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Defending Ignorance of Language: Responses to the Dubrovnik Papers

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This paper is a response to some interesting papers critical of main themes in my recent book, Ignorance of Language [2006a]. Those papers all arose out of wonderfully convivial and productive conferences on the philosophy of linguistics in Dubrovnik in September 2005 and 2006. They are now published in the present volume.

Four of the papers, Barry Smith's "Why We Still Need Knowledge of Language", Robert Matthews' "Could Competent Speakers Really Be Ignorant of Their Language?", John Collins' "Between a Rock and a Hard Place: A Dialogue on the Philosophy and Methodology of Generative Grammar", and Gurpreet Rattan's "The Knowledge in Language" are critical of my rejection of the received Chomskian "psychological conception" of linguistics in favor of a "linguistic conception". I shall respond to these criticisms in section 2.

Another received view, reflected in the title of one of Noam Chomsky's most famous books, *Knowledge of Language* [1986], is that being competent in a language requires *knowing* things about it. In contrast I argue for the view, reflected in the title of my book, that a person could be competent in a language and yet totally *ignorant* about it. Matthews, Rattan and Smith take issue with my view. So, in effect, does Nenad Miščević in "Intuitions: the Discreet Voice of Competence". I shall respond in section 3.

Finally, my linguistic conception of linguistics presupposes that there is a linguistic reality "out there", a reality of symbols made up of sounds, inscriptions, and the like, that really have linguistic properties. My book criticizes Georges Rey's denial of this realism. His paper "Conventions, Intuitions and Linguistic Inexistents: A Reply to Devitt" is a response. I shall respond to that response, and to the similar antirealism of Collins and Smith, in section 4.

However, I must start with a general comment on Smith's paper.

1. *Smith's Weird Misinterpretation*

Smith's paper is an exuberant, but exasperating, attack on *Ignorance of Language* and just about everything he takes it to stand for. His wide-

ranging and scornful criticisms—his epithets include "uninformative platitude" (p. 435), "wholly inadequate" (p. 436), "utterly blithe" (p. 437), "deeply flawed" (p. 440), "badly awry" (p. 445), "wildly speculative" (p. 445). "wildly amiss" (p. 447) and, my favorite, "lily-livered" (p. 451)—are vitiated, time and again, by misinterpretations and/or overlooked evidence and arguments. One misinterpretation is particularly serious. It is also weird.

Ignorance characterizes a thesis which it calls "the Representational Thesis (RT)" according to which "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" (p. 273). A reader of Smith's paper is likely to come away with the impression that *Ignorance's* strategy is to criticize Chomsky by attributing RT to him and then refuting it. That's all. The impression is conveyed throughout the paper but the following is particularly explicit:

> It must be said that Devitt's dialectical strategy is puzzling. It appears to be as follows: suppose Chomsky were saying that competent speakers of a language had propositional knowledge of its rules; look what a mess he would get into. What should we conclude from that? The natural repost is to say: why suppose this is the right or the only way to interpret Chomsky, why attribute to him such an implausible view? Why is no real attempt is made to make out a more plausible and less straw man version of Chomsky's position?[1] (p. 445)

Smith is way off track. (1) *Ignorance* makes no firm attribution of RT, or any other view of linguistic competence, to Chomsky. True, it starts with "the natural interpretation" that Chomsky and other linguists do believe RT. This interpretation is supported by a great deal of evidence (pp. 3–6, 96–7), most of which, I can't resist saying, Smith blithely ignores.[2] But far from there being "no real attempt to make out a more plausible…version of Chomsky's position", the book *immediately* raises doubts about the natural interpretation and entertains another according to which linguistic rules are embodied somehow without being represented (pp. 6–7). When I later address the interpretative issue at some length (pp. 62–71), I do not settle on an interpretation and conclude that "one is left uncertain of Chomsky's position" (p. 71; see also pp. 174–7). Smith ignores these discussions too.

(2) Although *Ignorance* does of course pay a lot of attention to the views of Chomsky and other linguists, my main purpose is not the negative one of attributing views to Chomsky or anyone else and then going on to refute them. The purpose, made very explicit, is the positive one of investigating the psychological reality underlying language (p. 3):

> I want to emphasize from the beginning that interpreting Chomsky is not my major concern. My major concern is to evaluate a variety of ways in which language might be psychologically real in the speaker, whether or not they are plausibly attributed to Chomsky (or his followers). (p. 7; see also p. vii)

(3) *Ignorance* does indeed set about refuting RT: its "second major conclusion" is that "there is no significant evidence for the Representational Thesis (RT) and, given what else we know, it is implausible" (p. 275). But RT is only one of many positions on psychological reality considered. Thus, the initial summary of the book, "The Plan" (pp. 8–14), describes some possible positions, to be introduced and named in section 3.4, as follows:

> These [positions] vary according to whether or not the rules of the language are embodied in the mind; whether or not some processing rules for language are represented in the mind (c.f. RT); whether or not some processing rules operate on metalinguistic representations of syntactic and semantic properties of linguistic items. (p. 9)

Positions that do not involve RT are discussed throughout the book and listed in the "Glossary" (pp. 274–5) and "Index" (pp. 301–2). Smith does not mention these discussions and always writes as if the only position considered is RT.[3]

Smith's construal of *Ignorance* is weird for three reasons. First, he makes it in the face of overwhelming evidence that it is wrong. Aside from the evidence already noted, consider the following: the book has seven major conclusions and seven tentative hypotheses, clearly identified and numbered in "The Plan" and in the text that follows, and also listed in the "Glossary" (pp. 275–6) and "Index" (pp. 300, 304); only one is about RT—the one stated above—and none refer to Chomsky. Second, Smith himself quotes (p. 445) a passage from *Ignorance* (p. 7) which discusses, on an equal footing, not only the consequences of taking Chomsky to hold RT but also those of taking him to hold that linguistic rules are embodied somehow without being represented. Third, Smith himself notes the passage quoted in (2) above and my remark that I shall take "no firm stand on this matter of interpretation" (p.7). Yet, despite all this evidence, Smith persists with his mistaken construal (p. 446). He thus persists in erecting a straw man to support his accusation that I have erected one.

[1] Unidentified references of this sort to the works of others are to papers in the present volume. Unidentified references to my work are all to Devitt [2006a].

[2] Smith compounds this error by blatantly misrepresenting *my* use of that evidence. He points out, rightly, that in one quote I discuss (p. 4), Chomsky "does not say a speaker/hearer has *propositional knowledge* of rules of the language", but he implies, wrongly, that I misuse the quote to support the view that Chomsky *does* say this (p. 443). In fact, I use the quote to support the view that Chomsky says that a speaker *represents* the rules (as indeed the very part of the discussion quoted by Smith shows!). But I use *other* quotes, to be found in a note just a few lines earlier (p. 3 n. 2), to support the view that Chomsky *does indeed* say that a speaker has propositional knowledge of the rules. These quotes include: "it is proper to say that a person knows that R, where R is a rule of his or her grammar" (Chomsky [1986], 268). In the face of all this evidence, it is simply disingenuous of Smith to ask, "But why should anyone think [RT] is what the Chomskian is committed to?" (p. 444). Does Smith not remember his Fodor?

[3] "The Plan" also offers guidance to those, like Smith, already convinced of the falsity of RT, about which bits of the book to skip (p. 9).

2. *Psychological and Linguistic Conceptions of Linguistics*

Ignorance of Language is concerned generally with the psychological reality underlying language and is concerned particularly with whether the principles and rules (briefly rules) of a language, revealed by its grammar, are part of the psychological reality of its speakers. There is a very fast argument that those rules must be part. It runs as follows: a grammar of a language is simply *about* the psychological reality of speakers; so if the grammar is true then *of course* the rules it posits are psychologically real. A consequence of the "first major conclusion" of my book, in chapter 2, is that this argument is not just fast but dirty. It's dirty because the grammar of a language is *not* about the psychological reality of its speakers, or at least not primarily about that, but rather about the linguistic reality that those speakers produce. I reject the psychological conception of linguistics according to which "linguistics is…concerned with…the cognitive structures that are employed in speaking and understanding" (Chomsky [1975b], 160). I argue instead for a linguistic conception of linguistics.

This controversial rejection did not go down well with my critics. Matthews responds at length with an interesting discussion of the psychological conception. So does Collins, through the persona of "Ling". And Rattan argues for the conception briefly in passing. Yet none of these critics pays much attention to my *argument* for rejecting the psychological conception.

