competent *in*; it is definitive of the nature of the competence.

Second, we note that Chomsky himself often describes his task in ways that suggest it is the one we have been contemplating. For example, consider the following from the early pages of *Syntactic Structures*: '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; see also 1980*a*: 222).

Third, prima facie, a great deal of the work that linguists do, day by day, in constructing a grammar is studying a language in the nominalistic sense I have described.[26] Work on phrase structure, case theory, anaphora, and so on, talk of 'nouns', 'verb phrases', 'c-command', and so on, all appear to be concerned, quite straightforwardly, with *the properties of expressions* in a language, symbols that are the outputs of a competence. This work and talk seems to be concerned with items like the very words on this page. And, we have already noted, four important features of grammar construction are also part of the contemplated study: the idealization of outputs; concern with all possible outputs; abstraction from irrelevant properties; the making of distinctions among the non-ideal.

Fourth, the linguistic evidence adduced for a grammar bears directly on a theory of the language in my sense; evidence about which strings of words are grammatical; about the ambiguity of certain sentences; about 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.

Objection: 'But this so-called "linguistic" evidence is largely the intuitions of the native speaker. These arise from her underlying competence. So the evidence bears directly on task (i), not your task.' Now it is indeed true that if the speaker's knowledge of her language consists in her representation of its rules and if her intuitions are derived from those representations by a causal and rational process, then those intuitions are direct evidence for task (i) because they are direct evidence of what rules are represented. I would argue that this view of the intuitions is mistaken; that the intuitions are theory-laden opinions resulting from ordinary empirical investigation, just like any intuitions.[27] But, whatever the truth of this matter, the main point now is that the intuitions are direct evidence about language in my sense, provided that we have good reason to think that they are accurate. It does not matter to this point whether we think that they are accurate because they are derived from representations of the rules or for some other reason. If they are accurate they are evidence about language because language is what they are about: they are about the grammaticality, ambiguity, etc. of linguistic *symbols or expressions*. So if the intuitions are indeed derived from a representation of linguistic rules, then they will be direct evidence for *both* task (i) and my contemplated task. If, on the other hand, as I think, they are not so derived but are none the less generally accurate, then they will still be direct evidence for my task even if only indirect evidence for task (i).

Fifth, the *psycholinguistic* evidence about language perception and acquisition, offered to support the view that a grammar is psychologically real,

[26] Or in Katz's Platonic sense, which can be taken as simply a convenient manner of speaking of language in my sense (sect. 5).

[27] Devitt (1996), 2.10–2.11; Devitt and Sterelny (1999), 8.6. Laurence (2003) goes badly astray in responding to our earlier discussion of linguistic intuitions (1989: 520–3). The central thesis of our paper is that linguistic theory is about the properties of linguistic symbols. Yet Laurence argues that our view of linguistic intuitions, together with what he calls our 'Methodological Principle' for deciding what linguistics is about, should yield the view that linguistics is really about the folk theory of linguistics (above, pp. 89–91). This is presumably intended as some sort of *reductio* of our position. The main problem with his argument is that the view of intuitions he attributes to us is clearly not one we hold. He attributes the view that these intuitions constitute the 'predominant' evidence for a linguistic theory. Yet, in discussing the evidence we never single out the intuitions as predominant. Quite the contrary. We single out *the linguistic symbols we produce and react to* as the main evidence. And we say twice that 'strictly speaking' these symbols, not our intuitions about them, are the evidence (pp. 520, 523). Aside from this puzzling misrepresentation, it is surely obvious that Laurence's argument is a misapplication of our Methodological Principle. The argument's conclusion that linguistic theory is about folk linguistics is, in effect, the instrumentalist view that the theory's task is simply to capture folk intuitions. J. A. Fodor calls this view 'the Wrong View'. We set it aside at the beginning of our paper (p. 498) before even introducing the Methodological Principle. And later, when we discuss realism and instrumentalism as general approaches to science, we endorse realism in no uncertain terms: 'Sydney realism is the most virulent known strain' (p. 511). The Methodological Principle is a principle for choosing among *realistically construed* theories and so could not possibly yield an instrumentalist conclusion.

