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Explanation and Reality in Linguistics

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This paper defends some anti-Chomskian themes in Ignorance of Language (Devitt 2006a) from the criticisms of John Collins (2007, 2008a) and Georges Rey (2008). It argues that there is a linguistic reality external to the mind and that it is theoretically interesting to study it. If there is this reality, we have good reason to think that grammars are more or less true of it. So, 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. For, all we learn about that reality from the grammar is that it "respects" the rules of the grammar.

Key words: linguistic reality, psychological reality, linguistic conception of grammar, antirealism, "folie illusions"

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 linguistics. My book, *Ignorance of Language* (2006a),[1] starts by arguing that this conception is wrong. Instead, I urge "the linguistic conception" according to which a grammar is about a non-psychological realm of linguistic expressions, physical entities forming symbolic or representational systems (ch. 2; see also 2003).

John Collins (2007) is not amused. In a lengthy, entertaining, but relentlessly unsympathetic, review of *Ignorance* he is particularly incensed that I should have the temerity to urge the linguistic conception. My position is denounced for "metaphysical glibness" (417), having "no empiri-

[1] All unidentified references to my work in this paper are to this book.

cal content" and being "in the thrall of folk linguistics" (418). Indeed, Collins gives the impression that I am against peace, motherhood, and certainly the Chomskian way. He thinks it's time for a jihad.

In the face of this, I should begin by affirming that I am *for* peace and motherhood and *only a little bit against* the Chomskian way. Most importantly, contrary to what Collins and others (Pietroski 2007; Slezak 2007) suggest, I am not against the research strategy of producing generative grammars in pretty much the way they are being produced.[2] I am indeed skeptical of some Chomskian claims *about the mind*. But the main purpose of my book is not to criticize Chomsky but to see what, at this point, we can reasonably conclude about the psychological reality underlying language (15–16). And my view, briefly, is: not very much.

The argument for the linguistic conception and against the psychological one rests on the application of three quite general distinctions to humans and their languages. So what we look for in a defense of the conception is criticism of these distinctions and/or their application. We don't find much of that in Collins' review. What we do find is a lot about the virtues of Chomskian linguistics, about points that "Chomsky has repeatedly made" (2007, 418), and so forth. But these matters are not in contention. We don't find much aimed at what *is* in contention. The review barely engages with my actual argument. Collins' much longer discussion in a paper in this issue, "Knowledge of Language Redux" (2008a), does a bit better but not much.[3] Indeed, my position remains almost totally incomprehensible to Collins, despite several agreeable exchanges in which we attempted a meeting of minds. Perhaps we are faced with an example of Kuhnian incommensurability.

In the next section I shall sketch my argument. In section 3 I shall consider Collins' discussion of the psychological conception and in section 4, his discussion of the linguistic conception. The key issue between us is over the viability of the linguistic conception. And, in the end, that issue seems to come down to the issue of "linguistic realism," the issue of whether or not there *is* a nonpsychological realm of linguistic expressions forming a representational system. So, this discussion leads naturally to the discussion in section 5 of Rey's antirealism; see his "In Defense of Folieism," in this issue.

[2] Collins makes more false accusations than I could readily answer. These include, (a), that I resist "the demarcation of a domain of inquiry", what he calls "linguistic cognition" and I call "linguistic competence" (2007, 418). Most of my book is about this domain. And they include, (b), that I make "*ex cathedra* stipulations of disciplinary boundaries" (*ibid.*). What linguists *do* does indeed determine the domain of linguistics, as Collins insists. That's one thing. *Opinions about* what domain has been so determined is another. I *argue* for my opinion—see section 2 below—I don't stipulate it.

[3] Similarly Smith 2006 and Slezak 2007 largely ignore my actual argument. Devitt 2006b is partly a response to Smith. Devitt 2007 is partly a response to Slezak 2007, partly to Collins 2007. (The present paper draws on that response to Collins.) However, forthcoming papers by Collins (2008b) and Smith (2008) will address my argument in some detail.

2. *My Argument for the Linguistic Conception*

The argument for the linguistic and against the psychological conception of linguistics is to be found in chapter 2 of the book and is summarized in "Defending *Ignorance of Language*: Responses to the Dubrovnik Papers" (2006b, sec. 2). In this section I shall present the argument more briefly but also more formally.

The three general distinctions on which the argument is based (17–23) are as follows:

1. Distinguish the theory of a competence from the theory of its outputs/products or inputs.[4]
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."[5]

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

[5] Smith delivers himself into my hands by asserting, with astounding confidence, that von Frisch's hypothesis "is highly controversial and has very little empirical credibility these days" (2006, 440). He does this without apparently bothering to check the evidence to the contrary that I cite (20, n. 3). More importantly, he misunderstands the place of the bee's dance in the dialectic; see my response (2006b, 585–7). Collins does too: "In the face of the last fifty years of generative linguistics, Devitt's presumption that the study of insect *behaviour* offers a default model of how linguistics should proceed is truly bizarre" (418). There is no such presumption or model. (i) The bee is used to illustrate distinctions 1–3. That's all. And it is not the

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" (23).

I take the discussion sketched so far to have established:

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

The discussion then turns to linguistics:

(B) These distinctions apply to humans and their languages. (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 expressions that constitute the human language. (ii) How would such a theory help with the theory of competence in that language? It would tell us that there is something-we-know-not-what within any competent speaker that respects the structure rules it describes (Respect Constraint).[6] This is the minimal position on psychological reality that I later call "(M)" (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 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. Finally, I argue that a grammar, produced by linguists, is a theory of the representational system that is a human language:[7]

(C) A grammar is a theory of the nature of the expressions that constitute a language, not of the psychological reality of that language in its competent speakers (beyond the minimal (M)). (30–8)

I take the linguistic conception of linguistics to be the view that a grammar is a theory of the nature of the expressions that constitute a lan-

behavior of the bee that matters to this illustration but the *representational system* that the behavior produces. (ii) The book offers no model at all of "how linguistics should proceed"; *a fortiori* it does not offer one using bees as a model. (iii) The bee case is part of an argument about the *subject matter* of linguistics not about its *methodology*.

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

[7] 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 (14–15).

guage, 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.

Let me conclude this discussion of the linguistic conception by emphasizing that the linguistic conception does *not* involve the absurd claim that psychological facts have nothing to do with linguistic facts. Some psychological facts *cause* linguistic facts (23–4), some "*respect*" them (25), some *partly constitute* them (39–40, 132–3, 155–7), some *provide evidence* for them (32–4), and some make them *theoretically interesting* (30, 134–5). 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. *Collins' on the Psychological Conception*

One way to reject (D) would be to defend the robust psychological conception of linguistics that I have defined from my criticisms. And the negative tone of Collins' response to those criticisms may give the impression that this is what he is doing.[8] Yet, in fact, Collins does not defend that conception. Rather, he defends an anemic psychological conception. This conception's view of what the grammar tells us about the mind does not differ much from mine. His criticisms of my discussion of the psychological conception are largely not on this substantive issue but rather on an interpretative one: he thinks I am wrong to attribute the robust conception to Chomskians. In this section, I shall start with the interpretative issue and then move to the substantive one. In section 4 I shall discuss Collins' main disagreement with (D), his vigorous rejection of the linguistic conception.