The failure to address arguments against the psychological conception is traditional. Such arguments have been around in some form for more than thirty years. Developed forms appeared in several papers in the 80s: Katz [1984], Soames [1984], and Devitt and Sterelny [1989]. More recently there was Devitt [2003], on which *Ignorance* chapter 2 is based. Yet, so far as I can determine, these arguments have been largely ignored by Chomskian linguists and their philosophical defenders.[4]

Smith breaks with this tradition. He addresses the argument in *Ignorance* at some length but, I shall argue, to strikingly little effect.

There is nothing difficult or profound about *Ignorance*'s argument against the psychological conception, it seems to me. Indeed, in making it I often feel like the child who said that the emperor has no clothes.

The argument is based on three quite general distinctions which are then applied to linguistics. The first distinction is:

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

(For convenience, I focus on the competence to produce certain outputs.) I illustrate this first with the crude example of a blacksmith and the horseshoes he produces. A theory of the horseshoes is one thing, a theory of the blacksmith's competence to produce them another (p. 17). I go on to some more interesting examples: between the theory of chess moves and the theory of chess competence; between the theory of *wffs* and the theory of a logic machine's "competence" to produce them; and between the theory of the bee's "waggle dance" and the theory of the bee's competence to dance. In each example we are clearly dealing with two distinct theories.

[4] My best account of this sad history is on p. 8.

The three more interesting examples illustrate another quite general distinction. In these examples, the *outputs* are rule-governed: their natures are constituted by their place in a "structure" defined by a system of rules. But these "structure rules" may be quite different from the "processing rules" that produce those outputs and the embodiment of which constitutes the competence. These are two different sorts of rules featuring in two different sorts of governing. So:

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

The bee provides my favorite illustration of this. A bee returning from a distant food source produces a waggle dance on the vertical face of the honeycomb. The positioning of this dance and its pattern indicate the direction and distance of the food source. These dances form a very effective representational system governed by a surprising set of structure rules. It is the task of a theory of that system to describe these structure rules. Karl von Frisch worked on this task for decades finally completing his theory in the 60s. He won a Nobel Prize. Here is a description of one of the structure rules of the bee's dance:

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

What von Frisch certainly did *not* win a Nobel for was a theory of how the bee performs this dance. Indeed, the processing rules within a bee that enables it to perform this remarkable feat remain a mystery to this day (pp. 18–21).

In sum, the representational system of the dance is one thing, the bee's internal state of competence to produce the dance, another. Thanks to von Frisch we know a lot about the former; nobody knows much about the latter. We could hardly have more persuasive evidence that these are two very different matters to study.

This having been said, surely the von Frisch's theory of the dance tells us *something* about the internal state that produces it? Indeed it does. After all, the state is *identified* as the one that produces the dance. So the state is one that, performance errors aside, produces dances that are governed by the structure rules discovered by von Frisch. I intro-

duce the technical term "respect" to capture this relation: the 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. So, on the strength of von Frisch's theory we know this minimal claim about the bee's competence: that there is something-we-know-not-what within the bee that respects the structure rules he discovered. But what we don't know is *what* there is in the bee that does this job: we don't know about the bee's processing rules. In particular, we don't know whether any of the structure rules that von Frisch discovered are *also* processing rules for producing the dance. Hence my third general distinction:

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

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" (pp. 21–3).

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" (p. 23).

I then go on to apply these distinctions to linguistics (pp. 23–41). We could sum up that application briefly as follows. (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. (ii) We need a theory analogous to von Frisch's to tell us about a human language. I argue that this is precisely what a generative grammar does. (iii) How does a grammar help us with the theory of the competence? It tells us that there is something-we-know-not-what within the speaker that respects the structure rules described by the grammar. This is the minimal position on psychological reality that I later call "(M)" (p. 57). But the grammar alone provides nothing stronger 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 grammar's rules are *also* part of the psychological reality that produces language. (iv) To move beyond the minimal claim and discover *the way in which* a speaker's competence respects the grammar's rules, we need further psychological evidence. In sum, I reject the psychological conception of linguistics in favor of the linguistic conception.

If the psychological conception of linguistics is to be saved, there must be something wrong either with the three distinctions or their application to linguistics. It's as simple as that. And if the problem is thought to lie not with the distinctions but with their application we need to be shown how human language is relevantly different from the bee's dance. Three of my critics considered in this section, Matthews, Collins and Rattan, do not make a serious attempt to show any of this. However, Smith does.

Matthews: In an excellent earlier paper, Matthews endorsed the psychological conception ([1991], 182). And he still does: "grammars are psychological hypotheses, and hence…linguistics is a subfield of psychology" (p. 465). Yet his views are generally hard to distinguish from mine. One wonders whether, at this stage, our apparent differences over conceptions of linguistics have become merely rhetorical. In any case, I want to emphasize that his discussion in the present paper does nothing to protect the psychological conception from my argument. In the parts of his discussion that may seem relevant, Matthews disagrees with me over point (iii) above of my application of distinctions 1-3 to linguistics: he thinks that (M) does not capture what the grammar alone tells us about psychological reality and proposes an alternative which I shall call "(M*)": this alternative takes grammars "as specifying intensionally the pairing that the language effects and that speakers compute in the course of language processing, albeit only under (significant) idealization and approximation" (p. 464) I shall raise some doubts about this in a moment. What needs to be emphasized now is that, even if he were right, that would not support the psychological conception of linguistics in the face of my argument.

According to point (i) of my application of distinctions 1-3, there is something for linguistics to study other than the psychological reality of speakers: there is a linguistic reality made up of symbols in a language, symbols that are external to speakers just as the bees' dances are external to the bee. And according to point (ii), generative grammars are theories of those symbols. Thus, consider the following, taken from a typical discussion in a syntax text of binding, case and *wh*-movement:

> 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.

As I pointed out, such claims about anaphors, pronouns, verbs, prepositions, and the like "all appear to be concerned, quite straightforwardly, with *the properties of symbols* of a language, symbols that are the outputs of a competence. This work and talk seems to be concerned with the properties of items like the very words on this page" (p. 31). A grammar of a language is a theory of the properties of symbols in the language just as von Frisch's theory is a theory of the properties of the bee's dance.

That positive claim is the most important one to the linguistic conception of linguistics. For, if that claim alone is right then *linguistics is, at least partly, about a linguistic reality* and the implication conveyed by the psychological conception that linguistics is *only* about psychological reality needs to be rejected. Matthews does not mention the arguments for the positive claim nor does he explicitly deny the claim. Indeed, at one point he seems to endorse it: "I also agree with Devitt that linguistics, as actually practiced, is concerned primarily with the products of linguistic competence and only indirectly with the competence itself" (p. 463). Yet he *still* goes on to urge the psychological conception—"linguistics is a subfield of psychology" (p. 465)—without any acknowledgement that, *so far as anything he has said, the linguistic conception is at least part of the truth*!

Turn now to whether the psychological conception is *also* part of the truth. Point (iii) of my application is concerned with this, concerned with what the grammar alone *also* tells us about the competence of speakers. Now, what is not in contention is that the grammar tells us *something* about this: I think it tells us (M), Matthews thinks it tells us (M*). But that difference is not crucial to the dispute over conceptions of linguistics. What is crucial is what the grammar does *not* tell us about competence: it does not tell us that any of the grammar's rules—like those illustrated above—are *also* among the processing rules that comprise the competence; *it does not tell us that any of those rules are psychologically real in speakers*. We need further psychological evidence to discover which processing rules *are* embodied in speakers; that was point (iv). Matthews does not say otherwise and the implication of his discussion of (M*) is that he would accept the crucial negative part of point (iii). I take this negative part to show, in the context, that *the psychological conception is not even part of the truth*.

In sum, the truth of a grammar entails that its rules do govern linguistic reality but does not entail that they govern psychological reality. So the linguistic conception is true and the psychological one false. Matthews's discussion leaves intact everything that is central to the argument for this. He may still feel entitled to insist that *simply on the strength of his (M*)*, the psychological conception is partly right; that, despite the fact that rules of a true grammar may not be psychologically real, it is enough that, according to (M*), the function specified by the grammar is psychologically real. Provided he conceded that the linguistic conception was also partly right, in the way I have described, the difference between us would then be simply rhetorical.