bears directly on a theory of the language in my sense. Thus, concerning perception, evidence that speakers are sensitive to a proposed syntactic property in parsing an expression is evidence that the expression really has that property, for it is evidence that their competence respects the structure rules that determine that property. The right theory of a language must ascribe rules to the language that competent speakers of the language respect: the Respect Constraint. In this way, the psycholinguistic evidence bears directly on our theory of the linguistic reality.[28] And, concerning acquisition, evidence about nature and nurture showing that a language with a certain structure could not be learnt by a person is direct evidence against any theory that ascribes such a structure to a language that has been learnt by the person.[29]

In the light of responses to a related point that Soames made about evidence (1984), I should guard against possible misunderstandings.

(*a*) I am making the empirical claim that, as a matter of fact, the linguistic and psycholinguistic evidence bears directly on a theory of language in my nominalistic sense (whatever its bearing on anything else). This sort of claim about the bearing of evidence on a theory is a familiar part of science and ordinary life. The claim is *not* an attempt to impose a priori restrictions on the domain of evidence relevant to Chomsky's task (i) or to my contemplated task (cf. J. A. Fodor 1981: 199–200; Chomsky 1986: 34–6; 1995: 33–4; Antony 2003; Laurence 2003: 101–4). I go along with the Duhem–Quine thesis which allows, roughly, that anything might be

[28] Cf. 'A parser which is well-attuned to the competence grammar can be a source of information about the properties of the grammar' (J. D. Fodor 1989: 174). My point is that the parser *has* to be a source of information for the grammar because it has to be sufficiently well attuned to assign the right syntactic structures, performance errors aside. Of course, on the received assumption that the grammar is psychologically real and applied in parsing, evidence about parsing will obviously be seen as bearing on the grammar; see e.g. Chomsky (1980*a*), 200–1; Berwick and Weinberg (1984), 35. My point is that the evidence bears on the grammar even without the assumption.

[29] Laurence (2003: sect. 5) names one of my earlier arguments (Devitt and Sterelny 1989: 514) 'the Martian Argument' and takes it 'to question whether *in principle* [psycholinguistic] data are even *relevant* to the evaluation of linguistic theories' (above, p. 95). I doubt that I ever questioned this but I obviously do not question it now. One of the two advantages that Laurence claims for the Chomskian view of linguistics over its rivals is that it brings psycholinguistic data to bear on linguistic theory. The Chomskian view does not have this advantage over the view I am urging.

evidence for anything. But it is clearly not a consequence of that thesis that a piece of evidence bears with equal directness on all theories. It is not a consequence, for example, that the experience of green grass bears equally on the theory that grass is green and the theory that echidnas have spikes.

(*b*) I am not claiming that the linguistic evidence mentioned in my fourth point *is* irrelevant to task (i). Indeed, since the processing rules of linguistic competence must respect the structure rules, any direct evidence about the structure rules must to that extent bear on task (i). For the same reason, the psycholinguistic evidence mentioned in my fifth point must also bear on task (i) to that extent. Of course, we hope that this evidence will bear on task (i) to a much greater extent, throwing light on *the way in which* competence respects the structure rules. However, I would argue that the psycholinguistic evidence now available does not in fact throw much light on this matter and gives no support to the view that competence respects the structure rules by representing them. So it gives no support to the Representational Thesis.[30]

In brief, my evidential point is simply that evidence that has played a big role in linguistic and psycholinguistic theorizing bears directly on the task that I have distinguished from Chomsky's task (i), whether or not that evidence, or any other evidence, bears on task (i). And my general point

[30] The second of the two advantages that Laurence (2003) claims for the Chomskian view of linguistics over its rivals is that it confers *explanatory power* on linguistic theory, in particular the power to explain language use and acquisition. (1) On the view I am urging, the power of a linguistic theory is to be found primarily in its explanation of the properties of linguistic tokens. (2) Still, the theory does contribute to the explanation of language use and acquisition because competence must respect the linguistic rules ascribed by the theory. So use and acquisition phenomena that would be predictable if those rules were the ones respected—for example, the phenomena Laurence describes (sect. 2)—are indeed partly explained by a theory that ascribes those rules. (3) Of course, the theory would make a greater contribution to the explanation of use and acquisition were it the case that competence respected the linguistic rules by representing them. I have just doubted that there is psycholinguistic evidence to support this thesis (or even the more modest thesis that competence respects the rules by embodying them without representing them). But the point to be made in response to Laurence is: if the psycholinguistic evidence were ultimately to support the thesis, thus expanding the explanatory power of linguistic theory, this expansion would not count against the view of linguistics I am urging. Rather, the expansion would be welcomed as an explanatory bonus: the theory not only explains language, it plays a larger role in the explanation of language use and acquisition than we had any reason to expect.