3.1 *The Interpretative Issue:* Do Chomskians have the robust psychological conception? Collins is very critical of what he claims are my mistaken views of what Chomskians think grammars tells us about the mind.

> The charge against Chomsky and many others, then, is that, one way or another, they illicitly take structure rules to be processing rules without any good "explicitly *psychological*" evidence. It is difficult to think of anyone who answers to Devitt's charge; Chomsky is certainly innocent. (2007, 417)

[8] Thus he begins his response in the paper by finding the "dialectical presumptions… odd" (2008a, 21) in my claiming that that "if the psychological conception is to be saved, there has to be something wrong" with the argument for (D) just summarized (2006b, 576–7). He says: "Topics of inquiry do not need to be 'saved'" (2008a, 21). But it is not the psychological *topic*, that of linguistic competence, which has to be saved (and which is indeed the main topic of my book), but the psychological *conception* that grammars tell us more than (M) about that topic.

Two comments. (1) I didn't put "the charge" quite like that. What I actually said in the context of proposing (B) and (C) was:

> there should be no a priori demand that an acceptable grammar must meet some [constraint beyond the Respect Constraint] concerning psychological reality, for example 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. And we should not decide which linguistic theory is right, as Janet Fodor suggests we might, "by considering the sorts of parsing procedures that each linguistic theory implies" (1989: 181). (36)

(2) Collins' dismissive response to the alleged charge is blatantly revisionist history. The long and complicated story of views of the relationship between grammars and theories of language use demands something much more nuanced. Thus, in the beginning there was the unpromising idea that transformation rules are run backward in parsing (Miller and Chomsky 1963). By the 1970s, this view had fallen out of favor (Fodor, Bever and Garrett 1974), although it was still being thought of as a "natural" first guess in the 1980s (Berwick and Weinberg 1984, 39). I'd say that the view that structure rules were processing rules was alive, even if not so well, into the 80s. And even in 1990s, the belief that grammatical rules played a central role in language use remained (Pylyshyn 1991, 232).[9]

Collins continues on from this shaky start as follows: "Chomsky has *never* thought that linguistic theory is about psychological processing" (2007, 417). Now, as Collins knows, I am well aware of this and of Chomsky's famous distinction between competence and performance. So what is bothering him? I am alleged to be "in the thrall of a terribly restrictive view of psychology" (2008a, ##11) "an a priori conception of psychology as dealing solely with processing" (24; see also 2007, 417–8). I gather that my mistake is thought to be reflected in my use of 'linguistic competence'.[10] Collins sees me as applying 'linguistic competence' simply to the state (not a process, mind) that embodies the language processing rules that are center-stage in performance,[11] whereas Chomsky thinks of it as applying to a *knowledge system* in a language faculty, a system that is *utilized by* the processing system but *independent* of it. Now, I actually construe 'linguistic competence' more broadly than Collins supposes (see, for example, 57–8, 89–90). But we need not argue this terminological matter because it is beside the point.[12] And there is

[9] See chapters 4 and 11 for my attempts to do justice to the history to this point and after.

[10] This became much clearer to me in a typically spirited but very helpful email exchange with Collins and Smith. This has led to many changes in my thinking and I am grateful to them both.

[11] Performance must also involve some "pragmatic" abilities; in comprehension, for example, that of removing ambiguities (233–5). And there is likely to be "noise" in any actual performance.

[12] Some *other* points in my book do depend somewhat on my view that linguistic competence is a piece of knowledge-*how* and hence in the same family as skills and

nothing restrictive or a priori about my conception of psychology. For, my conclusion is that, beyond (M), the grammar alone tells us *nothing at all* about "the psychological reality underlying language" (9), nothing at all about whatever language-specific psychological state or states any competent person must be in, *whatever one calls 'competence'*. Similarly von Frisch's theory alone tells us nothing at all, beyond an analogue of (M), about the "psychological" reality underlying the competent bee's dancing. Simply on the strength of the fact that the language processing rules must respect the structure rules described by a grammar, we *could* perhaps say that knowledge of those structure rules is "implicit" *in the processing rules* (c.f. 25); similarly we *could* perhaps say that knowledge of the dance rules is "implicit" *in the bee's processing rules* (c.f. 22–3). But it seems to me much better if we do *not* adopt this very unhelpful way of talking. And the important point is that the grammar of a language does not alone give any support at all to the idea that the structure rules are *embodied in some knowledge system* of those competent in the language or, indeed, that the competent even *have* a linguistic knowledge system in a language faculty distinct from the processing system. Any such robust idea of psychological reality requires the support of some powerful assumption other than the grammar (35–6).[13] That is why the psychological conception is wrong. And that conception is what I *really* take to be the standard Chomskian view of what the grammar tells us about the mind: that the grammar's rules are not merely respected by the mind but actually in the mind in some way, playing a causal role.

Collins does not have this robust conception, as we shall now see, but he writes as if no Chomskian ever has, indeed as if it is outrageous of me to suggest that any has. I give a great deal of evidence, some briefly described above, that many have.[14] Indeed, it seems to me *obvious* that, *at the very least*, *many appeared*, and *many still appear*, to have the robust conception.[15] If Collins thinks I am wrong about this he needs more than pronouncements: he needs to address the evidence. Interpretation is an empirical matter.

abilities. Chomsky has some objections to this view. I argue that the objections are unsuccessful (92–3).

[13] I consider one version of the robust idea—that "the structure rules of the language are represented and used as data by the processing rules of language use" (57)—and argue that it lacks any significant support and is implausible; that is entailed by my second major conclusion (272). But this version of the idea has no more appeal to Collins than it does to Smith (2006) and Slezak (2007). Another version of the idea might be that the structure rules used as data in language processing are embodied *but not represented*. I did not consider this version because it strikes me as psychologically incoherent, for reasons similar to those against the view that intuitive linguistic judgments might be derived from embodied but unrepresented rules (117–19).

[14] The evidence is mostly to be found in the discussion of my "third methodological point", 36–7, 62–84, 196. For evidence of the waning influence of the conception in psycholinguistics, see 230–41.

