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The "Linguistic Conception" of Grammars

1. INTRODUCTION

The received Chomskian view is that a grammar is about a mental "organ", the speaker's language faculty. A less charged way of putting this is that it is about the speaker's linguistic competence. On this view, linguistics is clearly part of psychology. So, let us call this "the psychological conception" of grammars. My book, *Ignorance of Language* (Devitt 2006a),[1] starts by arguing that this conception is wrong. Instead, I urge "the linguistic conception" according to which a grammar is about a nonpsychological realm of linguistic expressions, physical entities forming symbolic or representational systems (ch. 2; see also Devitt 2003).

The linguistic conception, indeed the book as a whole, has received several relentlessly unsympathetic criticisms from Chomskians (Antony 2008, Collins 2006, 2007, 2008a,b, Pietroski 2008, Slezak 2007, 2009, Smith 2006).[2] But these criticisms badly misrepresent my views and mostly fail to address the actual arguments (Devitt 2006b, 2007, 2008a,b,c, 2009).

The present paper is a shortened version of a one (2008b) stimulated by John Collins (2007, 2008a,b) and Georges Rey (2008). It aims to strengthen the case for the linguistic conception. I start with a sketch of the book's argument for that conception.

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[1] All unidentified references to my work in this paper are to this book. Devitt and Sterelny (1989) is an earlier version of the argument, but contains many errors.

[2] In contrast, Guy Longworth has the irenic view that there is really no disagreement between the psychological and linguistic conceptions: they "might be notational variants, or different determinations of a single determinable view" (2009: 34). This is an entertaining idea but, I argue, very wrong (Devitt 2009).

2. SKETCH OF THE ARGUMENT IN *IGNORANCE*

The argument rests on the application of the following three general distinctions to humans and their language. The distinctions are (Devitt 2006a: 17–23):

1. Distinguish the theory of a competence from the theory of its outputs/products or inputs.[3]

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

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

I give several illustrations of these distinctions, but my favorite involves the honey bee. Thus, distinction 1 is illustrated by the difference between the theory of the bee's "waggle dance" that indicates the direction and distance of a food source and the theory of the bee's competence to produce that dance. Distinction 2 is illustrated by the difference between the structure rules of the dance, a representational system discovered by Karl von Frisch, and the largely unknown processing rules by which bees produce the dance. Distinction 3 introduces my technical term "respect": the bee's state of competence, and the embodied processing rules that constitute it, must "respect" the structure rules of the dance in that they are apt to produce dances that are governed by those rules. But this is not to say that those rules are included among those processing rules.

Simply on the strength of von Frisch's theory we know this minimal proposition about any competent bee: that there is something-we-know-not-what within the bee that respects the structure rules that von Frisch discovered. But what we don't know is *what* there is in the bee that does this job. To move beyond the minimal claim and discover *the way in which* the bee's competence respects the structure rules of the dance, we need evidence beyond anything discovered by von Frisch, evidence about the bee's "psychology".

A theory of a competence and a theory of its outputs are different, but it follows from this discussion that they must both meet what I call "the Respect Constraint": "a theory of a competence must posit processing rules that respect the structure rules of the outputs"; "a theory of the outputs must posit structure rules that are respected by the competence and its processing rules" (Devitt 2006a: 23).

I take the discussion sketched so far to establish:

(A) There are the general distinctions 1 to 3.

The discussion then turns to linguistics:

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[3] For convenience I focus on the competence to produce certain outputs.

(B)　　These distinctions apply to humans and their languages (Devitt 2006a: 23–30).

(i) Just as the theory of the representational system that is the bee's dance is one thing, the theory of the bee's competence to produce the dance, another, so also is the theory of the representational system that is a human language one thing, the theory of the speaker's competence to produce it another. We need a theory analogous to von Frisch's to explain the nature of the human representational system, a theory that will be even more interesting that von Frisch's. (ii) How would such a theory help with the theory of competence in that language? It would tell us that there is some-thing-we-know-not-what within any competent speaker that respects the structure rules it describes (Respect Constraint).[4] This is the minimal position on psychological reality that I later call "(M)" (Devitt 2006a: 57). But the theory of the language provides nothing more about the mind than (M): it does not tell us *what* there is in the speaker that does the respecting. In particular, we don't know whether any of the theory's rules are embodied some way or other in the mind and so are also part of the psychological reality that produces language. To move beyond the minimal claim and discover *the way in which* a speaker respects the grammar's rules, we need further psychological evidence of actual *processing*. Finally, I argue that a grammar, produced by linguists, is a theory of the representational system that is a human language:[5]

(C)　　A grammar is a theory of the nature of the system that is a language, not of the psychological reality of that language in its competent speakers (beyond the minimal (M)) (Devitt 2006a: 30–38).

