

# *Psychological Conception, Psychological Reality: A Response to Longworth and Slezak*

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*My book, Ignorance of Language (2006a), challenges the received Chomskian “psychological conception” of grammars and proposes a “linguistic conception” according to which a grammar is a theory of a representational system. My response to Guy Longworth rejects his claim in “Ignorance of Linguistics” (2009) that there is “mutual determination” between linguistic and psychological facts with the result that both of these conceptions are true. Peter Slezak’s “Linguistic Explanation and Psychological Reality” (2009) is full of flagrant misrepresentations of my discussion of the psychological conception and of the psychological reality of linguistic principles and rules. My response summarizes the worst of these misrepresentations.*

**Key words:** Chomsky; grammars; psychological conception, linguistic conception, psychological reality; linguistic competence; supervenient; Longworth; thoughts; conventions; Slezak

## *Longworth*

The first major conclusion of my book, *Ignorance of Language*, is that linguistics is not part of psychology (2006a: ch. 2; see also 2003). This goes against Noam Chomsky’s view that a grammar is about linguistic competence, a mental “organ” that is the speaker’s language faculty. In later versions of my argument I called this view “the psychological conception” of linguistics/grammars. Instead of it, I urged “the linguistic conception” according to which a grammar is about a nonpsychological realm of linguistic expressions, physical entities forming a symbolic or representational system (2006b, 2008a,b,c). This conclusion has been the main focus of my Chomskian critics (Antony 2008; Collins 2007, 2008a,b; Slezak 2007; Smith 2006). And it is the focus of Guy Longworth in “Ignorance of Linguistics” (this issue).<sup>1</sup> But whereas the earlier

<sup>1</sup> All page references to Longworth are to this paper.

critics argue, in no uncertain terms, that my conception is wrong, Longworth has the irenic view that there is really no disagreement here: the two conceptions “might be notational variants, or different determinations of a single determinable view” (p. 36). This is an entertaining idea but it is very wrong. The conceptions are in real disagreement.

My view of the disagreement is as follows. According to the linguistic conception, a grammar is a theory of a representational system, a language. This is not to say that it tells us nothing about the psychological reality of a competent speaker of the language. It tells us that her competence “respects” the principles and rules posited by the grammar in that the competence is apt to produce and understand expressions that are governed by those principles and rules. The grammar thus provides the minimal position on psychological reality that I call “(M)” (2006a: 57). But the grammar 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 principles and rules is embodied some way or other in the mind and so is 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*. In contrast, according to the psychological conception, a grammar is a theory of the psychological reality of a language in its competent speakers that goes beyond (M).

Now Longworth does not challenge my claim that the linguistic conception is true: “Insofar as Devitt aims only to support the claim that Linguistics involves the study of language, I think that he is right about the object of Linguistics.”<sup>2</sup> But he thinks I am wrong to claim that the psychological conception is false: “Linguistics is just as much concerned with the study of linguistic competence. And to that extent, Chomsky is also right about the object of Linguistics” (p. 36). So Longworth is committed to grammars telling us more than (M) about the psychological reality underlying language. That is where we differ.

Longworth’s argument starts by considering what I call “the supervenience defense” of the psychological conception. This defense, repeated time and again,<sup>3</sup> is that the linguistic properties of expressions supervene solely on psychological facts and so the study of those properties must be part of psychology. Longworth quotes a passage from *Ignorance* (2006a: 40) which aims to show that the supervenience defense is *entirely erroneous*. And if it were not, all the sciences—economics, psychology, biology, etc.—would be parts of physics. Longworth thinks that, “in the abstract”, I am right about this, *but*

<sup>2</sup> So Longworth accepts “linguistic realism”: there really are linguistic symbols external to the mind with the sorts of properties attributed to them in grammars. I have defended this realism (2006a: 184–9; 2006b: 597–605; 2008a: 221–9; 2008b: 251–4). Georges Rey thinks that antirealism is the received view in linguistics and has mounted a detailed defense of that view (2006a; 2006b, 2008); see also Collins 2006 and 2008a,b, and Smith 2006.