[15] Rey agrees. And he also agrees that the grammar alone does not support the robust view of psychological reality. (2008, ##17)

3.2 *The Substantive Issue:* Turn now to Collins view of the psychological conception. If Collins were to defend this conception he would have to take the grammar to show us more than (M) about the competent speaker. Yet, he actually thinks it shows us less! Collins says that the grammar is a theory of "the function computed" by the mind/brain (2007, 418). I have discussed this familiar idea before (66–7; 2006b, 579–80) arguing that, insofar as it is right, it amounts only to something that is not in contention: that the grammar yields position (M). Collins agrees, but with an important qualification: "there is indeed a respect condition, but it is internal…not deferential to anything external" (2007, 417; see also 2008a, 22). So what the grammar tells you about the mind of a competent speaker is not more than (M) but rather (M) stripped of the idea that the structure rules that are respected govern anything external to the mind.

Two comments. First, if the *linguistic* conception is right—see sections 4 and 5 below— and there is an external linguistic reality explained by the grammar, then there is no motivation for the stripping.[16]

Second, and much more important, Collins' view that the grammar shows us less than (M) manifestly will not save the psychological conception I have defined and taken to be the standard Chomskian view. It does not entail that any of the syntactic rules posited by the grammar, rules like the following, govern psychological reality:

> An anaphor must be bound by another expression in its governing category.
> A pronoun must not be bound by another expression in its governing category.
> Accusative case is assigned by a governing verb or preposition.
> A verb which fails to assign accusative case fails to theta-mark an external argument.
> Movement cannot cross more than one bounding mode.

According to Collins, the grammar gives us reason to believe that these sorts of rules are respected by the mind, in his internalist sense, but no reason to believe that they are in the mind.

So where do Collins and I disagree over what the grammar alone tells us about the mind?[17] One disagreement is clear: on my view the grammar tells us only (M) whereas on his it tells us only "internalist (M)", an even weaker view. I think that there is another significant disagreement. I take him to think that his internalist respecting is located

[16] Collins has this to say about my externalist respecting: "For sure, one could say that the mind/brain respected the [external] structure, but then we would be owed an account of respect: what are the mind/brain structures such that the external structures are respected?" (2008a, 23) This is a good question but why is it put as if it were an objection? The point of my talk of externalist respecting is to capture the very little that grammars tell us about the mind. This does of course raise Collins' question. And most of my book is devoted to trying to answer it. And his internalist respecting raises a precisely analogous question.

[17] I have recently argued (2006b, 577–80) that my disagreement with Bob Matthews (2006) on this matter is small, perhaps nothing but rhetoric.

in a language faculty independent of the language processing system (2008a, 22, 25). In my view, the grammar gives no support to that idea. Still, these disagreements are far less than one would suppose from the tone of Collins' discussion.[18]

Why is Collins content with such an anemic view of what grammars tell us about the mind, much less than Chomskians have standardly supposed? I assume the answer is that he rejects the linguistic conception and this leaves nothing else for the grammar to be about.

4. *Collins' Critique of the Linguistic Conception*

Collins is vehemently opposed to the linguistic conception. This really is the key issue between us.

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.

A point of clarification. Strictly speaking, (3) is not part of the linguistic conception as defined because that definition makes no mention of theoretical interest. Still, obviously, a grammar conceived of as a theory of linguistic reality must *be* theoretically interesting if the linguistic conception is to be taken seriously. And I argue that a grammar thus conceived *is* theoretically interesting (28–30, 134–5).

Turn now to Collins. Although he does not mount a sustained attack on (1), he clearly rejects it. And he does offer some considerations which he takes to count against it. He does not discuss (2), presumably thinking, rightly, that if (1) is false then (2) must be. In any case, Collins is most concerned to reject (3): he thinks that even if there were a nonpsychological linguistic reality that grammars were more or less true of, that wouldn't be *theoretically interesting*. That, in a nutshell, is his main problem with the linguistic conception.

So I shall start by discussing (3) in section 4.1. And for the purposes of this discussion, *I am going to take (1) and (2) for granted*, take for granted that there really is a linguistic reality that grammars are more or less

[18] Thus, after rightly pointing out that linguistics will provide an account of the function computed by a speaker and that a theory of processing must be informed by this account, he claims to "genuinely fail to understand" why I have "persistently claimed that such considerations are not enough to justify claims of psychological reality" (2008a, 24). But I have not persistently claimed this at all. The considerations in question are, near enough, (M) and what I have persistently claimed is that linguistics will not justify any view of psychological reality *stronger than (M)*. I emphasize that the truth of a grammar does straightforwardly yield the psychological conclusion that the linguistic competence in question respects the structure rules posited by the grammar. But that is the only psychological conclusion it yields. One wonders why Collins manufactures disagreements with me. There are surely enough real ones to keep him busy.

true about. For, if I can establish (3) on that basis, and (1) and (2) can be established, then clearly the linguistic conception will be established.[19] So, after that discussion I must turn to (1) and (2). I shall consider Collins' objections to (1) in section 4.2 and Rey's objections to it in section 5.

4.1 *Theoretical Interest and Assumption (3):* Collins thinks that anything a grammar tells us about a linguistic reality outside the mind lacks theoretical interest:

> we talk of external concreta having linguistic properties, for sure…but all that is irrelevant to linguistics qua an empirical science. The only interesting question is whether linguistic properties so construed enter into theoretical explanation. (2007, 419)

And Collins is convinced that they do not: "external factors are just not salient to current scientific inquiry" (420). Grammatical claims explain cognitive phenomena like the way speakers *construe* expressions, not the expressions themselves:

> In the sentence *Bob's brother loves himself* neither *Bob* nor *brother* c-command *himself*, which in part explains why English speakers construe the reflexive to be referentially dependent on the whole DP rather than *Bob* or *brother* alone…absent the human mind/brain, that *Bob's brother* c-commands *himself* is no more interesting a property of our example sentence (understood as an inscription) than that the pairs <h, h> and <e, e> are cross-serial…The only conceivable reason to pick out c-command is that it, as opposed to an indefinite number of other properties, enters into an *explanation* of human cognition (e.g., judgements of referential dependence). (420)

Collins is not mostly concerned to argue against a linguistic reality because such a reality is *irrelevant*, it is "noumenal" (2008a, 24). This characteristically sweeping conclusion "is not a *proof* that there is not a linguistic reality; it is only intended to demonstrate the irrelevance of the idea to current linguistic thought and any other conceivable science" (2007, 420).

I have two points to make about Collins' rejection of (3): (a) it is almost entirely unargued; (b) it is false.

(a) So what is Collins' argument against (3)? In the context of discussing this issue of theoretical interest, he has this to say:

> syntax can be realised by more or less anything one likes (for starters, consider the set of conceivable orthographies, hand gestures, and acoustic signals within the human frequency band). But the rub here is that there is no unity to this heterogeny save for that provided by the human mind/brain. (420)

But the response is obvious: what unifies these various physical entities and makes them the object of linguistic inquiry is that *they all have linguistic properties*. The study of those properties is interesting and that's why linguistics is interesting.

[19] I think that a case can be made that the linguistic conception should survive even if (1) were wrong but I shall not attempt to make the case here.