I take the linguistic conception of grammars to be the view that a grammar is a theory of the nature of the system that constitutes a language, and the psychological conception to be the view that a grammar is a theory of the psychological reality of a language in its competent speakers (beyond (M)). It then follows trivially from (C) that:

(D)　　The linguistic conception is true and the psychological one false.

The truth of the grammar of a language entails that its rules govern linguistic reality, giving a rich picture of this reality. In contrast, the truth of the grammar does not entail that its rules govern the psychological reality of speakers competent in the language and it alone gives a relatively impoverished picture of that reality.

Let me conclude this sketch by emphasizing that the linguistic conception does *not* involve the absurd claim that psychological facts have nothing to do with lin-

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[4] I apply 'rule' to syntax not with its technical sense in linguistics but with a broader sense covering principles (Devitt 2006a: 3, n. 1).

[5] The grammar would not be a *complete* theory of the language (even if finished): a grammar is a theory of the syntax of a language (broadly construed) and would need to be supplemented by a theory of the word-world connections that constitute word meanings. I mostly ignore this complication (Devitt 2006a: 14–15).

guistic facts. Some psychological facts *cause* linguistic facts (Devitt 2006a: 23–4), some "respect" them (2006a: 25), some *partly constitute* them (2006a: 39–40, 132–133, 155–157), some *provide evidence* for them (2006a: 32–34), and some make them *theoretically interesting* (2006a: 30, 134–135). But psychological facts are not the *subject matter* of grammars. The dispute is not over whether linguistics relates to psychology but over the way it does.

3. STRENGTHENING THE CASE

Three assumptions are important to the linguistic conception. (1) There is a nonpsychological realm of linguistic expressions, physical entities forming symbolic or representational systems. This is "realism" about linguistic entities. (2) Grammars give more or less true accounts of the natures of these representational systems. (3) Grammars, as accounts of these natures, are theoretically interesting.

My focus will be on assumption (1). For, if (1) is true and linguistic realism holds, then it is irresistible to suppose that grammars are more or less true of that reality and so (2) is true. And if both (1) and (2) are true then, contrary to what Collins claims (2007: 420; 2008a: 25–28), it is fairly easy to argue for (3). And my argument that concludes as follows seems to me to do the job well enough.

> Language is an extraordinarily effective way of making the thoughts of others accessible to us, thoughts that otherwise would be largely inaccessible; and of making our thoughts accessible to others, often in the hope of changing their thoughts and hence their behavior. So we have a great theoretical interest in explaining the properties of linguistic expressions, including their syntactic properties, that enable the expressions to play this striking role (Devitt 2006a: 134).

So let us consider (1). And let us return to the bee. Von Frisch's hypothesis that the bee's dance is a representational system was not initially so plausible, and it had its skeptics, but the overwhelming consensus now is that it is true. Of course, the idea that animals have representational systems to communicate with one another is familiar. Most such systems are, however, considerably less interesting than the bee's, partly because they simply communicate information about the animal's own current state; for example, that the animal is hungry, or wants a mate. However, bees are certainly not unique in having a system that communicates information about the external world; for example, Gunnison's prairie dogs convey information about which sort of predator is threatening and about the characteristics of a particular predator of that sort (Slobodchikoff 2002). And, famously, dolphins and various primates have been *taught* rudimentary languages based on ours.

In hypothesizing that a certain behavior involves a symbol that represents something, we are supposing that the behavior was produced *because*, in some sense, it involves that symbol representing something; and it is *because* of what the symbol represents that other members of the species respond to the behavior as they do. Nobody thinks, of course, that all behaviors of an organism involve representations. The

point is that the best explanation of *some* behaviors takes them to involve representations. And in such a case *the explanation of the cause of the behavior is dependent on the explanation of the nature of the representation*. But it is important to see that these explanations are distinct: Von Frisch explained the nature of the waggle dance but nobody has yet explained the bees' dancing.

Wherever some of the outputs of a community of organisms form a representational system it is appropriate to ask in virtue of what those outputs have their representational properties. Consider the case of the bee's dance, for example:

> 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).

What makes it the case that, given the position of that spot on the horizon, the particular angle of a dance represents the direction of the food source? Presumably, that answer must appeal to what is innate in the bee. Similarly, with the representational systems of many birds. I'm told, however, that the appeal will sometimes be partly to what is conventional in a community of birds. And with human languages, although the appeal may be partly to what is innate, the oft-noted arbitrariness of language shows that the appeal must be largely to what is conventional in a community. So it would be bad news for my linguistic realism if there were not the conventions this requires. Chomsky (1996: 47–48) has indeed claimed that there are few linguistic conventions and that such conventions as there are do not have "any interesting bearing on the theory of meaning or knowledge of language". I responded, arguing that there are many conventions and that they are important in language acquisition and use (Devitt 2006a: 178–184).