<sup>3</sup> Consider, for example, Dwyer and Pietroski (1996: 350), Smith (2001: 284; 2006: 441–2), Laurence (2003: 87–8), and Rattan (2006: 508).

the question whether a special science has a proprietary domain, rather than being absorbed by a subvening domain, is not trivial to answer. In particular, the answer to such a question depends on whether the special science has its own proprietary generalizations and explanations, and does not simply restate the generalizations and explanations already contained in the subvening domain. (p. 29)

I have argued at considerable length, not only in *Ignorance* but also in this very journal (2006b: 574–88; 2008a: 205–21; 2008b: 249–51), that the generalizations and explanations proposed by a grammar are not psychological. Generalizations about the representational systems of organisms, whether that of a human, a bee, a prairie dog, or whatever, are not generalizations about the inner state of the organism that uses that system. In particular they tell us *absolutely nothing* about the facts in virtue of which symbols have their syntactic and semantic properties. Thus, consider the grammatical generalization, “An anaphor must be bound by another expression in its governing category”. This tells us about the properties of certain symbols, anaphors; it tells us nothing about what those properties supervene on.

So, I am not only right “in the abstract”, I am right about linguistics: it has its proprietary domain. Longworth is not convinced. He goes on to claim that a special science will not have its own domain if there is “mutual determination” by the facts and explanations in its domain and in the subvening domain. He follows this with a *very* surprising claim:

Devitt offers no reason to think that there is a failure of mutual dependence between relevant psychological facts and relevant linguistic facts. That is, he provides no reason for expecting linguistic properties not to supervene on competence properties, or expecting competence properties not to supervene on linguistic properties. (p. 29)

Let us start with the supervention of linguistic facts on psychological facts. Contrary to what Longworth claims I have quite a lot to say about this. I argue for the view that a linguistic symbol is a *social* entity. It has its linguistic properties in virtue of environmental, psychological, and social facts (2006a: 39–40; 2006b: 582–4). I offer a Gricean explanation of these properties. First, the speaker meaning (and syntax) of a sentence on an occasion of utterance is explained in terms of the thought meaning that produces it. But the linguistic properties that interest us, the social ones, are the (largely) conventional ones.<sup>4</sup> These are explained in terms of regularities in speaker meanings:

The conventional meaning of a sentence on an occasion of utterance—what it conventionally means in a language given the context—is explained somehow in terms of regularities in speaker meanings: Speakers of the language regularly use one linguistic word to express one mental word, another, another; they regularly use linguistic sentences with one speaker syntax to express one mental syntactic structure, another, another. (2006a: 156)

<sup>4</sup> Chomsky claims that linguists are not interested in conventional properties (1996: 47–8). I have responded (2006a: 180–89; 2006b: 581–2, 598–604; 2008a: 216–28; 2008b: 251–4).

Mere regularity in speaker meanings is not enough for a conventional meaning, of course: we need to add “some sort of mutual understanding”. And it is hard to say precisely what sort (pp. 179–180).

So, on this account, linguistic facts supervene on conventions for expressing thoughts. They do not supervene on facts about linguistic competence. In particular, a linguistic symbol does *not* have the conventional properties that interest us in virtue of the competence of the speaker who produced it.<sup>5</sup> Finally, to repeat my standard response to the supervenience defense, even if linguistic properties did supervene on psychological ones this would not make the linguistic task psychological.

But what if competence properties supervened on linguistic ones, as Longworth suggests? So, were I wrong about the reverse supervenience, we would have “mutual determination”. After sharply distinguishing these two sorts of properties in chapter 2, *Ignorance* is largely devoted to examining what we know about the competence properties and how we should go about discovering more. Now in all that discussion I don’t think I addressed the issue of what these properties supervene on and so may indeed not have *provided* a reason for “expecting competence properties not to supervene on linguistic properties”. But the idea that the competence properties do so supervene is surely too implausible to take seriously. To see this, consider what competence *is*.