One detects several other strains of thought in Collins on this issue but none that constitute an argument.

(i) Collins draws attention to some interesting psychological matters that he thinks grammars explain. But that is of course quite compatible with their also explaining interesting linguistic matters. Grammars include many claims like those displayed in section 3.2 about anaphors, pronouns, and the like. When such theoretical claims are applied to particular linguistic expressions they yield claims like "*Bob's brother* c-commands *himself*" that help explain the nature of *Bob's brother loves himself*. Syntax texts are full of such theories and applications, explaining the nature of linguistic expressions. These explanations can then be *used* to explain cognitive phenomena—see below—but, as they stand, that is not what they are doing. At least, that is what they are doing assuming, as we are for the moment, (1) and (2). What we need from Collins is an argument that these explanations of the nature of linguistic expressions are not theoretically interesting

(ii) Rather than argue, Collins is content with repeated *ex cathedra* pronouncements that the *only* interesting explanations in linguistics are of cognitive matters. He writes as if one can simply *read this off* from the practice of linguists:

> it is an empirical question, not one for philosophy, whether environmental factors are theoretically salient to linguistic inquiry. On the face of it, they appear to be quite distant. (2008a, 23–24)

On the contrary, I think that explanations like that in (i) are on the face of them not of cognitive matters. Chomskians do, of course, *say* that they are investigating cognitive matters, as Collins insists: "Generative linguistics is explicit about its domain of inquiry—linguistic cognition" (2008a, 23). But I am *arguing* that the *practice* of grammar construction, despite what Chomskians (but not all linguists) *say about it*, is primarily one of constructing interesting theories of linguistic reality.

(iii) Collins may feel no need to argue that the explanation of linguistic reality *is not* interesting because he thinks the onus is on me to show that it *is*. This emerges in his discussion of a line that is repeated time and again by defenders of the psychological conception:[20] that the linguistic properties of expressions supervene solely on psychological facts and so the study of those properties must be part of psychology. This "supervention defense" is *entirely erroneous*, as I have emphasized elsewhere (40; 2006b, 582–4). And if it were not, all the sciences—economics, psychology, biology, etc.—would be parts of physics. Now Collins insists that he is not making the supervention defense (2007, 419; 2008a, 24). Yet he claims:

> What does determine [theoretical] interest…is the locus of determination of this thing we call language. Since Devitt readily acknowledges that the determining load is carried by the mind/brain, the burden is on Devitt to show how looking beyond might be of theoretical interest. (2008a, 24)

[20] Consider, for example, Dwyer and Pietroski (1996, 350), Smith (2001, 284; 2006, 441–2), Laurence (2003, 87–8), and Rattan (2006, 508).

But this doesn't begin to follow. The "determining load" in economics is also "carried by the mind/brain" yet there is no special burden on the economist to show how looking beyond the mind/brain is of theoretical interest in economics. The determining load in biology is carried ultimately by physical facts but the biologist has no special burden to show how the study of living things is theoretically interesting. Despite Collins' denial, it seems to me that, when push comes to shove, he implicitly falls back on something close to the supervention defense.

(iv) Another clue to Collins' failure to argue may comes from his noting—I think rightly—that the linguistic conception is the folk conception. To be identified with folk opinion is to stand condemned in Collins's eyes: "It is a *virtue* of a science that it leaves behind our folksy conception" (2008a, 27 n. 37); 'folksy' is probably his favorite term of abuse. I have emphasized elsewhere how folk theory is often wrong and always incomplete (Devitt and Sterelny 1999) but I think Collins view here is way over the toI follow Quine in thinking that science is continuous with commonsense. Combining this with the epistemic virtue of conservatism (Quine and Ullian 1970) yields the conclusion that it is a virtue of a theory to be in *accord* with folk theory. It goes without saying, of course, that this virtue is and should be often overridden by other virtues. Collins owes us an argument that the virtue should be overridden in the case of the linguistic conception.

So much for the considerations that Collins seems to adduce *against* (3), against the view that the task of studying the nature of linguistic expressions is theoretically interesting and worthwhile. I turn now to my argument *for* (3), an argument that Collins simply ignores.

(b) My argument comes in two parts. Here is my summary of the first part before I presented the second part:

> First, [the task] must be worthwhile if the study of linguistic competence… is worthwhile because that study involves my task. Indeed, my task has a certain epistemic and explanatory priority over the study of competence. Second, I noted the interest of an analogous task, explaining the code of the bee's dance. Third, I claimed that substantial and interesting theories—generative grammars—are fulfilling the task. Fourth, and most important, I claimed that the properties of tokens that the task studies—meanings, hence the syntactic properties that partly constitute meanings—play striking roles in our lives. (134)

In the second part, I develop this last reason, concluding:

> 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. And just as our interest in the properties of the bees' dance leads to an interest in the bees' competence to produce dances so also does our interest in linguistic expressions lead an interest in our competence to produce them. We have the following "direction of theoretical interest": from thoughts to language to linguistic competence. (134–5)

So I have no problem with the idea that our interest in a theory of the nature of linguistic reality stems from our interest in the mind. Indeed, if our interest in the mind requires that theory then *of course* the theory is interesting!

One can only imagine the scorn that Collins has for these ideas. But scorn is not an argument. It follows from my argument that his *ex cathedra* pronouncements about what grammars explain are false.

We are currently assuming (1) and (2): grammars explain the nature of an external linguistic reality. I have one more comment on the theoretical interest of these explanations. Collins emphasizes the role of grammars in explaining cognitive phenomena. I am all for such explanations, although I think that good ones are mostly hard to come by (just as they are for the bee). But the important thing is that, given (1) and (2), the goodness of such an explanation *depends on the grammar being true of linguistic reality*. It is *because* the grammar gives a good explanation of the symbols that speakers produce that it can contribute to the explanation of the cognitive phenomena. Take Collins' example of c-command. Why do certain people construe the reflexive in *Bob's brother loves himself* as referentially dependent on the whole DP rather than on *Bob* or *brother* alone? Answer: (i) those people are competent in English and hence "respect" its rules;[21] (ii) *Bob's brother loves himself* is an English sentence in which *himself* is c-commanded by the whole DP but not by either of its parts; (iii) it is a rule of English that, in these circumstances, the reflexive must be bound by the whole DThis cognitive explanation depends on (ii) and (iii) providing a good linguistic explanation of the nature of that English sentence. *In general, English speakers construe English expressions as if they had certain properties because, as the grammar explains, the expressions really have those properties.*

In conclusion, assuming (1) and (2), Collins claim that it is not theoretically interesting to study the nature of linguistic reality is unsupported and obviously false. If there is a nonpsychological realm of linguistic expressions, physical entities forming symbolic or representational systems, and grammars are describing this realm, then it is clearly interesting to theorize about. So, the linguistic conception rests on assumptions (1) and (2). We shall now consider them.