Now it is true that in my initial presentation of the argument for the linguistic conception I rather took the realist assumption (1) for granted. The hypothesis that a great number of the sounds and inscriptions that humans produce do constitute representational systems seemed to me *much more* plausible than any of the widely accepted analogous ones about mammals, birds and bees. I embrace the common view that the human capacity to produce these extraordinarily sophisticated representational systems is central to our triumph as a species. Still, I did note the antirealist view that the sounds and inscriptions we produce that are commonly thought to have linguistic properties do not really have them and hence are not really linguistic expressions. I described that view, I think generously, as "curious" (Devitt 2006a: 27). And later in the book I developed the case for realism in the just-mentioned discussion of conventions. I went on (2006a: 184–192) to argue against the antirealism that had been urged by Georges Rey (2006a). There were several critical responses (Rey 2006b, Collins 2006, Smith 2006) to which I have responded (2006b: 597–605). Rey

has recently made another contribution, "A Defense of Folieism" (2008). I shall conclude this paper with some further discussion of Rey's antirealism.

4. REY'S ANTIREALISM

What does Rey have to say on the two questions that are central to realist assumption (1): (I) Why would anyone think that "SLEs" — Rey's shorthand for "standard linguistic expressions" — do not exist? (II) Why is it important to suppose that they do exist?

(I)(a) The focus of Rey's antirealism is on phonology. Indeed, it often looks as if his whole case that there are no SLEs rests on sounds not really being phonemes. I have already made some objections to his argument for this view of phonemes (Devitt 2006a: 186; 2006b: 599). Here is one more. Rey claims that phonemes are "*perceptual* inexistents" like Kanizsa triangles, objects that appear to be triangles but aren't really (2006b: 558). Yet, so far as I can see, he never produces an argument that they are such illusions. We need, at least, some argument that the sounds that are associated in complicated ways with a phoneme should be described antirealistically as *mere cues* for the phoneme rather than realistically as various *instantiations of* the phoneme. Here are two problems he would need to address. (1) I assume that hearing a sound as a certain phoneme is largely innate and so, to that extent, it is just like seeing a certain Kanizsa figure as a triangle. But to that extent it is also just like seeing a certain object as red. And whereas the Kanizsa figure is not really a triangle, the object is really red. Or so thinks the neo-Lockean about secondary qualities. So we need an argument that phonemes are like illusions not these secondary qualities. (2) The great variation between languages in the association of sounds and phonemes shows that phonemes are not entirely innate: they are partly conventional. In this respect they are clearly quite unlike Kanizsa triangles.

(I)(b) Suppose that Rey were right and phonemes did not exist. A language that has phonemes can, and often does, have other forms: inscriptions, Morse, hand signs, flags, etc. How would the inexistence of phonemes cast any doubt on other forms of the language? Rey's antirealism is largely based on the complicated facts of phonology, facts that have led to shelves of books and to a large subdivision in linguistics.[6] These facts could hardly cast any doubt on the existence of graphemes, for example: they could hardly show that the shapes that we take to be letters of the alphabet are not *really* letters of the alphabet. Antirealism about phonemes would not generalize.

Rey may think otherwise because he seems to think graphemes are also perceptual illusions: he seems to think that the *real* alphabet is like the *Kanizsa* alphabet (2008: 191). This is a mistake: Kanizsa letters, like Kanizsa triangles, are plausibly taken to be illusions, but real letters are not. Consider Kanizsa triangles: an "incomplete" figure that is "not really a triangle" but appears to us as a triangle. There is of course a

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[6] For a nice summary of the vexed problems of phonology, see Burton-Roberts, Carr and Docherty (2000).

real figure there with certain superficial properties which our innate perceptual system leads us to "complete," thus seeing it as having superficial properties that it does not in fact have. Since anything that had those latter properties would be a triangle, we see the figure as a triangle. Similarly with a Kanizsa letter: we "complete" an inscription that has a certain shape, P1, so that we see it as having another shape, P2, and hence as being, say, the letter 'A'; it is plausible to say that we see an existent P1-object as an inexistent P2-object and hence as an 'A'. *But in virtue of what do we see a P2-object, whether an existent one that really has the shape of a P-2 object or an inexistent one that merely appears to have that shape, as the letter 'A'?* What makes a P2-object that particular grapheme? An appeal to illusions is no help in answering. We need to appeal to conventions. Whereas hearing a sound as a certain phoneme may be only a little bit the result of participating in a convention, seeing a P2-object as a certain grapheme is entirely so. The convention is something we are taught in school, sometimes painstakingly (think Japanese). A consequence of this is that any *actual* P2-object is a grapheme by convention. How then could graphemes not exist? Graphemes, and their analogues in other nonphonological forms of a language, *obviously* exist. Rey seems to disagree. In which case he needs a mighty powerful argument, an argument he can't find by looking to the science of phonology.