is that linguists appear to be studying, partly at least, a language in my nominalistic sense.

Sixth and finally, the appearance that linguists are studying language in this sense is just what we should expect *given Chomsky's assumption (on the natural interpretation) that the competence that is the concern of task* (i) *is knowledge of the language, involving the representation of its rules; i.e. given 'the Representational Thesis'* (Section 1). For, *the language that would be thus known and represented would be the very same language that is the output of the competence*. Chomsky assumes that competence consists in knowledge about the I-language. The point I am emphasizing is that this very I-language is, indeed *must be*, at the appropriate level of abstraction, the output of that very competence. So, *given Chomsky's assumption*, task (i) requires just the same study as we have been contemplating. So it is no surprise to find Chomsky moving straight from an account of the task like the one quoted to the following version of task (i):

> 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)

Given the assumption of the Representational Thesis, task (i) and the contemplated task are much the same.[31] At one and the same time we study the symbolic system that is the output of the competence and the competence itself which is a representation of that very system.

If this is so, the contemplated task is not open to objection from Chomsky. Given his assumption, it is a task that must be performed in performing his task (i). The contemplated task acknowledges the link between competence and language but differs from task (i) in being neutral about the precise psychological nature of that competence.

Not only must Chomsky accept the contemplated task, we should all accept it. A competence is a competence to do something. So whenever there is a competence to investigate there is also a product of that competence to investigate. When the output is a language, it should go without saying

[31] A similar assumption yields a similar conflation in some philosophers of language; hence Michael Dummett's slogan 'a theory of meaning is a theory of understanding' (1975: 99).

that its investigation is theoretically interesting. Still, we can say why it is and I have started to do so in the last section.

7 Four Methodological Points

If this discussion is right, it has a great deal of methodological significance. I have pointed out (Section 1) that Chomsky is irritated by the issue of the psychological reality of language. For him the only issue here is the truth of the grammar: if the grammar is true then of course it is true of psychological reality because that is what the grammar is about.

7.1 *First methodological point*

There is something theoretically interesting for a grammar to be true about other than the internal reality of speakers, just as there is something theoretically interesting for a theory of chess moves, *wff*s, or bees' dances to be true about other than the internal reality of chess players, logic machines, or bees. The grammar might be true about a symbolic system, a *linguistic* reality. The claim that 'the language has no objective existence apart from its mental representation' is false (Chomsky 1972: 169). So we can take the grammar realistically without taking it to be true of *psychological* reality. Furthermore, given the weight of evidence adduced for a grammar, it is plausible that it *is* (more or less) true of linguistic reality.

7.2 *Second methodological point*

The view that a grammar has any more to do with psychological reality than the amount allowed by the minimal claim requires a powerful psychological assumption about competence, if not Chomsky's assumption then one of similar strength. Without such an assumption, the grammar simply concerns a language system. This system is the output of something psychological but it remains to be argued that it is itself psychological.

Of course, this does nothing to show that the grammar is *not* true of the psychological reality, that the rules of the language are *not* actually realized in the speaker. The point is that whether the grammar is true of psychological reality is a *further* question to its being true of the linguistic reality. Settling that further question depends on settling the truth of a

powerful psychological assumption. *The psychological reality of language is not something 'you get for nothing' with the truth of the grammar.* I fuss about this because I think it is hard to find evidence for a psychological assumption that will do the trick.[32]