My fourth major conclusion is that the nature of competence “should be investigated from a perspective on thought” (2006a: 129). So I consider the consequences for that investigation of various perspectives on thoughts. The most theory-neutral view of competence in a language comes from a perspective with no commitment to thoughts at all: competence is “the ability to produce and understand sentences with the sounds and meanings of that language” (p. 128). I then consider a series of increasingly theory-laden views which I later sum up as follows:

- (a) Intentional realism (8.1), together with the view that language expresses thought—LET (8.2)—led us to the following view (8.3): the competence is the ability to use a sound of the language to express a thought with the meaning that the sound has in the language in the context of utterance; and the ability (together with some pragmatic abilities) to assign to a sound a thought with the meaning that the sound has in the language in the context of utterance (similarly for inscriptions, etc.). Hence, a certain conceptual competence has an ontological priority over the linguistic competence....(b) RTM [the Representational Theory of Mind] was the first, fairly modest, step toward a theory of thoughts (9.1). It yielded the view that competence is the ability to *translate*

<sup>5</sup> Longworth responds to this by pointing out that the supervenience of linguistic facts on facts about thoughts is compatible with their supervenience on facts about competence if the latter also supervene on facts about thought (p. 31). But there will be this compatibility only if the facts on which the competence facts supervene include the ones on which the linguistic facts supervene. And they do not, as the following discussion of competence makes clear. Longworth also objects “that the structure of thought alone can’t sustain the distinctive properties of language” (p. 31). *Ignorance* rejects this sort of objection (2006a: 153–8).

back and forth between mental representations and the sounds of the language. (c) Acceptance of LOTH (the Language of Thought Hypothesis) is a further, fairly huge, step. It yielded the very theory-laden view that competence is the ability to translate back and forth between mental *sentences* and the sounds of the language. So the conceptual competence that, along with a processing competence, constitutes competence in the language is the competence to think mental sentences with meanings expressible in the language. (p. 148)

Against a background of each of these views in turn, I go on to consider what the evidence suggests further about the nature of competence. Does the competence reside in a language faculty? Does it involve the principles and rules of the language whether represented or simply embodied? Do the processing rules operate on metalinguistic representations of syntactic and semantic properties of linguistic items or are their operations fairly brute causal? Attempting to answer these questions leads to one major conclusion and several tentative proposals.

We need go no further into these details because it is surely already apparent that linguistic competence will not supervene on linguistic facts on any plausible theory of that competence. That competence is a combination of a conceptual competence to think the thoughts expressible in the language and a processing competence for expressing those thoughts and understanding their expression. The processing competence will supervene on neurological states and the conceptual competence will largely do so. However, given an appropriately externalist view of reference, conceptual competence must partly supervene on causal links to the external world. And I do allow that, to a small but interesting extent arising from “reference borrowing”, conceptual competence is indeed determined by linguistic facts (pp. 138–41). But the idea that linguistic competence as a whole is so determined is truly bizarre.

Why does Longworth think otherwise? The answer seems to arise from a misunderstanding of the constraint that “respect” places on competence, a misunderstanding involving equivocation over the word ‘determine’.

I have noted that a grammar of a language entails a certain minimal position (M) about competence in the language: the competence “respects” the principles and rules posited by the grammar in that the competence is apt to produce and understand expressions that are governed by those principles and rules. So if we know that this grammar is right for a language we know that a speaker’s competence respects its principles and rules. Longworth takes this to show that “relevant properties of the competence are determined, via the ‘respect’ constraint by properties of the language”, where the “relevant properties” are those of respecting the linguistic properties (p. 30). Now, we can make this a (rather loose) true statement by construing ‘determined’ epistemically. So construed the statement is that if we *know* the properties of the language—have a true grammar—then we *know* the “relevant properties” of the com-

petence. But there is no truth in the statement at all if ‘determined’ is construed metaphysically. And that is how it has to be construed to support Longworth’s talk of mutual determination. It has to be the case that the competence properties *supervene on* the linguistic ones: the competence has its properties *in virtue of* the language having its.<sup>6</sup> The respect constraint does not give you that. And, as noted, it is a bizarre suggestion anyway.

In any case, the “relevant properties” of competence that we know about from a grammar are very thin. The grammar tells us only that the competence includes something-we-know-not-what that respects the principles and rules of the grammar. At that point the task of discovering the nature of competence has hardly begun. I struggle with this task throughout *Ignorance* but end, as one would expect, without much accomplished. Despite the great success of generative linguistics, and the ingenious work of psycholinguists, we still know little more about human linguistic competence than we do about the bee’s dancing competence (2006a: 195–243). And we know just about nothing about that.