4.2 *Linguistic Reality and Assumptions (1) and (2):* I noted that Collins does not discuss (2), presumably thinking, rightly, that if (1) is false and there is no "external" linguistic reality—"linguistic antirealism"—then the idea that grammars are more or less true of this reality must be false too. I observe now that 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. So all the attention should be on (1).

[21] More precisely, the people are competent in and respect idiolects that all have the properties picked out as those of English in (ii) and (iii) (180–4).

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.[22] Of course, the idea that animals have representational systems to communicate with one another is familiar.[23] 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 the 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 arbi-

[22] See Devitt 2006b, 585–6, for information about this consensus.

[23] See, for example, Bekoff and Jamieson 1996, chs 16–19, and Bekoff *et al.* 2002, part III.

trariness 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 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" (1996, 47–8). I responded, arguing that there are many conventions and that they are important in language acquisition and use (178–84).

Now it is true that in my initial presentation of the argument for the linguistic conception in chapter 2 (and 2003), I rather took the realist assumption (1) for granted. As I noted later,

> I do indeed make the common assumption that there are semantically significant links between words and the world; between, say, 'George W. Bush' and a certain individual, between 'tiger' and certain animals, between 'snow is white' and a certain state of affairs, and so on for other SLEs [standard linguistic entities]. (2006b, 601)

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 (SLEs). I described that view, I think generously, as "curious" (27). And later in the book I developed the case for realism in the discussion of conventions that I have just mentioned. I went on (184–92) to argue against the antirealism that had been urged by Georges Rey (2006a). In a recent issue of this journal, Rey (2006b) responded at some length, Collins (2006) and Barry Smith (2006), more briefly. I responded in the same issue (2006b, 597–605), addressing two questions that are central to realist assumption (1): (I) Why would anyone think that SLEs do not exist? (II) Why is it important to suppose that they do exist?

Collins has some further things to say on the matter. I start with some novel remarks that bear on (I):[24]

(a) "*Prima facie*, perhaps the most serious problem facing Devitt is how the abstractness of syntax might be accommodated in his model of linguistic reality" (2008a, 32). The problem is thought to be particularly pressing in the case of "non-overt" or "unvoiced" constituents of a sentence. How is it possible for a sentence to have such a constituent? I responded: "The simple answer is: *there is nothing to stop there being a convention of this sort*" (2006b, 599). Consider the string 'Bob tried to

[24] Collins also repeats, in bolder terms, a line of thought in Rey (2006, 566–7) that "there are no correlations…between our linguistic performances and values of environmental variables" (2008a, 22–3). I have already responded to this line (2006b, 601–2). Collins does not advance the discussion.

swim'. The idea is, roughly, that each word in the string has a syntactic property by convention (e.g. 'Bob' is a noun). Put the words with those syntactic properties together in that order and the whole has certain further syntactic properties largely by convention; these further properties "emerge" by convention from the combination. The most familiar of these properties is that the string is a sentence. A more striking discovery is that it has a "PRO" after the main verb even though PRO has no acoustic realization. There is no mystery here.[25]

Collins thinks that this proposal "suffers from a technical problem and a conceptual problem." He points out that linguists take there to be two instances of PRO in 'Bob tried to swim', one a copy of the other. The technical problem then is: "how a convention might get a grip on the pair of copies" (2008a, 34). Collins describes various reasons linguists have for positing these copies. He continues: "On Devitt's proposal, speakers must be attuned to such motivations through their setting up of conventions. But, to ask again, how can this be…?" (34–5) And, of course, I don't know. But I don't need to know to sustain linguistic realism. I have shown that it is plausible that a whole lot of sounds and inscriptions that humans produce form representational systems. Those systems are not fully innate and so must be partly conventional. I have shown how it is *possible* for conventions to yield unvoiced elements. I have indicated in a general way, referring to David Lewis (1969), how linguist conventions, like other conventions (that are not stipulated), arise from regularities together with some sort of "mutual understanding." The regularities for linguistic conventions are in speaker meanings (156, 179–80). It would be nice to go much further, giving full explanations of the forming of linguistic conventions—indeed, of the forming of *any* conventions—but the hypothesis that there are such conventions does not depend on giving these. Lewis begins his book by claiming that it is a "platitude that language is ruled by convention" (1969, 1). This is surely right.

Collins emphasizes "the abstractness of syntax." On my view, syntactic properties are relational properties of physical entities, sounds, inscriptions, and the like. There is no known limit to the "abstractness" of the relational properties a physical entity might have or, indeed, have by convention.

Of course, if it could be shown that the required conventions were *impossible* then my proposal would be in trouble. But giving such an "impossibility proof" would be a mighty tall order. After all, the phenomena that lead linguists to theorize that expressions have certain structures must be phenomena that speakers *could be* sensitive to in forming a

[25] However there is a worry, related to one acknowledged in the book (156, n. 25), that still needs to be addressed. Why does a yellow flag with the meaning of the English sentence 'This ship has yellow fever' not have the syntax non-overtly that the sentence has overtly? Perhaps the answer is that since no parts of the yellow flag have syntactic properties, no such properties can emerge from the combination of the parts; from nothing comes nothing.

language. Yet, Collins' "conceptual problem" for my view does seem to amount to a claim that these linguistic conventions are impossible:

> Devitt's proposal begs the crucial issue…how one gets syntax off the ground via convention in the first place…. The problem in the linguistic case is to fix something non-linguistic so that the linguistic properties might be fixed over them… (2008a, 35)

The answer is that the sounds are "fixed." In the beginning, we bestow speaker meanings on them by using them to express thoughts. If this leads to regularities and mutual understanding then we have conventions. Collins later claims:

> Thus, something essentially syntactic is being presupposed in order to fix PRO. One must, therefore, in turn explain how the various syntactic positions are to be conventionally fixed. (35)

Probably so. Probably the convention for PRO could only come after other syntactic conventions had been fixed. But nothing significant follows from that. Nor does it from the claim that "PRO, like any other syntactic element, is related to other elements in hierarchical terms, not linear ones…it is simply a conflation of orthography with syntax to say that PRO occurs after *this* and before *that*" (35). Well, if that is so, *don't* say it. Say instead that *we represent PRO as* after this and before that. No matter. Collins goes on and on in this vein but none of it could cast doubt on the possibility of linguistic conventions.

So much for (I) and why Collins thinks SLEs do not exist. I turn now to (II). Collins makes two points against my view that SLEs do explanatory work.