7.3 *Third methodological point*

According to the Respect Constraint, a theory of competence in a language must posit processing rules for perception and production that respect the structure rules of the language. And the grammar must posit structure rules that are respected by the competence and its processing rules. So, the Respect Constraint makes the justification of the grammar partly dependent on the justification of the theory of competence, and vice versa. Beyond that, however, *the grammar and the theory of competence are independent of each other.* So there should be no a priori demand that an acceptable grammar must meet some further constraint concerning psychological reality, e.g. what Robert Berwick and Amy Weinberg call 'transparency' (1984: 38). And a grammar should not be dismissed—as, for example, transformational grammars were by Joan Bresnan and Ronald Kaplan (1982)—for failing to meet such further constraints. A grammar that attributes rules that are respected by users of the language may be a perfectly adequate theory of the language however little it can be incorporated into a model of language use. Nor, in setting out to examine psychological reality, should we look to the grammar for any insights beyond those arising from the Respect Constraint. So far as the grammar is concerned, 'the psychological cards can fall where they may', subject only to the Respect Constraint. Similarly, the theory of the bee's dance, including the previously described theory of how the dance indicates the direction of the food source (Section 3), provides no help to the theory of the bee's competence to dance beyond that arising from the fact that the competence must respect the rules of the dance.

This bears on a popular criticism of the claim that a grammar's rules are

[32] In Devitt and Sterelny (1989) my case for the thesis that a grammar is about linguistic reality rested heavily on the view that it was very likely not true of psychological reality. This is what Laurence (2003: sect. 4) criticizes as 'the Methodological Argument'. I am still doubtful that the grammar is true of psychological reality but my present case for the thesis does not rest on that doubt.

psychologically real.[33] The criticism is that we lack evidence as to *which* grammar's rules are psychologically real: 'If we can come up with one grammar for a language, we can come up with many which, though they posit different syntactic rules, are equivalent in their explanation of meaning: they are equally able to capture all the syntactically determined facts about meaning. We need psycholinguistic evidence to show which grammar's rules are in fact playing the role in linguistic processing, evidence we do not have.' This criticism is not quite right. We need evidence that the rules of *any* grammar are processing rules. These rules may simply be the *wrong sort of rules* to be processing rules, just as the rules of the bee's dance very likely are, and the rules of the logic machine's language certainly are. Suppose, as seems quite likely, that the human language capacity is an adaptation. Then nature faced the problem of designing this capacity out of pre-existing structures in our ancestors. We should not think, in advance of empirical discovery, that nature solved this problem by making humans go through processes governed by linguistic rules. We should not suppose a priori that a correct account of the linguistic reality will describe the psychological reality. A grammar may have nothing more to do with psychological reality than comes from its meeting the Respect Constraint.

So it was a mistake to assume that psycholinguistic evidence would decide which of many meaning-equivalent grammars was true of psychological reality: perhaps none of them are. But something interesting remains of the criticism: we need psycholinguistic evidence to decide which of many meaning-equivalent grammars are true of *linguistic* reality. For, we need the psycholinguistic evidence to tell us which grammar meets the Respect Constraint, which one posits rules that are respected by the competence and its processing rules. The syntactic properties determined by rules that are respected are the ones that linguistic tokens really have.

7.4 *Fourth methodological point*

We have noted (Section 5) that a grammar as a theory of a language has a certain epistemic and explanatory priority over a theory of the psychological reality underlying language. We cannot make any significant progress studying competence in a language until we know a good deal about that

[33] Devitt and Sterelny (1989) is an example. The criticism is related to what Laurence (2003: sect. 5) calls 'the Martian Argument'.

language. So it is appropriate that, from the start, much of the work in generative grammar has been directly concerned more with the linguistic than with the psychological reality.[34]

I think that we can go further. Our *theoretical interest* in explaining competence in a language surely starts from our theoretical interest in that language. Think of the bee once more. Were it not for our interest in the nature of the bee's dance, we would never have become interested in the state that manages to produce that dance: it is because that state produces something so theoretically interesting that the state itself is so theoretically interesting. I think that the same goes for the state that produces language. If so our theoretical interest in a language is prior to our interest in its psychological reality.