Finally, I turn to the much less important matter of Matthews' criticism of position (M) in favor of (M*). He thinks that (M) is both too strong and probably too weak. He has two reasons for thinking it too strong. First, grammars typically treat some sentences as grammatical that competent speakers cannot process, they are "computationally intractable" (p. 463). He has overlooked the admittedly brief qualification "performance errors aside" in the initial presentation of the minimal position (p. 25). The processing failures that Matthews mentions can be explained away as performance errors (see also pp. 227–8). Furthermore, Matthews own proposal (M*) needs just the same sort of qualification! And Matthews supplies it in a similarly brief way: "albeit only under (significant) idealization and approximation" (p. 464). Second, he points to "the obvious fact" that speakers can process ungrammatical strings (p. 463). But this is irrelevant. (M) is simply a commitment to a mental state that processes grammatical strings. It has nothing to say on a speaker's capacity to process anything else, whether it be ungrammatical strings, body language, jokes, or whatever.[5]

Matthews' thought that (M) is probably too weak arises from a misunderstanding: "Devitt…argues that the grammars attributed by linguists to speakers enjoy only a minimal psychological reality", the reality captured by (M) (p. 462).[6] I don't argue this. I argue that (M) is the only psychological reality we get *from the grammar alone*.[7] With help from psychology we can hope to establish much more, as Matthews rightly notes. Indeed, the whole of my book beyond chapter 2 is devoted to seeing what more we can now establish. The answer, briefly, is "Not very much", although my "first tentative proposal" is that "a language is largely psychologically real in a speaker in that its rules are similar to the structure rules of her thought" (p. 152).

So (M) survives Matthews' criticisms. Let us now consider his (M*). As quoted earlier, (M*) claims that "grammars are standardly taken as specifying intensionally the pairing that the language effects and that speakers compute in the course of language processing" (p. 464). And I think that he is right that they are standardly taken this way; it is, for example, how Collins's character Ling takes them (p. 486). Why are they so taken? Matthews explains: "the functions that the speaker computes in the course of language production and language understanding…are mappings from sounds to meanings or vice-versa, whereas what the

[5] On the understanding of ungrammatical strings, see p. 151.

[6] Rattan has a similar misunderstanding (pp. 510–11).

[7] This is the view that Smith calls "lily-livered" (p. 451). But accepting a robust view of psychological reality on the basis of evidence that does not support that view is not virtuous, it is epistemically reckless. Of course, my "lily-livered" view raises a question, posed nicely by Smith: "How does competence — a mere ability of one who may be totally ignorant of the language — succeed in respecting the rules of the language?" (p. 450). But, strangely, he continues on as if this question, the main concern of the book, is never addressed. Stranger still, Smith poses the question again a few pages later (p. 452) but this time gives as my answer a claim about UG—my "sixth tentative proposal"—that concerns another question altogether. (He goes on to criticize the proposal as "unsupported", overlooking the pages surrounding its introduction where it *is* supported: pp. 256–60.) Finally, strangest of all, he concludes an earlier discussion by tossing off the following remark: "Similarly, his thoughts about how a competence that involves neither knowledge nor representations of the language enables us to produce and comprehend indefinitely many sentences are wildly speculative" (p. 445). Yet, so far as I can see, Smith *never discusses* what I actually have to say on these psychological matters, particularly in chapter 11!

grammar specifies is a function that has as its range…sound-meaning pairs" (p. 464).

This description is puzzling. At best it seems misleading, particularly in its talk of meanings. First, take language production. What a speaker actually computes is a function that maps a *thought* with a sound that expresses it. Of course, the thought *is* meaningful (contentful). And so is the sound that expresses it. Indeed, the thought and sound are, in an important sense, synonymous. But the mapping of a thought to a synonymous sound is not aptly described as a mapping of a meaning to a sound. Next, take the grammar. I responded to a similar claim to Matthews' as follows:

> we surely cannot take the grammar to be a theory of a function for associating sounds and meanings. We might say that the grammar is a theory of a function for generating sentences from a lexicon, where the lexical items all *have* sounds, syntactic characters, and meanings, and where each sentence generated has its sound, syntactic character, and meaning in virtue of the way it is generated from the lexicon. That is very different from a function for associating sounds and meanings. (p. 67)

We might then link the grammar's function to the competence of people who speak the language: that function is "respected" by that competence in that, performance errors aside, that competence at turning thoughts into synonymous sounds produces items generated by that function. That's just a version of (M). In sum, insofar as (M*) is right, it seems to be (M). In *Ignorance* I declared the distinction between (M) and Matthews' earlier position on the psychological reality of language "too subtle for me" (p. 82). I think the same of any distinction between (M) and (M*).

Perhaps I am wrong about (M*). Perhaps the grammar alone justifies a more robust position on psychological reality than (M). No worries. What the grammar does not justify is the view that any of the syntactic rules it posits are psychologically real. Yet it does justify the view that those rules are linguistically real. That is why the linguistic conception of linguistics is true and the psychological one false.[8]

Matthews fires a final shot at the linguistic conception: if it were correct the "learnability considerations" that linguists are responsive to "would be irrelevant" (p. 465). But he is wrong about this. Learnability considerations are evidence for a grammar as I conceive it:

> concerning acquisition, evidence about nature and nurture showing that a language with a certain structure could or could not have been learnt by a person from the "primary linguistic data" is direct evidence for or against any theory that ascribes such a structure to a language that has been learnt by the person. (pp. 32–3)

My argument for the linguistic conception remains intact.

[8] I point out (p. 82n) that a position contemplated by Matthews in another paper ([2003], 200–2) is also hard to distinguish from (M); similarly, the position in Radford [1988]. See also the discussion (pp. 79–81) of the position in Berwick and Weinberg [1984]. All of these positions on the psychological reality of grammars seem far too weak to support the psychological conception of linguistics.

Collins: In the second part of Collins' entertaining and revealing dialogue, "Why Linguistics is Part of Psychology", the character of Ling defends the psychological conception of linguistics. But Ling, like Matthews, fails to address the arguments against this conception: he does not attempt to undermine distinctions 1–3 nor show that they do not apply to linguistics. I shall attend to some of the things that Ling does do. Sadly, many of them amount to serious misunderstandings of the rival linguistic conception.[9]

First, Ling says that the research program that is allegedly guided by the psychological conception "is not just one way of proceeding; it is the fundamental way of proceeding, and the other programs seem to me to be quite moribund in comparison, at least as evaluated according to scientific criteria" (p. 477). There seems to be an implication here that the linguistic conception urges a different research program.[10] But it does not: it is as enthusiastic as it could be about generative grammar. It urges, rather, a different way of conceiving of the generative research program, a different "interpretation" of the theories the program is producing: they should be taken to be about linguistic not psychological reality (pp. 15–16). The program is indeed a great success and it is, of course, typically accompanied by expressions of the psychological conception. But it follows from my argument that this conception is not responsible for the success.

Second, a recurring theme of Ling's remarks is that the linguistic conception arises out of an a priori commitment to folk opinion:

> I can see no reason to a priori constrain inquiry to cleave to the folk understanding of any given range of phenomena. There is no such constraint in physics, chemistry and biology, and nor, in the absence of special pleading, should there be in linguistics. (p. 478)

I couldn't agree more and say so often, including in *Ignorance* (pp. 95–121, 125–6). I do think that the linguistic conception of linguistics is the folk conception but I do not adopt it on that basis. I argue for it by making distinctions 1-3 and applying them to linguistics.

Third, a consequence of my "sixth major conclusion" is that the linguistic reality that linguistics is about is made up of linguistic expressions that share meanings in idiolects, expressions that can mostly be roughly grouped together as languages like English or Chinese (p. 183). And the shared meanings are largely conventional (although partly innate). Related to this:

[9] Ling starts with a long discussion of the alleged "mystery" of language use arising from the "creative aspect" of language (pp. 473–7). I think that such mystery as there is here is not correctly located in language: it is a combination of the mysterious creativity of thought and the general mystery of freewill (pp. 176–7). In any case, these mysteries do not seem to bear on the disagreement over conceptions.

[10] Similarly, Rey seems to think that the linguistic conception yields a different "theory" from Chomsky's which, if it is to be taken seriously, must "have as substantial explanatory power as Chomsky's" (p. 568).

> Acquiring a language is almost entirely a matter of moving, under the causal influence of primary linguistic data that are (performance errors aside) instances of local linguistic conventions, from an innate "initial state" of readiness for language to a "final state" of participation in those very linguistic conventions. (p. 181)[11]

Among the conventions that have to be acquired are the settings of "parameter values". Ling responds to this idea: "parameter setting doesn't require conventions, however we construe that notion; it simply requires the child's exposure to constructions or features" (p. 579). Quite right! But the claim is not that conventions are *necessary* for a child to set parameters. The claim is rather that, *as a matter of fact*, conventions set those parameters. The child comes to have those settings because it experiences the idiolects of many people in its community, each idiolect having those settings. And *it is no accident* that the idiolects have those settings because the settings are conventional in the community. As a result of those experiences the child adopts the settings and thus participates in those conventions. Thus, the 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. "Very occasionally an idiolect's parameter settings may be eccentric but almost always they will be conventional" (p. 181).