(b) I argue that language acquisition provides one reason for believing in SLEs: the best explanation of the setting of parametric values in the typical member of a linguistic community is that she comes to participate in the parametric conventions of the community (181). Collins and I are at strange cross purposes over this. In the guise of his character Ling, Collins implicitly took me to be saying that conventions are *necessary* for parameter setting (2006, 477). But I am not saying this, as I emphasize in my response (2006b, 582). Yet Collins persists with this misconstrual: he takes my view to be that "parameter setting *depended* on conventions" (2008a, 28). But my view is not that a person—say the child of an isolated single mother—*could not* set parameters in the absence of any community. Of course he could. My view is that *the best explanation* of our *actual* acquisition adverts to conventions. So that is a good reason to believe that there are conventions. The child sets his parameters in a certain way because it regularly experiences others who have set them in that way. Conventions explain those regularities. Thus, "conventions explain the child's experiences which explain its settings. And the conventions explain why all the children in that community usually come to have the same setting" (2006b, 582). I am arguing about what *actually* causes acquisition not about what *could* cause it.[26]

[26] Collins' discussion at one point might give the impression that I hold "that syntax is not innate" (29). So let me emphasize that the innateness of part of syntax is a background assumption of my discussion (181; 2006b, 581–2).

(c) My defense of conventions in response to Chomsky puts most weight on their role in explaining communication (182, 186–8). Collins is very unconvinced. He claims "no-one has any idea how to explain communication, with or without conventional meanings" (2008a, 30). This is surely right if he means that we lack anything close to a complete explanation. But then we lack anything close to a complete explanation of language use (ch. 11) and acquisition (ch. 12), indeed of *anything* involving the psychology of language. However, I am arguing that conventions must be center stage in the explanation of communication. Collins has a different proposal:

> The proposal is a set of cues and prompts to which our mind/brains become attuned through the course of normal development;…we naturally project 'meaning' onto the cues and prompts, insofar as they are stable, but the prompts and cues are not meanings or syntactic structures, such things remain located in the head….[The proposal] does not carry the strange ontological burden of conventional meanings that play no essential explanatory role. The problem for Devitt's alternative is that even if there were external conventional meanings, exactly the same problems would remain as to how individual speakers orientate themselves in relation to the external objects. (2008a, 30)

But this proposal fails to address the crucial questions: What explains the essential fact that members of a linguistic community are *similarly* attuned to "the cues and prompts"? What is it that makes us "project" *one meaning and not another* onto a cue? The similar attunement and the particular projection are certainly not, for the most part, innate. My answer, in responding to Rey, was that "the superficial properties of sounds (inscriptions, etc.) provide such good clues of the speaker's message because there are conventions of using sounds with those properties to express meanings that are parts of the message" (2006b, 604). Individual speakers "orientate themselves in relation to the external objects" by participating in linguistic conventions (179–80). By appealing to conventions we have the beginnings of a good explanation of the similar attunement and the particular projection. Without this appeal, Collins leaves us with no prospect of an explanation at all. Communication is left a mystery. I shall return to this in discussing Rey.

This concludes my discussion of Collins.[27] I have argued, first that he is wrong to write as if no Chomskian has ever had the robust psychological conception that the rules of a language are embodied somehow in its competent speakers. Collins' own view of what a grammar tells us about the mind is anemic, differing little from mine. He is content with this anemic conception, I surmise, because he strongly rejects the linguistic conception. This leaves nothing else for the grammar to be about

[27] Collins also has some scathing remarks about my view of linguistic intuitions (2007, 421–2; 2008a, ## 19–20). I remarked earlier (section 3) that the negative tone of his discussion of the psychological conception gives a misleading impression of the extent of his disagreement with me. Just the same goes for his discussion of intuitions. I set aside demonstrating this until a later paper (2008).

but the mind, however impoverished a picture it gives of that. Collins' main problem with the linguistic conception is that even if there were a nonpsychological linguistic reality it would not be theoretically interesting to study; he rejects assumption (3). I have argued that his view is unsupported and false. Finally, I have argued that Collins does not cast any doubt on the existence of this largely conventional linguistic reality and has not undermined the view that it plays a key role in explaining language acquisition and use. Realist assumption (1) survives.

5. *Rey's Antirealism*

I turn now to Rey's "A Defense of Folieism" (2008). I shall consider what he has to say on my two questions about linguistic reality: (I) Why would anyone think that SLEs do not exist? (II) Why is it important to suppose that they do exist? Rey (2006a, b) and I (184–92, 2006b) have already gone a few rounds on these questions. So I will focus on what may seem to advance the discussion.[28]

(a) Rey has quite a lot to say on (I). He has previously alleged that the variability in pronunciation within a speech community poses a problem for my view of SLEs (2006, 560). I responded that this is not a problem because all of the sounds produced by the various people can be SLEs as the result of conventions (2006b, 600). Rey is unconvinced: "*Which* hearers understand *which* speakers under *which* circumstances varies far too widely for this to be other than an *ad hoc* and highly variable sociological suggestion" (2008, ##8). I shall be blunt: this issue is a red herring. In any group of people, G, there is indeed likely to be variation in the sorts of sounds that express a certain meaning. If the people speak what we loosely pick out as different languages—for example, English, French, Japanese—the variation will obviously be vast. If they speak what we loosely pick out as the same language—for example, English––the variation will be much less but still may be considerable, as Rey emphasizes. But each sort of sound will (normally) express that meaning in virtue of conventions in some group of people, sometimes not the group G. And those shared (largely) conventional meanings are, I argue, the prime focus in grammar construction (183–4). *The variability shows that there are many conventions for sounds not that there aren't any.*

[28] Rey finds "an annoying terminological difficulty" with my text in the context of discussing his distinction between "the reality of the causal structure of cars and that of SLEs" (2008, ##7, n. 4). I am not sure what the problem is. My view is that linguists are concerned with actual *syntactic* structures, which are theoretically interesting because they have a causal role, and which are nonintrinsic relational properties of sounds (inscriptions, etc.). I wonder whether there is some confusion here between causal, syntactic, and intrinsic structures. In sum, I think that, (i), existing physical objects really have properties of being a noun, being Australian, being unemployed, being a cleaner, being a vote, being a paperweight, etc.; that, (ii), the objects play causal roles in virtue of having those properties; that, (iii), they do not (largely) have those properties in virtue of their intrinsic structures and hence do not (largely) play their causal roles in virtue of those structures.

Rey goes on: "In the case of SLEs there is considerable disagreement, at least about what things count as tokens of them" (##8). But this is just a disagreement about the boundaries of conventions, a disagreement that *presupposes* there are the conventions to disagree about. There is little theoretical interest in whether the somewhat different sounds produced by an Australian and an American in saying 'mate' exemplify two different but related conventions or the one rather broad convention. And there is even less interest in precisely which bundle of conventions is taken to constitute what we loosely call "English." Whatever decisions one makes on these matters the key point stands: the sounds have their meanings, and hence are SLEs, (largely) by convention.