In sum, there is almost no truth in Longworth’s thesis of the mutual determination of linguistic facts and facts about competence. Psychological facts play an important role in determining linguistic facts but those determining facts are not ones about competence. Linguistic facts have near enough no role in determining facts about competence.<sup>7</sup> The disagreement between the linguistic conception and the psychological conception is very real.<sup>8</sup>

However, I shall finish on a note of agreement. Longworth claims that “the Linguist studies the properties of products of competence only as those properties are exploited by speakers” (p. 33). This follows a similar claim about the bee’s dance. I have said something along much the same lines in a general discussion of representational systems:

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. (2008a: 216)

Positing a representational system involves a substantial hypothesis about a species. And von Frisch’s hypothesis in the case of the bee was initially quite implausible.<sup>9</sup> But the key point is that the need for such

<sup>6</sup> We would have to equivocate with ‘determined’ in a different way to get some truth in the following claim: “linguists appear to show no interest in elements of language that are not determined by elements of competence” (p. 31, n. 6). We would need to construe ‘determined’ in a causal way rather than an epistemic or a subvention way. (Even then the claim is not true because linguists are interested in properties like the reference of indexicals that are not caused by competence.)

<sup>7</sup> So I reject both Longworth’s (a) and his (b) (p. 33).

<sup>8</sup> Longworth raises the good question what difference this disagreement makes to linguistic practice (p. 28). *Ignorance* addresses this question briefly (2006a: 15–16).

<sup>9</sup> Barry Smith still finds it so (2006: 440). I have responded (2006b: 585–7).

a hypothesis does not make the study of the representational system the study of the competence. Von Frisch told us just about everything about the bee's representational system, just about nothing about the bee's competence.

## *Slezak*

I remarked recently that “there is a pattern to Chomskian criticisms of *Ignorance*. The pattern is one of misrepresentation, *ex cathedra* pronouncements, relentlessly uncharitable readings, and a wearily impolite tone” (2008c: 671). I cited Peter Slezak's “Linguistic Explanation and ‘Psychological Reality’” (2007) as one of several paradigms.<sup>10</sup> His paper in this issue (2009) is a later version of that paper.<sup>11</sup> It is still a paradigm.

I have commented at length on Slezak 2007 in a paper available on my homepage (2007).<sup>12</sup> Before that, I had given him detailed comments on an even earlier version. Yet the present version is still full of flagrant misrepresentations of my discussions of the psychological conception of grammars and the psychological reality of linguistic principles and rules. I shall summarize the worst of these misrepresentations, referring often to Devitt 2007. I shall also attend to changes that Slezak has made in this version.

(1) Throughout his paper, Slezak persists in conflating the psychological *conception*, according to which grammars are psychological theories, with a psychological *realism*, according to which the principles and rules of grammar are somehow embodied in the mind (Devitt 2007: sec. 4.1). I do have a negative view of the psychological conception, as we have seen in my response to Longworth. My view of psychological realism is complicated, involving several proposals, most of them tentative and one of them *positive* not negative: “A language is largely psychologically real in a speaker in that its rules are similar to the structure rules of her thought” (2006a: 152). So, contrary to what Slezak claims, my “negative critique of Chomsky's psychological realism regarding grammars” is *not* “one of two pillars” of my position (p. 5).

(2) Slezak still alleges that the other “pillar”, in effect my linguistic conception, “entails nothing less than undoing the mentalism of the ‘cognitive revolution’ in a return to the *nominalistic* aspects of Skinnerian, Bloomfieldian behaviourism” (p. 5). This is perhaps the silliest charge that Slezak makes, as I noted before (2007: sec. 4.4).<sup>13</sup>

<sup>10</sup> Other paradigms cited are papers by Louise Antony (2008), John Collins (2007, 2008a,b), and Barry Smith (2006). For responses, see Devitt 2006b, 2008a,b,c. Longworth's paper is an exception on all counts. I fear for his future.

<sup>11</sup> All page references to Slezak are to this paper.

<sup>12</sup> Parts of that paper were delivered in a response to Slezak at a symposium at the University of New South Wales in July 2007. My present response draws on one I made to Slezak's presentation at the annual conference of the Australasian Association of Philosophers in Melbourne in July 2008.

<sup>13</sup> Devitt 2008c (p. 681) responds to a similarly silly charge by Antony (2008).