Finally, Ling has some provocative things to say about communication. I set these aside until section 4.

Rattan: Rattan's main concern in his ingenious paper is to argue that a speaker's knowledge of her language is propositional knowledge, knowledge-that. I shall consider that argument in section 3. However, he has some negative things to say in passing about the linguistic conception.

First, he claims that if we can justify his propositional view of speaker's knowledge "it will be difficulty to follow Devitt" in this conception (p. 510). Now I do think that if that if we could justify that view, *interest* in the distinction between the two conceptions would diminish. For, if that view were right, at one and same time we would be studying the symbolic system that is the output of the competence and the competence itself which is propositional knowledge of that system.[12] But the diminishing interest depends on the propositional view, and the view is controversial, to say the least. In any case, the distinction remains.

Second, Rattan thinks that the psychological conception will "seem inevitable" "once we realize that the linguistic properties of the outputs must be derived from some features of our mentality" (p.510). I hear this point time and again, often repeated in the one conversation. Many are clearly in the grip of it. So I need to emphasize, what I have said before, that the point is *entirely erroneous*:

[11] A convention is a regularity with some sort of mutual understanding. It is, of course, hard to be more precise; see pp. 179–180 for a discussion.

[12] This parallels my response to the bearing of RT, a related view, on the two conceptions (p. 34).

> Even if symbols had their properties in virtue of certain mental facts that would not make the theory of those symbols about those facts and so would not make the theory part of psychology. Indeed, consider the consequences of supposing it would, and then generalizing: every theory—economic, psychological, biological, etc.—would be about physical facts and part of physics because physical facts ultimately determine everything. A special science does not lose its own domain because that domain supervenes on another. (p. 40)

According to the view urged in *Ignorance*, symbols are *social* entities (pp. 39, 138–9, 155–6). So, they are like the unemployed, money, smokers and the like in having their properties in virtue of environmental, psychological, and social facts. But, manifestly, this does not make sociology and economics parts of psychology.

Another important point needs to be made. A grammar tells us absolutely nothing about the facts in virtue of which symbols have their syntactic properties. So linguistics could hardly be part of psychology even if all of those facts were psychological. *The grammar tells us about the syntactic properties of symbols—see the examples from a syntax text a few pages back—not in virtue of what symbols have those properties.* So the grammar tells us just what the linguistic conception says it does. Investigating the in-virtue-of issue is, of course, very worthwhile, and I have some Gricean suggestions about it in sections 8.4 and 9.5.[13] My present point is simply that this issue is not what the grammar is addressing.

Smith: Smith's main case for the psychological conception and against the linguistic one lies in criticisms of the view that there is a linguistic reality "out there". I will consider this antirealism in section 4. In the present section I will consider Smith's criticism of the "analogies" that I use to draw distinctions 1 to 3.[14] But, first, two comments on other aspects of Smith's discussion.

(i) Smith seems to endorse the just-discussed view that linguistics is about psychology because symbols have their linguistic properties in virtue of certain mental facts :

> A sound's having these linguistic properties consists in there being a relation between the sound and features of a speaker's mind/brain. For this is the only candidate for the other relatum, as Devitt himself seems to admit….

[13] These discussions are referred to in a note to the just-quoted passage from *Ignorance* (p. 40 n. 36). Smith considers that passage, as we shall see in a moment, but must have overlooked the note and the discussions. For, he complains (having just quoted a passage that immediately precedes one of those discussions!): "So where is this perceived structure? In virtue of what does a string of sounds or marks have the structure the linguist describes? We are offered nothing more to go on" (p. 439). Indeed, "we are prevented from asking" these and other questions about linguistic reality. Further, I am alleged to evade these other questions with the "uninformative platitude" that in linguistics we are describing linguistic reality (p. 435). My answers to these other questions, also overlooked, can be found on pp. 25–6 and 31, pages from which Smith quotes elsewhere.

[14] Rey's discussion of linguistic antirealism makes a passing criticism of my analogy with the bee's dance. I shall also consider this in section 4.

> All the fine-grained linguistic detail and richness, which gives sounds their place in a language, are to be found in the representations in the speaker's mind/brain. Why not then concede that it is these domain-specific features of a speaker's cognitive organisation, rather than the brute-physical tokens, that the linguist most needs to focus on….Sure, we can say that physical tokens of sound *have* linguistic properties, but crucially they do so because they stand in important relations to the psychological states of language users. (p. 442; see also [2001], 284).

First, I do not admit what Smith thinks I do. The linguistic conventions that constitute the linguistic properties of a sound are not determined solely by the speaker's mind/brain: other mind/brains and the environment contribute. Much more importantly, even if those properties *were* so determined, this would not make linguistics part of psychology any more than a similar determination makes psychology part of physics. That is the point of my response quoted above.

Smith takes a very dim view of this response. He quotes the part about the consequences of generalizing the view and describes it as "wholly inadequate" (p. 436). Yet nothing he then goes on to say shows that it is inadequate at all! Indeed, what he says is very much in the spirit of the thoughts about supervention and levels that underlie the passage quoted. What is going on? So far as I can see, this is another misunderstanding. Smith responds to the quote as if it was offered as the, or at least an, argument for the linguistic conception. But it isn't. It is explicitly presented as a response to an argument like Rattan's for the psychological conception, an argument proposed by several authors including Smith. My argument for the linguistic conception is to be found in the application of distinctions 1–3 to linguistics.

(ii) Smith draws attention to the fact that speakers would not "hear" (p. 438) or "recognise" (p. 440) sounds as in a language unless they were competent in the language. He often seems to be suggesting that this truism counts in favor of the psychological construal. Suffice it to say that it does not count one bit.

Turn now to Smith's criticism of my "analogies". These analogies are the four examples—horseshoes, chess moves, the *wffs* of a logic machine, and the bee's dance—offered to illustrate distinctions 1 to 3. Strangely, Smith counts only two and has no discussion of chess moves and *wffs* (p 440).

He responds to the horseshoe example with the following passage:

> The analogy is deeply flawed. It is true that once horseshoes have been produced, *they* can exist independently of the blacksmith. We could come upon them without knowing what they were, and begin to fashion all sorts of hypotheses about them. Even if we did recognise them we could describe and study horseshoes without knowing how to make them. But it cannot be like that with the sentences of language. In order to come across them, or recognise them at all, we need to be linguistically competent users of a language who know what they are….Unlike horseshoes, linguistic data are not out there independently of the competence of those who seek them out. (p. 440)

This is baseless. Worse, it is irrelevant.

It is baseless for the following reasons. (a) Once we have produced the sounds and inscriptions of our language they "exist independently" of us just as much as do the bits of iron that are horseshoes: they are all independently identifiable parts of the physical world. (b) Of course, those sounds and inscriptions would not be *sentences* were it not for their relations to minds. But, similarly, those pieces of iron would not be *horseshoes* were it not for *their* relations to minds ([1997], 246–9). A physical object's nature *as* either a sentence or a horseshoe is in this way "mind-dependent". (c) Finally, a theorist observing the role of those sounds and inscriptions in our lives (on which, see pp. 29–30, 134–5) can come up with the very interesting hypothesis that they are pieces of language just as, observing the role of the bits of iron, she can come up with the rather less interesting hypothesis that they are horseshoes. We have been given no reason to believe, and it is implausible to think, that a theorist—say a highly intelligent Martian—who could form the hypothesis about horseshoes *could not* form the one about language if she happened to lack any linguistic competence.

The passage is irrelevant because the point of the example is *simply* to illustrate distinction 1: "The key point is that the 'theory' of the horseshoes is one thing, the theory of the competence, another, because horseshoes are very different from the competence to produce them" (p. 17). Similarly, I later "distinguish the theory of a speaker's competence in a language, a psychological state, from the theory of the outputs of that competence, sentences in the language" (p. 23). *That* is the *only* respect in which the crude horseshoe example is supposed to be analogous to language. Even if Smith's baseless claim about the differences between theorizing about language and horseshoes was right it would not undermine this analogy.