So even if the sounds of a language lacked phonological properties, this would not show that the inscriptions of a language lacked graphemic properties. More seriously, it *does not begin* to show that these sounds and inscriptions lack syntactic and semantic properties. Why not?

Well suppose that phonemes are really mental entities. That does not reflect on the reality of sounds, of course. Now, take any phoneme. Some sorts of sounds, but not others, stand in an important relation to that phoneme. We might describe that relation in various ways: as the relation of being *apt to cause* the phoneme; of being *cues* for it; of *seeming to be* it; or whatever. Call that relation *R*. Now think of the series of phonemes, S, that constitute /cat/. There is a convention in English of using a series of sounds that stand in relation *R* to S to represent cats (to express cat-thoughts) and to be a noun. As a result those sounds really have the semantic property of representing cats and the syntactic property of being a noun (just as indefinitely many sorts of entities have the property of being a vote). So far as semantic-syntactic realism about Rey's SLEs is concerned, it makes no difference whether the regularities that lead to the conventions involve sounds that *are* phonemes or sounds that *R* phonemes.

In sum, *not only would the argument for antirealism about phonemes not generalize to other forms of language it would not generalize to syntax and semantics.* There is no case for antirealism here.

(II). Why does Rey think that it is not important to suppose that SLEs exist? He thinks that "there's no explanatory reason for those properties to be instantiated in the acoustic stream" (Rey 2008: 186). Yet surely we need to posit real SLEs to explain communication. Or so I have argued (Devitt 2006a: 179–86; 2006b; 2008a). Rey (2008: 177) disagrees:

communication…is a kind of *folie à deux* in which speakers and hearers enjoy a stable and in-
nocuous illusion of producing and hearing […] "SLE"s […] that are seldom if ever actually
produced. "They" are what Franz Brentano called "intentional inexistents," "things" that we
represent and think of as "out there," but which do not exist.

There is an obvious problem: What is the explanation of the *stability* of these
"*folie* illusions"? How does the message understood come to match so reliably the
message intended? Rey seems to leave unexplained, even miraculous, what we need
a conventional language to explain. But Rey (2006b: 558) has an ingenious response:
an intentional inexistent, hence an SLE, can enter into a convention even though it
doesn't exist.

As noted, Rey thinks that phonemes are *perceptual* inexistents. And he thinks
that we can find conventions involving these inexistents by looking to stable *per-
ceptual illusions* (Rey 2008: 191). *But what Rey has not shown is how we could get
from his alleged stable perceptual illusions involving inexistents to the syntactic and
semantic conventions we need to explain his alleged folie illusion.* Furthermore, I have
argued (Devitt 2008a) that any attempt to show this will collapse into linguistic realism:
any convention involving his alleged inexistents will be one involving real SLEs.

I have one final thought in favor of linguistic realism. I have pointed out that sci-
entists frequently hypothesize that a species has a representational system which its
members use to communicate with each other. The scientists then go on to theorize
about the nature of the representations. What precisely do the representations *mean*?
Think, for example, of studies of the dances of bees and of the barks of prairie dogs.
Sometimes, most notably with representational systems that we have *taught* to dol-
phins and primates, scientists suppose that symbols have their meanings partly in
virtue of having a rudimentary syntax. Now, of course, any of these hypotheses
might be wrong. Still, many of them are widely accepted. And the important thing to
note is that *these hypotheses are all committed to analogues of what we have been
calling "linguistic realism".* The scientists are supposing that these animals are pro-
ducing behaviors that *really do* involve representations having semantic and some-
times syntactic properties. And "a Martian scientist" would surely think at least as
much about our behaviors. Indeed, he would think that our representational systems
are distinguished from the others in being vastly more sophisticated syntactically and
semantically. What he surely would not think is that we, unlike the other animals, are
failing to produce representations that effect communication; that although we are
under the illusion of producing these, we are not really doing so; that whereas we
have succeeded in teaching systems to primates and dolphins that have a rudimentary
syntax, we have not succeeded in producing one ourselves that has a sophisticated
syntax. He surely would not think this because it is rather preposterous.[7]

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[7] An early version of the paper on which this is based (2008a) was given at the third annual
Dubrovnik conference on the philosophy of linguistics held in September 2007. Versions of this paper
were delivered at: University of Szczecin (Poland), April 2008; the annual conference of the Society

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