(b) The bee's dance and a human language differ in many ways, of course. Still, I claim, they are similar in one important respect: they are both representational systems. Rey challenges this similarity:

> What's striking about the waggle dance is that it does have a specific causally efficacious structure: the actual *direction* and *amplitude* of the dance are the properties to which the bees respond…It is because these properties can be specified independently of the psychology of the bees that von Frisch can win his Nobel prize without knowing anything about bee psychology. By contrast, even if we were to suppose that SLE tokens are real, *by Devitt's own lights, they would have to be picked out "relationally,"* in terms of, e.g., social facts and conventions that would inevitably involve considering *how perceivers intend or understand* those tokens….Without human minds, there can't be any SLEs. But, even without bee minds, there still could be waggles, amplitudes, angles and all (the waggles might be caused by a non-intentional mechanism, like the photo-tropism of a flower). (2008, ## 15)

There are no significant differences here between SLEs and the bee's dances. (i) A sound that is an SLE does have "a specific causally efficacious structure." The hearer responds to this structure because it is related by conventions with a certain meaning (185–6; 2006b, 598). (ii) The sounds that feature in these conventions have physical properties that "can be specified independently of the psychology" of humans just as can the physical properties of any other physical entity. (iii) The property that makes a sound a symbol, just like the property that makes a dance a symbol, is a relational one. There is a difference here: innate features largely explain the dance's property, I presume, whereas conventions largely explain the sound's. But this difference is beside the point: they are both symbols, whether by innateness or convention. (iv) Without a certain sort of relation to the human mind the sound we produce would indeed not be a symbol, hence not an SLE. But without a certain sort of relation to the bee's "mind" the dance the bee produces would not be a symbol either. Rey is right, of course, that "without bee minds, there still could be waggles." But then the waggles would not be symbols. Similarly, without human minds there could still be the sounds of languages but they would not be SLEs. The hypothesis that waggles and sounds are symbols involves the assumption that there are producers and receivers using them to convey information. That is the needed "psychological" assumption; see section 4.2 above.[29] Von Frisch made the hypothesis in the bee's case. We make the analogous hypothesis about human sounds. The challenge then is to explain the nature of the symbols. Von Frisch did that for the bee's dance. Grammars are partly doing it for our sounds.

(c) 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 (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.[30] 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.

(d) 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?[31] 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.[32] 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 al-

[29] Of course, the fact that a sound or waggle is related to a certain worldly situation can, with difficulty, be discovered without any knowledge of psychology. But what makes the sound or waggle a *representation* of that situation, and hence worthy of study, is its relation to minds; see section 4.1 above. And the relation to the worldly situation is *evidence* that the sound or dance is a symbol for what else could explain it?

[30] I have urged such an account of secondary qualities (1997, sec. 13.6).

[31] It might be objected that these other forms of language are not the concern of linguistics (c.f. the objection that competence in them is not the concern of the language faculty, which I discuss on 170–1). But that is part of what is at issue: according to the linguistic conception, the grammar of a language describes the syntactic properties of all forms of the language.

[32] For a nice summary of the vexed problems of phonology, see Burton *et al.* 2000.

phabet 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, ##10). 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 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 lack phonological properties, this would not show that the inscriptions of a language lack graphemic properties. No more would it show that these sounds and inscriptions lack syntactic and semantic properties. *Not only would the argument for antirealism about phonemes not generalize to other forms of language it would not generalize to syntax and semantics*. In my previous response to Rey I remarked that he had not yet presented any good reasons for abandoning realism about SLEs (2006b, 602). He still hasn't.

I turn now to question (II). Why does Rey think that it is not important to suppose that SLEs exist?

(e) Having argued that there is no "mapping" between acoustic streams and syntactic and phonological properties, Rey continues: "*the lack of this mapping makes absolutely no difference to the explanatory interest of linguistic theories*. For there's no explanatory reason for those properties to be instantiated in the acoustic stream" (2008, ##9). Yet surely, for the reasons I have already indicated in discussing Collins (section 4.2 (c)), we need to posit real SLEs to explain communication. Not according to Rey:

> communication…is a kind of *folie à deux* in which speakers and hearers enjoy a stable and innocuous 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. (##1)

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? Just like Collins, Rey seems to leave unexplained, even miraculous, what we need a conventional language to explain. But Rey has an ingenious response: an intentional inexistent, hence an SLE, can enter into a convention even though it doesn't exist (2006b, 558). I argued against this possibility, using Rey's example of Santa Claus (2006b, 604). Rey has responded (2008, ##14). I shall set that general issue aside and focus on the particular issue of SLEs.

As noted, Rey thinks that phonemes are *perceptual* inexistents. And he thinks that we can find conventions involving these inexistents by looking to stable *perceptual illusions*:

> it's easy to imagine conventions systematically arising from such stabilities and attaching to these apparent objects. To repeat yet again: many public advertisements involve Kanizsa alphabets, which patently involve mere illusions of the standard orthographic letters, all without the slightest difficulty for any of the standard linguistic conventions… (2008, ## 10)

Rey needs to explain his *folie* illusion of communication. Picking up on the idea that conventions are central to explaining normal human linguistic communication, Rey claims that intentional inexistents can enter into conventions. They can do this because they are the result of stable perceptual illusions. So, Rey's order of explanation is from stable perceptual illusions to conventions to stable *folie* illusions.

I have objected to this idea briefly before (2006b, 604). Rey gives that objection very short shrift (2008, ## 15, n. 13). So I shall now develop it at some length.

Rey thinks that antirealism about SLEs is common in linguistics. I think he is right about that (185 n). And he has done a service by raising the problem this poses for a theory of communication. But he has not gone nearly far enough in confronting that problem. *He has not shown 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.* That is the main point I wish to make. But I shall now try to indicate why I think that any attempt to show this will collapse into linguistic realism: any convention involving his alleged inexistents will be one involving real SLEs.

(i) Taking the Kanizsa alphabet as our example once again, consider the stability we get straightforwardly from a perceptual illusion. We get the stability of taking an existent "incomplete" P1-object as an inexistent "completed" P2-object. But, what we don't get is the stability we need for the *folie* illusion, that of taking the P1-object *as an L-object*, an object with graphemic, syntactic, and semantic properties. To explain that stability we need to explain the convention according to which anything taken as a P-2 object, including something that *really is* a P2-object, is taken as an L-object. How could Rey explain that, given his view that there aren't any L-objects?

(ii) As a first step in attempting to answer this, I think we should return to the sounds of a language. Rey's view is that these sounds stably prompt illusions involving inexistent phonemes. I don't accept this view—see (c) above—but it certainly has *some* plausibility. Then, *perhaps*, these perceptual illusions alone could yield the stability of taking sounds as phonemes, as L-objects. So let us suppose that this were so. Rey seems to think, implausibly, that graphemes are like phonemes in also being the inexistent result of an illusion. He may think further that perceptual illusions alone could yield the stability of taking inscriptions as graphemes; to that extent, the illusions alone would make P2-objects L-objects. This would be contrary to what I have just claimed in (i) and seems to me to have no plausibility at all; see (d) above. Still, let us suppose for the moment it were so. What would still remain to be explained are the conventions, essential to Rey's *folie* illusion, of taking inexistent phonemes and graphemes to have *syntactic and semantic properties*.