Turn now to the bee's dance. Smith's criticisms of this example include some similar ones to those just discussed, to which I would of course make similar responses. But he introduces a striking new element: "Although Devitt lays great weight on this analogy, the 'language' hypothesis is highly controversial and has very little empirical credibility these days." Smith cites a number of works in support of this and of the alternative hypothesis that he favors: "The lead bee most probably leaves odour trails along the route for the other bees to follow". He goes on:

> The availability of negative evidence, the lack of replication of Von Frisch's results, the proliferation of different versions of the dance signals and their meanings, the counter-evidence from odour-based tests all suggest that Von Frisch's 'language' hypothesis has little going from it….the best hypothesis of what we are looking at is unlikely to have anything to do with representational language. (p. 441)

One marvels at the confidence of these pronouncements, particularly as Smith, once again, ignores the evidence I cite. The citation is to be found in a footnote which reads: "Any skepticism there may have been about von Frisch's discovery should disappear in light of its confirmation by a recent study that involved putting radar transponders on bees (Riley

et al. 2005)" (p 20 n. 3). So what did this very recent study, published in *Nature*, have to say on the subject? Its opening paragraph informs us: "In spite of some initial skepticism, almost all biologists are now convinced that von Frisch was correct" (p. 205). The claim about almost all biologists is supported by seven citations. The skeptics referred to include most of the ones Smith cites. The authors of the study conclude that their results "will also be accepted as a vindication of the von Frisch hypothesis" (p. 207). And they take their experiment to have refuted the odor-trail hypothesis favored by Smith: "There was also, of course, no possibility that [the recruited bees] were following either regular foragers directly, or ephemeral odour trails left in the hive-to-feeder flight corridor by regular forager traffic" (p. 206). Consider also the following assessment of other studies:

> The consistent lesson from these studies is that odors carried by dancers are not sufficient to explain patterns of recruitment. Instead, essentially all experimental results can be accounted for by Frisch's original hypothesis that dancers convey both spatial and olfactory information but can weight one more than the other depending on the strength or reliability of the information. The odor search hypothesis has not been abandoned by its adherents…, but most researchers consider the dance language controversy to have been resolved beyond any reasonable doubt. (Dyer [2002], 921)

A very recent study gives further support to von Frisch and includes the following: "Environmental factors, such as rain and wind, would ensure that deposited scent provides (at best) intermittent and weak sensory signals. Odometry, by contrast, is far more reliable than scent guidance" (Vladusich *et al.* [2006], 1374).[15]

Now, of course, all these entomologists may be wrong about the state of play in their field and wrong about what their own studies show. But it rather looks as if Smith's claim that von Frisch's hypothesis "has very little empirical credibility these days" is, as Mark Twain said of a report of his death, "greatly exaggerated".

In any case, Smith's discussion is, once again, largely irrelevant. For, the only point of the dance example is to illustrated distinctions 1 to 3 and it could do that *even if von Frisch were not right*. Thus, it would illustrate distinction 1 as well as the horseshoe example does even if Smith's theory of the dance were right: that theory would still be a very different theory from any theory that we may some day come up with of the bee's competence to dance. The illustration of distinctions 2 and 3 would be a bit more subtle. Only something governed by structure rules, like chess or a representational system, can illustrate these distinctions. And if von Frisch is right, the bee's dance is such a system and illustrates the distinctions in the following way: the dance has certain structure rules and yet it remains an open question what processing rules produce the dance (distinction 2); and, whether or not those structure rules are among the processing rules, they are certainly "respected" by the processing rules

[15] I am indebted to Michael Tetzlaff for these references.

(distinction 3). So if von Frisch is right, the dance provides a very nice *actual* example that illustrates the distinctions. And, I confess, I would be disappointed if von Frisch were not right. But *it would not matter to my argument*. It is enough for the dance example to illustrate the distinctions that it provide a *possible* example: suppose that the bee's dance *were* the representational system von Frisch describes then it *would have* structure rules that might not be processing rules but that *would be* respected by the processing rules. Thus, the possibility illustrates the distinctions and thus, *and only thus*, is the dance supposed to be analogous to language. (A possibility based on von Frisch's hypothesis, a piece of serious science if ever there was one, should seem "more real" than the likes of Twin-Earth and Swampman.) And even if this illustration fails, we could always come up with other ones, as I did with the examples of chess moves and *wffs* that Smith ignores. The distinctions do not depend on any one illustration.

As I have emphasized, my argument for the linguistic, and against the psychological, conception of linguistics rests on the application of general distinctions 1–3 to linguistics. It is not clear to me whether Smith intends the differences he alleges between language and the two "analogies" he discusses to undermine the distinctions or, rather, their application to linguistics. In any case, if my response is right, his discussion undermines neither. However, he also has in mind something else that does seem to undermine the application to linguistics: linguistic antirealism. We shall consider this in section 4.

Finally, Smith raises a good question: "if the leading practitioners in linguistics take themselves to be studying speaker's competence, why not leave it to the experts to settle the subject matter of their own discipline?" (p. 442). I certainly think that "linguistics, like other sciences, largely determines its own domain" (p. 27). And we would normally rely on the practitioners of a successful science to tell us what that domain is. Still, practitioners can be wrong about this as we can all be about anything. And I have produced an argument to show that Chomskian linguists are wrong about the domain of linguistics.[16] If the argument is right, there is something decidedly odd about the situation in linguistics. But, then, odd things do happen.

To conclude this section, a grammar is about the linguistic outputs of a linguistic competence in that the grammar entails that the rules it posits govern those outputs. Although the grammar tells us something about competence—the minimal position (M)—the grammar does not entail that the rules it posits govern that competence. So the linguistic conception is true and the psychological one false. *Prima facie*, the idea that grammars are about psychological reality has little merit. The four papers discussed in this section do not, it seems to me, add to the merit.

[16] And, we should note, there are other linguists who do not agree with the Chomskians; for example, Gazdar *et al.* [1985], 5.

3. *Ignorance of Language*

A main theme of my book, reflected in its title, is that "a person could be competent in a language without representing it or knowing anything about it: she could be totally *ignorant* of it" (p. 5); her knowledge of her language is mere know-how, a skill or ability (ch. 6). On the one hand, as already mentioned (sec. 1), my second major conclusion rejects RT, the thesis that "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" (p. 273). So a speaker can be ignorant of the syntactic rules (hence of the grammar). On the other hand, I argue that a speaker could be ignorant of particular syntactic facts because her intuitive judgments about such facts do not come from her competence (ch. 7). Matthews, Rattan and Smith take issue with my ignorance theme.

Matthews: Matthews does not resist my view that linguistic competence is not "knowledge of a grammar or semantic theory". But, nonetheless, he thinks that "propositional knowledge *is* constitutive of linguistic competence": "What a competent speaker minimally has to know…is the pairing of sounds and meanings that his or her language effects" (p. 461). I shall first consider his response to my arguments for ignorance and then look critically at this proposal.

Matthews takes my argument for ignorance to presume

> the so-called Representational Theory of Mind [RTM], according to which someone possesses an attitude A towards some proposition p (e.g., knows that p) just in case that individual has a mental representation that has the propositional content p and which plays the appropriate causal role in the possessor's psychological economy….Devitt's argument is simply this: if there are no representations, there are no propositional attitudes. And if there are no propositional attitudes, then there is a fortiori no propositional knowledge, i.e., no knowing-that, and no cognizing-that either, for that matter. But there are no representations; hence, there is no knowledge-that. (p. 466)

Since Matthews is no fan of RTM, he is not bothered by this argument for ignorance.

RT has two parts, the first taking a competent speaker to have a propositional attitude to linguistic rules, the second, a representation of them. Now, it is true that the *focus* of my argument against RT is against its second, representational, part, not its first, propositional-attitude, part. And it is also true that I favor RTM. So I *could have* argued for ignorance of the rules along the lines Matthews suggest: that is, presuming RTM, the rejection of the first, propositional-attitude, part of RT follows from the rejection of the second, representational, part. But, so far as I can see, I *never do* argue for ignorance like this. And Matthews gives no references to support his view that I do. In any case, what is certain is that this RTM argument is not central to my case for ignorance of the rules.