Earlier (Section 5) I suggested that our theoretical interest in language arises from our interest in thoughts. I said almost nothing to support this suggestion but suppose that it is right. Now put it together with what I have just claimed. We have the following 'direction of theoretical interest': from thoughts to language to linguistic competence. This order of interest does not, of course, undermine the relative independence of the theories of these three realities.

8 Interesting Psychological Matters

I trust, then, that it is obvious that I am *not* suggesting that the psychological reality underlying language is unworthy of study. Indeed, the theoretical interest in a language leads immediately to an interest in two matters psychological. (i) It is not enough to know that there is something-we-know-not-what within a speaker that respects the rules of her language, any more than it is enough to know that there is something-we-know-not-what within a bee that respects the rules of the bee's dance. We would like to go beyond these minimal claims to discover the ways in which the competence of the speaker, and the competence of the bee, respect these rules.[35] But,

[34] Cf. '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 1981: 7); '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).

[35] Hence the frustration of entomologists mentioned in n. 23.

in studying these matters, to emphasize my third methodological point, it is a mistake to insist on finding or even to expect to find, embodied in the organism, processing rules that are also structure rules of its outputs. The processing rules and structure rules have very different jobs to do. We should keep a totally open mind about how the organism manages to respect the structure rules.

(ii) The language a person is competent in has one structure and not another. We should like to know *why* the person speaks such a language, why the something-we-know-not-what that she embodies respects the structure rules of that language and not other structure rules.[36] The bee's competence to dance is surely innate. To what extent is this also true of a person's linguistic competence and to what extent is that competence the result of the person's environment?

Our interest in language will surely also lead us in the end to an interest in a very different psychological matter. (iii) It is impossible to give deep explanations of linguistic reality without appeal to the psychological: very likely, psychological facts together with environmental facts determine linguistic facts. So in the end we shall need to study the psychological in order to explain the linguistic. But in the beginning we do not. Syntactic investigations of *being c-commanded*, *being doubly embedded*, and the like, the sort of investigations that linguists do every day, are not psychological. Even when, in the end, we have to appeal to psychology to explain in virtue of what tokens have these properties, the object of our study remains linguistic. Analogously, a study of the property of the bee's dance that indicates the direction of the food source is not a study of the bee's 'psychology' even if the explanation of in virtue of what the dance has that property appeals to inner states of the bee. A linguistic symbol, like a bee's dance or a horseshoe, really has its properties whatever the explanation of its having them. The symbol objectively exists with its properties 'apart from its mental representation'.[37]

[36] Cf. 'we want to know why there are these social regularities and not others, or why we consider these abstract mathematical structures and not others. Surely the facts might be otherwise' (Chomsky 1980c: 57).

[37] At one point Laurence suggests that 'someone with broadly Chomskian sympathies' might accept that 'linguistics *is* about symbols' and yet still maintain that 'it is, in the first instance, about competence'. She can do this because: 'the important properties of these symbols—the properties in virtue of which symbols have their linguistic properties—are

9 Conclusion

Linguistics has something worthwhile to study apart from the psychological reality of speakers: it can study a linguistic reality. This reality is in fact being studied by linguists in grammar construction. The study of this linguistic reality has a certain priority over the study of psychological reality.

The truth of a grammar for a language leaves the question of the psychological reality of the language open. To close the question we need to look for other evidence, especially to psycholinguistic evidence about language production, perception, and acquisition. I think that this evidence will leave the Representational Thesis unsupported and implausible. And it will make it hard to choose among a range of other positions on the question of psychological reality.

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properties pertaining to our linguistic competence, and perhaps aspects of how these symbols are processed in language comprehension and production. . . . The important issue is not over whether linguistics is about symbols but over the nature of the facts which determine the linguistic properties of symbols' (above, pp. 87–8). There are two errors here. The first is to think that because symbols have their properties in virtue of certain facts then the theory of symbols is about those facts. If this were generalized, then every theory—economic, psychological, biological, etc.—would be about physical facts because physical facts ultimately determine everything. A special science does not lose its own domain because that domain supervenes on another. The second error is to assume that the determining facts for linguistic properties are facts about linguistic competence rather than other psychological facts and environmental facts. I do not know of any evidence for this assumption and it seems to me almost certainly false (Devitt and Sterelny 1999: chs. 7–8).

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