(iii) So let us now try to explain those conventions. I take it that Rey's idea must be that inexistent phonemes and graphemes are regularly associated in communication with certain syntactic and semantic properties and hence, with the help of some sort of "mutual understanding," those inexistents come to have those properties conventionally. But how could this happen without establishing conventions involving the existent sounds and inscriptions that prompt these alleged inexistents, and hence without creating real SLEs? The problem for Rey is most vividly demonstrated by considering an imaginary case. I have rejected the view that graphemes are, as a matter of fact, (typically) inexistents. But one can imagine a situation where it would be plausible to say that graphemes were inexistents: suppose that the only inscriptions ever used for English were the "incomplete" P1-objects of the Kanizsa alphabet. On observing these objects we might have the illusion of the "completed" P-2 objects of our present alphabet. In time, those inexistent P-2 objects might become the graphemes of English. And Rey's idea would be that the regular association of these inexistents with intended syntactic and semantic properties would yield the needed conventions. But how would this regular association come about? It would *always* involve a P-1 object, an existent but "incomplete" Kanizsa figure. After all, the inexistent P-2 objects do not occur "out of the blue": they are prompted in a systematic, even if complicated, way by P1-objects. But then it would be *because of* the regular associations of existent P1-objects with the intended syntactic and semantic properties that the inexistent P2-objects would come to be associated with those properties; the former associations would *explain* the latter. So, the very same processes that would conventionally relate P2-objects to syntactic and semantic properties would, given "mutual understanding," conventionally relate P1-objects to them. So, the perfectly real but "incomplete" Kanizsa figures would come to *have* those properties.[33] So they would be SLEs. So, linguistic realism. In sum, the illusion of seeing certain graphemes would simply be part of the realist explanation of the syntactic and semantic conventions; the illusion is simply a route to realist conventions.

(iv) So, even in this imagined situation where it really is plausible to say that graphemes are inexistent, there would be SLEs. This story carries over to phonemes. Suppose that Rey were right that phonemes don't exist. He would still agree that certain superficial properties of the sounds of an utterance are the *clues* that prompt the inexistent phonemes and hence largely enable someone to associate the utterance with the syntactic and semantic properties intended by the speaker. *How* could those superficial properties thus serve as clues? Only because there are systematic, even if complicated, connections between those properties and phonemes, connections studied by phonologists. So *whenever* Rey's inexistent phonemes would be regularly associated with syntactic or semantic properties, the sounds that prompt the phonemes would also be regularly associated with those properties. Indeed, the latter associations *explain* the former. So, given "mutual understanding," the sounds become conventionally related to syntactic and semantic properties. So they are SLEs. So, linguistic realism. The fact that sounds must be systematically related to phonemes in order for the superficial properties of the sounds to be clues to speaker meanings leads straight to realism. And the inexistent phonemes would play just the same intermediate role in establishing the syntactic and semantic conventions for real sounds as the inexistent graphemes did for real Kanizsa inscriptions. The phonological illusion would simply be a route to conventions involving those sounds.

The moral of this attempt to explain the conventions Rey needs for his *folie* illusion is that even if we granted that phonemes were inexistents, which Rey may be able to argue, and that graphemes were inexistents, which they could be but surely (typically) aren't, there would still be SLEs. There would still be sounds and inscriptions with syntactic and semantic properties and linguistic realism would be established. Perhaps there can be an antirealist explanation of the conventions. My main point is that Rey has not produced one.

Humans are very successful at conveying thoughts from one to another. How do they do it? The linguistic realist's explanation is that they use a (largely) conventional language, a syntactically complex representational system of sounds, inscriptions and the like. Other explanations may be possible; thus humans wouldn't need this language if they could communicate by telepathy. But we need some explanation if communica-

[33] I think they would similarly come to have graphemic properties, but I am not insisting on this.

tion is not to be left a mystery. That is how Rey has left it.

This concludes my discussion of Rey. I have argued, first, that he has not presented any good reason against linguistic realism. His argument from the variability of language is a red herring. It shows that there are many linguistic conventions for sounds not that there aren't any. I have argued that the bees' waggle dance and a human language are both representational systems. The systems differ in many ways, of course, particularly in that the dance is innately based whereas the language is largely conventionally based. Rey's attempt to undermine the view that the language is a representational system by finding other differences between it and the dance fails. In particular, both the waggles of the dance and the sounds and inscriptions of a human language can be characterized without any appeal to psychology, yet the status of them all as symbols depends on relations to "minds." Rey's view that phonemes are perceptual illusions needs more argument. In any case, it does not generalize to graphemes and other forms of language and does not show that any form lacks syntactic and semantic properties and hence does not consist of SLEs. I have argued, second, that the explanation of communication requires linguistic realism. Rey's contrary *folie à deux* view faces the problem of explaining the stability in the matching of messages understood with messages intended. At the very least, his ingenious proposal that inexistent phonemes and graphemes can enter into conventions needs much more development than he has given it. He needs to show how we could get from his alleged stable perceptual illusions involving inexistents to the required syntactic and semantic conventions without collapsing into linguistic realism.

I have one final thought in favor of linguistic realism. I have pointed out (sec. 4.2) that scientists frequently hypothesize that a species has a representational system which its members use to communicate with each other. In making such a hypothesis, scientists suppose that certain behaviors are produced *because* they involve symbols representing things, and that it is *because* of what the symbol represents that other members of the species respond to the behaviors as they do. This is thought to be the best explanation of what is going on. 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 the bees and of the barks of the prairie dogs. Sometimes, most notably with representational systems that we have *taught* to dolphins 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 the analogue of what we have been calling "linguistic realism."* The scientists are supposing that these animals are producing behaviors that *really do* involve representations having semantic and sometimes syntactic properties. And "a Martian scientist" would surely think at least as much about our behaviors.[34] 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.

6. *Conclusion*

I have argued that there is a linguistic reality external to the mind and that it is theoretically interesting to study it. If there is this reality, we have good reason to think that grammars are more or less true of it. So, 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. For, all we learn about that reality from the grammar is that it "respects" the rules of the grammar. Finally, contrary to what Collins claims (2007, 420) there is indeed a "science to be had" of linguistic reality, and generative grammars are at the very center of that science.[35]

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[34] In doing so the Martian would be following in the footsteps of our own cognitive ethologists: "Human language is usually presumed to include referential signals," a language that has evolved from those of other animals (Allen and Hauser 1993, 82).

[35] An early version of part of this paper was given at the third annual Dubrovnik conference on the philosophy of linguistics held in September 2007. The paper has benefited greatly from the vigorous discussion at that conference, from the written comments of John Collins and Peter Slezak, and, particularly as usual, from many exchanges with Georges Rey.

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