Here is a summary of the case. First of all, I find no support for the view that our knowledge of language is propositional in (a) the rejection of behaviorism (ch. 5), (b) the acceptable folk view that a competent speaker of a language "knows" the language (ch. 6), nor (c) the evidential role of intuitions (ch. 7). So, the discussion of (a), (b) and (c) reveals no threat to the view that knowledge of a language is a skill or ability, mere knowledge-how not knowledge-that. But this is really just an appetizer to the main argument for ignorance which occupies most of chapter 11. That argument draws on psychology to argue, first, that "our linguistic competence has all the marks of a cognitive skill" (p. 210). It then goes on to consider what we can learn about the nature of this competence from the general psychology of skills and their acquisition. The focus is on evidence that this nature is not likely to involve representations of the rules governing the skill, but the argument also shows that it is not likely to involve "declarative", hence propositional, knowledge of the rules; it is likely to be simply "procedural" knowledge acquired by "implicit learning" (pp. 210–19). This prediction is confirmed by the study of language use that follows (pp 220–41). In sum, my real argument for ignorance of the rules rests mainly on empirical psychology not on RTM.

What about my argument that a speaker might be ignorant of syntactic facts? This argument, to be considered later in this section, makes no appeal to RTM.

Turn now to why Matthews thinks the competent speaker cannot be ignorant of her language. He adduces two considerations.

> First, it seems that we cannot explain our rational reliance on language as a reliable means of communication if we can't attribute to competent speakers certain knowledge about their language. Language is a reliable means of communication precisely because we know what we and others are saying when we utter the things that we do. But we can't be said to *know* what we or others have said in uttering the sentences that we do unless we know inter alia the pairing of sounds and meanings that we effect in speaking the language that we do. (p. 467)

This is not convincing. First, a quibble. What speakers and hearers must do in communication, strictly speaking, is pair *thoughts with synonymous sounds*. Next, why must that pairing involve any propositional knowledge about the pairing? I don't doubt that anyone who expresses a thought using the appropriate English sound, who has a concept of meaning, and who reflects upon what he has done, might be likely to conclude that that sound is synonymous-in-English with the thought. But why does competence demand that he draw this conclusion? Communication simply demands that he match the sound to the thought and he could surely do that without ever having had a thought about meaning in his life. Couldn't the pairing be a fairly brute-causal process? My "fourth tentative proposal" is that it is:

> The speedy automatic language processes arising wholly, or at least partly, from linguistic competence are fairly brute-causal associationist processes that do not operate on metalinguistic representations of the syntactic and semantic properties of linguistic expressions. (p. 276)

My argument for this proposal (pp. 220–43) is certainly far from conclusive; that's why the proposal is tentative. Still the proposal seems to me more plausible than the view that this communication process must involve propositional knowledge. At least, we need an argument that it must involve this. Perhaps Matthews' book (forthcoming) will provide one.

Matthews' second consideration is even less persuasive. It has to do with

> how commonsensically we understand competence: competent individuals just are individuals who are knowledgeable about whatever the competence is a competence for, and it is this knowledge that is constitutive of the competence in question…The knowledge in question counts as *propositional*, or at least an instance of knowledge-that, simply because when we endeavor to say with any precision just what this knowledge constitutive of the competence is, we find ourselves forced to the knowledge-that locution. (pp. 467–8)

Even if he were right about our commonsense view of competence it is hard to see why we should place much weight on the view simply because it is commonsense. I join with Collins, as already noted, in seeing "no reason to a priori constrain inquiry to cleave to the folk understanding of any given range of phenomena" (p. 478). Although commonsense is often right, it is also often wrong. In any case, Matthews is surely not right about commonsense. The folk think of their competence to swim, ride a bicycle, touch type, think, and so on as "mere know how", not involving much if any propositional knowledge. And if the folk didn't think this, then they would almost certainly be wrong. Thus, consider one of my favorite examples, the competence to catch fly balls:

> An essential part of this skill is being at the right place when the ball descends to catch height. An experiment showed that skilled fielders "ran at a speed that kept the acceleration of the tangent of the angle of elevation of gaze to the ball at 0" (McLeod and Dienes [1996], 531). (p. 50)

It is not known how they manage this but they surely don't have propositional knowledge about it. And consider the competence of insects. The bee is competent to dance but surely does not have propositional knowledge about the dance. And the folk surely do not think otherwise.

Rattan: Rattan does not criticize my argument for ignorance of the rules[17] but rather offers an argument for the contrary conclusion, "the Cognitivist Thesis" that "knowledge in language is a kind of knowledge-that" (p.507). He bases his argument on a brief passage in Chomsky [1986] (pp. 9–10) nestled between passages that I criticize in chapter 6, passages arguing that knowledge of a language is not an ability or knowledge-how (p. 516).[18] Chomsky's passage discusses three sentences:

[17] Which may be just as well because his interpretation of this argument—the one I summarized in discussing Matthews—and of the related argument against RT is sadly flawed (pp. 510–13). My argument is to be found in chapters 5, 6, 7, 11 and, to a small extent, 12.

[18] Smith makes approving mention of this argument of Chomsky's (pp. 449–50) but fails to take any note of my criticisms of it.

(1) his wife loves her husband
(2) John is too clever to expect us to catch Bill
(3) John is too clever to expect us to catch

Rattan takes Chomsky to be making three points about our responses to these sentences:

> First, he seems to be saying that an uncertainty or mistake in judgment about the grammatical properties of certain sentences can be settled or corrected, or more generally improved, by "some thought" or reflection of the case. Second, he thinks that this phenomenon of improvement in judgment through reflection requires a stability in knowledge of language. And third, he seems to think that this stability goes beyond any that the view of knowledge of language as knowledge-how can provide. (p. 516)

Rattan argues that the stability does indeed undermine the knowledge-how view.

Rattan's argument rests on the crucial assumption that the improved judgments are cases of knowledge "made explicit through reflection" by one of two processes:

> *Either* that *propositional* content available at some tacit level becomes *available* in explicit form in consciousness, *or* that an essentially *practical, non-propositional ability* somehow *transforms* into propositional shape in coming to explicitly form in consciousness. (p. 517)

These two alternatives are versions of the received view of intuitive linguistic judgments according to which, as I like to put it, those judgments are "the voice of competence". On this received view, the intuitions reflect *information provided somehow by competence*, whether that competence is knowledge-that or knowledge-how. Rattan then argues that the information could not be provided by competence if it were knowledge-how. So competence is not knowledge-how. So, given the crucial assumption, competence must be knowledge-that. His Cognitivist Thesis is established and my ignorance theme is mistaken.

The problem with Rattan's argument is the crucial assumption. I devote a chapter (ch. 7; see also Devitt [2006b]) to arguing against the received view that intuitions are the voice of competence (in any version) and hence, in effect, against the crucial assumption. I sum up as follows:

> I argue for a different view based on a view of intuitions in general. Linguistic intuitions do not reflect information supplied by represented, or even unrepresented, rules in the language faculty. Rather, they are empirical central-processor responses to linguistic phenomena differing from other such responses only in being fairly immediate and unreflective. (p. 10)

That is the main part of my "third major conclusion" (pp. 95–121). (The other part is that intuitions are not the main evidence for grammars.) On this modest view—modest because it makes do with cognitive states we were already committed to—*the importance of competence to intuitions is not as a source of information but as a source of data*. A person's

competence provides evidence of what she would say and understand. Her central processor provides intuitive opinions by reflecting on such data. "In particular, the grammatical (sometimes partly grammatical) notions that feature in these judgments are not supplied by the competence but by the central processor as a result of thought about language" (pp. 110–11).[19] Whereas on the received voice-of-competence view, competent speakers must know the syntactic facts captured by their intuitions, on the view I urge speakers could be entirely ignorant of their language (although, of course, they mostly aren't).

My view accommodates the first point that Rattan takes from Chomsky: that reflection can revise an initial judgment. It can go along with the second point's presumption that there is stability in knowledge of language during this revision. But it rejects the third point: competence's role of providing data for central-processor reflection is perfectly compatible with the view that the stable state of competence is mere knowledge-how and hence that speakers can indeed be ignorant of their language. Rattan's contrary view rests on a mistaken view of intuitive linguistic judgments.