(3) My second major conclusion rejects “The Representational Thesis (RT)”. According to RT a speaker of a language stands in an unconscious or tacit propositional attitude to the rules or principles of the language which are represented in her language faculty”. Slezak still claims that I attribute RT to Chomsky (p. 5). I pointed out (2007: sec. 4.2(i)) that the evidence he produced for this claim in his earlier version (2007) was simply fabricated. In response he has abandoned that evidence and fabricated some more using the same technique (pp. 5–6, n. 4).

(a) I do indeed claim that there is “strong evidence” (2006a: 10), “a great deal of evidence” (2006b: 572), for an *interpretation* that attributes RT to Chomsky, but I do not make the attribution. Rather, I raise the possibility of another interpretation according to which the principles and rules are “*embodied somehow* without being represented” (p. 7), an interpretation that Slezak later notes! I explicitly “take no firm stand on this matter of interpretation” (2006a: 7). It is noteworthy, however, that Slezak, like Smith (2006) before him, makes no serious attempt to explain away the evidence I cite that Chomsky and others *do* subscribe to RT (2006a: 3–6, 72–81, 96–7; particularly, 2007: sec. 4.3). He contents himself with *ex cathedra* pronouncements (p. 5).

(b) I do indeed claim that there is “massive evidence” for *something* that concerns RT but not for Chomsky subscribing to RT. The massive evidence is that RT is “to be found in linguistics” (2006a: 7).

(c) Slezak notes that I cite an exchange between Chomsky (2003) and Georges Rey (2003a,b) over what Chomsky means by ‘represent’. Slezak apparently thinks that my “agnosticism” on this issue is evidence that I attribute RT to Chomsky. First, agnosticism is a *lack* of commitment and so could hardly be evidence of a commitment! Second, what Chomsky means by ‘represent’ is one thing, whether he subscribes to RT is another. Rey no more attributes RT to Chomsky than I do.

(4) In the earlier version, Slezak claimed explicitly that my critique *depended on* my attributing RT to Chomsky. The claim is at least implicit in the current version. It is quite false (2007: sec. 4.2(ii)).

(5) Slezak still responds to my charge of “a certain use/mention sloppiness” (2006a: 69) by drawing attention to the well-known ambiguity in Chomsky’s use of ‘grammar’, “used to refer both to the speaker’s internally represented ‘theory’ and also to the linguists’ account of it” (p. 7). But that ambiguity is beside the point of the sloppiness I am describing, which includes, for example, identifying structural descriptions of expressions with the expressions themselves.

(6) A certain distinction is important to my discussion of psychological reality. It is that between processing rules that govern by being represented and applied and those that are simply embodied without being represented (2006a: 45). Slezak persists with his mistakes about this. (a) He takes it as “just an elaboration of [the] Quinean distinction between ‘fitting’ and ‘guiding’ rules” (p. 8). It is not. Quine’s distinction is between an object behaving as if governed by a rule and its being re-



ally governed by it. Mine is between two ways of really governing (2007: sec. 4.6). (b) “Devitt says that his thesis that linguistics is not part of psychology rests in part on this principle” (p. 8). I don’t say it and it isn’t true. My discussion of psychological *reality* rests on the distinction not my rejection of the psychological *conception*.

(7) Slezak accuses me of “egregious misuse of Chomsky’s key technical term ‘competence’” because he takes me to identify competence with performance and hence to ignore Chomsky’s famous distinction (p. 8). This is preposterous. Chomsky’s distinction is an ever-present background to my discussion of psychological reality (see, e.g., 2006a: 62–8). Linguistic competence is a *state*, linguistic performance is a *process*. The state embodies processing rules which are center-stage in performance. *Performance errors aside*, the state will produce expressions governed by the structure rules described in the grammar.

(8) Slezak notes approvingly that “Chomsky’s ground for attributing causal efficacy to rules is just that they have a place in our best explanatory theories”. Slezak takes this to establish the psychological reality of the rules posited by a grammar (p. 10). And so it would if the grammar were a psychological theory. But that is precisely what I argue that the grammar is not (2006a: ch. 2). Slezak persists in restating the Chomskian position without attempting to answer my argument.

(9) A frequent refrain in Slezak’s paper is still that my views are really “only a re-statement of Chomsky’s own views, though much less perspicuous than the original” (p. 12). This is weird (2007: sec. 4.5). First, if it were so, one wonders why Slezak heaps such scorn and abuse on my views. Second, it obviously isn’t so as a quick look at a summary of the book’s theses would show (2006a: 265–6).

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