Smith: Smith subscribes to the received voice-of-competence view of linguistic intuitions: "Unconscious, information-bearing states of the language faculty gives rise to conscious knowledge that is immediately reflected in the speaker's intuitive linguistic judgements" (p. 444). Still, he has trouble with the version of that view that I call the "standard" one. This version is a development of Rattan's first alternative, the knowledge-that one, and entails RT. According to this development, speakers *derive their intuitive judgments from their representations of rules* by a causal and rational process like a deduction. I cite a lot of evidence to support the claim that this is the standard version (pp. 96–7). This evidence includes a passage from Chomsky ([1986], 270) that is about as explicit a statement of this version as one could wish for. Smith quotes the passage but then goes in for some special pleading that Chomsky couldn't really mean what he says: "Chomsky's picture is surely different", and so on (p. 444). And maybe Smith is right and Chomsky does not hold to RT despite many passages, like the one Smith quotes, where he seems to. As I noted in section 1, I take no firm stand on this interpretative issue. But the trouble then is: *What are we to make of the voice-of-competence view without RT*? The standard version of the view is certainly lacking in details: "We would like to know about the causal-rational route from an unconscious representation of rules in the language faculty to a conscious judgment about linguistic facts in the central processor" (p. 96). And it has other problems (pp. 100–3, 114–16). Still, if one could swallow the implausible RT, which Smith rightly can't, we would have the beginnings of an explanation of intuitions, because the idea of one sort of representation leading to another is familiar. But if RT is rejected, then we must look for what I call a "nonstandard" version of the voice-of-competence view. This must be a development of Rattan's second alternative, the knowledge-how one: the intuitions must be provided somehow by embodied but unrepresented rules. And the first problem is that, so far as I know, no such development has ever been produced (pp. 96–8).

Smith has a breezy approach to this. He tells us that the psychological process by which the underlying knowledge produces the intuitions is *not like* "a derivation in a generative grammar". So what *is* it like? Well, Smith accepts that mentalists don't have an explanation but, no worries, that's someone else's problem: "It is a task for the psycholinguist to tell us how the actual processing story goes and how it enables the speaker to arrive at his judgements given the linguistic information he possesses." (p. 446)

I think Smith is way too sanguine about this, for reasons set out in another discussion he overlooks (pp. 117–19). There are reasons for thinking that no such explanation will be forthcoming. This is the second, and more serious, problem for a nonstandard version. If the explanation is to be a genuine voice-of-competence view, and hence sustain the view that a competent speaker cannot be totally ignorance of her language, then it must not collapse into my modest explanation. The nonstandard explanation "would require a relatively direct cognitive path from the embodied rules of the language to beliefs about expressions of that language, a path that does not go via central-processor reflection on the data." And the problem is that it is hard to see what that path could be:

> Consider some other examples. It is very likely that rules that are embodied but not represented govern our swimming, bicycle riding, catching, typing, and thinking. Yet there does not seem to be any direct path from these rules to relevant beliefs. Why suppose that there is such a path for linguistic beliefs? Why suppose that we can have privileged access to linguistic facts when we cannot to facts about these other activities? (p. 118)[20]

[19] In his defense of the psychological conception of linguistics, Collins' Ling has a lot to say about the data, including intuitions, for a grammar (pp. 480–6), much of which I agree with. The intuitions I am discussing are, of course, judgments. I follow convention in taking judgments to involve propositions, in this case, metalinguistic propositions (p. 95). Ling has a Humpty-Dumpty response to this: "That is not what I mean by 'judgment'. Just take judgment to be something like 'interpretation'. We are interested in how speaker/hearers' interpret strings, either their own or those of others." A little later, Ling talks dismissively of "the absurd idea that we are after speaker/hearers' explicit propositional judgments on the linguistic status of strings". What Ling is calling interpretations are facts about language use. I also emphasize the importance of these facts as evidence (pp. 98–9). But what he dismisses as an absurd idea is surely not so: intuitive propositional judgments are also evidence. In any case, what he dismisses is the received view in linguistics; see the works cited (pp. 95–7).

[20] Smith resists my use of "privileged" to describe this access (p. 448) but our difference here seems to be merely verbal. As we have noted, Smith holds to the voice-of-competence view: "the language faculty gives rise to" the intuitions about syntactic facts. It is that sort of access to syntactic facts that I am calling "privileged" to contrast it with the access to facts that we have through normal empirical investigation (p. 96).

The skilled catcher of fly balls is governed by rules that keep the acceleration of the tangent of his angle of elevation to the ball at zero but he has no access to this information: the underlying state is cognitively impenetrable. Why suppose that a speaker has any better access to the underlying rules governing her linguistic activities? In the absence of even the beginnings of an answer to such questions, we have no serious rival to the modest explanation of linguistic intuitions: they are ordinary empirical judgments not the voice of competence.

Of course, Smith believes in the psychological conception of linguistics, which gives him the psychological reality of the linguistic rules for nothing: the fast and dirty argument (sec. 2). So doubtless he thinks that these embodied rules *must* supply the intuitions. But if my argument is right, even if those rules are embodied—and my first tentative proposal is that they are (p. 276)—we have no reason to think that they supply the intuitions. Furthermore, the evidential role of the intuitions gives us no reason to think that the rules are embodied.

Miščević: Miščević leans hard on my view of linguistic intuitions. In particular, he focuses on my claim that these intuitions are theory laden, and on what happens in the process of forming one. His discussion sharpens the issue in an illuminating way. I need to say more about my view.

On my view, linguistic intuitions are just like the intuitions we have about other aspects of the world, particularly like the intuitions we have about the outputs of other human competencies; for example, intuitions about touch typing and thinking. So, in the typical situation, an ordinary person asked about a certain string of words will first simulate the behavior of attempting to produce or understand the string, thus exercising her linguistic competence. She will then go in for some quick central-processor reflection upon this experience, deploying her concept of *grammaticality*, *acceptability*, or whatever from folk linguistics, to form a judgment.[21] The judgment itself is propositional, of course, but the datum for the judgment is not. The datum is the experience of simulating the behavior, which is no more propositional than is an experience of actually producing or understanding a string in normal language use (pp. 109–11).[22] So competence supplies the datum for the intuition, the central processor provides the intuition.

Miščević has a different view, a voice-of-competence view. So, on his view, a competent person can't be ignorant of her language. Where I posit two stages in forming an intuition, he posits three. My first stage,

[21] If the person is a linguist then she will of course deploy her concepts from her linguistic theory. So Miščević gets me a bit wrong in describing my view as "that intuitions are *direct products of folk science*" (p. 527). Indeed, I think that we should generally prefer the intuitions of linguists to those of the folk in seeking evidence (p. 111).

[22] At one point, in a passage quoted by Miščević (pp. 59, 540), I carelessly misstated the view as follows: "So she asks herself whether this expression is something she would say and what she would make of it if someone else said it. Her answer is the datum" (p. 109). Her answer is not: it is part of the central-processor reflection. The datum is the experience that the answer is about.

the act of simulation, is his stage 1. My second stage, the central-processor reflection, is his stage 3. In between these two he inserts stage 2. This stage is the "immediate spontaneous verdict", a judgment delivered solely by the competence (p. 530). "In the language case, *the immediate product is already a judgment*" (p. 547). This judgment is the datum for theorizing by the central processor but itself precedes any theorizing: "If the datum is the result of simulation, there is *no* theorizing involved. The simulation itself yields the verdict". This judgment at stage 2 is properly thought of as the intuition and, in many cases, the central processor will simply pass it on without alteration at stage 3: "It is the Competence itself that is doing the work, the central processor at best just passively reports the verdict of the competence, which is the intuition" (p. 528).

Miščević's view is motivated by the following criticism of my view: "just rehearsing a sentence in the head, and finding it okay is not *theorizing*, by any stretch of meaning of 'theory'" (p. 541). But this criticism conflates the idea that a judgment is *formed by theorizing* with the idea that it is *theory-laden*. My claim about intuitions is the latter not the former. These intuitions are theory-laden in the way observation judgments are in general. The anti-positivist revolution in the philosophy of science, led by Thomas Kuhn and Paul Feyerabend, drew our attention to the way in which even the most straightforward of judgments arising from observational experiences are theory-laden in that we would not make them if we did not hold a range of background beliefs and theories, some involving the concepts deployed in the judgments; in brief, there is no "given".[23] This is not to say that we consciously bring this range into play in a way that amounts to *theorizing* about the experience. Surely, we mostly don't. Nonetheless, this range plays a role in the judgment as a causally necessary background.[24] Replace the talk of observational experience with the earlier talk of simulation experience and we arrive at my view of the theory ladenness of intuitive linguistic judgments: we would not make these judgments in response to simulation experiences if we did not hold a range of background beliefs and theories, some involving linguistic (or quasi-linguistic) concepts.

[23] [1997], 144–7. This *epistemic* theory-ladenness is not to be confused with *semantic* theory-ladenness, the view that the meaning of an observational term is determined by the theory containing it. This "semantic holism", also part of the revolution, has little to be said for it in my view ([1996], 87–135).

[24] Miščević's discussion of chicken sexing (p. 543) provides a nice example: "The conceptualization and thereby theory involved in it is minimal, limited to the application of concepts MALE/FEMALE". Right: the theory involved may well be minimal but there is theoretical involvement nonetheless. And Miščević is also right in implying that the sexer's judgment is not "theoretical": it is observational, but still theory-laden. And he is right in claiming that the judgment is "a relatively direct product of [the sexer's] competence". But he is wrong to draw an analogy between that competence and linguistic competence. For, the sexer's competence simply *is* the competence to come up with true judgments about the sex of chickens whereas linguistic competence is the competence to process language. Whether linguistic competence is *also* the competence to come up with judgments about the syntax of sentences is precisely what is at issue.

So, why should my modest view be preferred to a voice-of-competence view? As already noted, there are two versions of the latter view, the standard one committed to representations of rules in the language faculty—RT—and the nonstandard one committed only to those rules being embodied. My case against the standard one is an inference to the best explanation. I argue that my view can explain linguistic intuitions and their use as evidence (pp. 108–14). And I argue that it is better than the standard explanation for several reasons. (i) If competence really spoke to us, why would it not use its own language and why would it say so little (pp. 100–03)? (ii) There would be a disanalogy between the intuitions provided by the language faculty and by perceptual modules (p. 114).[25] (iii) There would be problems arising from the differences between the intuitions of the folk and the linguists (p. 115). (iv) If represented rules in the language faculty provided the linguistic intuitions they would surely also govern language use and yet there is empirical evidence that they don't do both (pp. 115–16). (v) Finally—and this is the consideration to which I give most weight—there would be "the extreme *im*modesty" of a view committed to RT, a bold and, my book argues, implausible and otherwise unsupported claim about the mind. What about the nonstandard voice-of-competence view? I could have adapted problems (i) to (iv) to count against this version too. But the main problem with this view is the one indicated in discussing Smith: we do not have even the beginnings of a nonstandard explanation of linguistic intuitions and there seems to be little likelihood that one will be forthcoming (pp. 117–19).

Miščević discusses all these arguments but does not undermine them, it seems to me. In particular, although he finds the nonstandard voice-of-competence view "very appealing", he does not address its central problem (p. 547): How *could* embodied but unrepresented rules provide information for the central processor to pass on?

Finally, Miščević offers a misguided *ad hominem* argument. The English sentence

(1) Tex likes exciting sheep

Is clearly syntactically ambiguous. We can paraphrase its two meanings as follows: "Tex enjoys causing sheep to become excited"; "Tex enjoys the company of exciting sheep". If the language-of-thought hypothesis ("LOTH") is correct, (1) could express either of two "mental sentences" which, I point out (p. 153), might "look somewhat like":

[25] This criticism is part of a response (pp. 112–13, 114, 118) to a defense of the voice-of-competence suggested to me by Rey. The defense claims that these two sorts of intuitions *are* analogous. Rey develops this defense in his paper in this volume (pp. 564–8). I accept an analogy between intuitive judgments based on deliverances to the central processor by the visual module and by the language module: the former judgments are of what is seen and the latter of what is said. But intuitions about *what is said* are not the intuitions that concern us, for they are not intuitions about *the syntactic and semantic properties of expressions*. In my view, Rey's interesting discussion of the outputs of the visual module does nothing to show that *these intuitions that concern us* are analogous to perceptual intuitions.

(2) $[_{S}[_{NP}[_{N}\text{Tex}]][_{VP}[_{V}\text{likes}][_{VP}[_{V}\text{exciting}][_{N}\text{sheep}]]]]$
(3) $[_{S}[_{NP}[_{N}\text{Tex}]][_{VP}[_{V}\text{likes}][_{NP}[_{A}\text{exciting}][_{N}\text{sheep}]]]]$.

According to Miščević, "Devitt seems here to be offering very persuasive evidence against his own theory". Why? Suppose that Helen has the intuition that (1) has two meanings captured by the above paraphrases. According to Miščević, this intuition "is *read off from the representation of the sentence in LOT*. No additional 'theorizing' is needed" (p. 543). But there is no "reading off" of information about the ambiguity of (1). An intuition is a thought. So, in forming her intuition abut ambiguity, as in forming any other thought, Helen will represent its content in her LOT. But prior to forming her intuition she had no such representation and hence *could not* form the intuition by "reading" that representation. What she may well do in forming her intuition is entertain two thoughts *about Tex*, hence represent their content with mental sentences "looking like" (2) and (3). Miščević goes on: "The question is then whether the Mentalese sentences (2) and (3) are accessed by the CP. It is incredible that they are not, since we are indeed, thinking in Mentalese sentences; how could we do it if we didn't have access to them" (p. 543). This confuses thinking *in* a language with thinking *about* the language. We have access to the language we think in only in that we do think in it. We have no special access to the *properties* of that language, no access that would provide us with thoughts about the syntax of that language. (And if we had, assessment of the controversial LOTH would be easy: all we would have to do is look inward and we could *just see* whether the system of mental representations in which we think is language-like rather than, say, map-like!)

In conclusion, the arguments of Matthews and Rattan that a competent speaker must have propositional knowledge of her language are unconvincing. Smith does not offer an account of how rules embodied in the language faculty *could* provide intuitions about syntactic facts. Nor does Miščević, despite his helpful criticisms of my alternative view. It seems to me that my central arguments that the speaker could be totally ignorant of the rules (ch. 11) and syntactic facts (ch. 7) of her language remain intact.

4. *Linguistic Antirealism*

Rey: The linguistic conception of linguistics, defended in section 2, presupposes linguistic realism: it presupposes that sounds, inscriptions, and the like, really have phonological, syntactic and semantic properties. In a previous work, Rey argues for the contrary view, claiming that these linguistic entities, which he calls "SLEs", do not exist: they are "intentional inexistents" [2006]. Rey rightly thinks that linguistic antirealism is common in linguistics. I criticize Rey's version of it in *Ignorance* (pp. 184–9).[26] Rey's present paper responds to those criticisms.

[26] Alex Barber's "Testimony and Illusion", in the present volume, offers a different criticism.

Rey's position raises two interesting questions. (I) Why would anyone think that SLEs do not exist? (II) Why is it important to suppose that they do exist? I shall consider them in turn.

(I) Rey does not of course deny the existence of the sounds etc. that are *naturally thought* to be SLEs. He just denies that they have the properties that would make them SLEs. Thus, linguistic texts tell us that a sentence has "an elaborate tree structure".

> But what thing in space and time possesses such a structure? Not, evidently, any *noises* anyone makes: none of the wave forms produced by people when they speak have a tree structure in the way that, for example, a real tree, or river, or network of neurons might, or (to take an example of artifact for which a type/token relation could be defined) in the way that parts of an automobile have the structure of an internal combustion engine. (p. 556)

This is strange. The trees that appear in linguistic texts are simply *ways to represent perspicuously* the syntactic properties of expressions, properties that can be, and often are, represented in other ways. So, *of course* linguistic realists are not committed to the noises we make having tree structures the way real trees do! They *are* committed to noises and other SLEs having the properties *represented* by structure trees. And it is quite beside the point that it is not superficially evident that SLEs have such properties, that, as Smith says, "they are not observable or intrinsic properties of the speech sounds speakers produce" (p. 438).[27] Such properties are *relational* and, as I emphasize using the example of *being Australian*, we cannot simply observe whether an object has a relation property (unless it is a response-dependent property) and yet all objects have many of them (p. 185); objects do not, we might say, "wear their relational properties on their sleeves". SLEs are social objects like the unemployed, money and smokers (sec. 2 above), all of which have defining properties that are, at least partly, relational. So too are the defining properties of Chomskians, cars, paperweights, moons and echidnas. Yet, I hope we can agree, all those objects exist.

Sometimes relational properties are correlated well with superficial properties and hence their presence is easily detected, but sometimes they are not (pp. 185–6). Thus it is fairly easy to detect money, cars and echidnas, but not so easy to detect Australians, the unemployed, smokers, Chomskians, paperweights and moons. And it is mostly easy to detect SLEs because they are established by conventions that correlate linguistic properties with superficial ones. The clues to a linguistic

[27] C.f., Collins' Ling who supports his antirealism with the following remarks: "Sorry, I just don't get this idea of looking at language. Where are languages such that one can look at them directly?" (p. 481). "Needless to say, such an explanation essentially appeals to items and structures that are not marked in the strings themselves. Looking at strings gets you nowhere." He goes on to claim that we "project" meanings, conventions, and language; that "it doesn't matter if there are such things out there, for they would be completely inaccessible to us—reality isn't all it's cracked up to be"; and finally—mercy! mercy!—tells philosophers to "read more Kant" (p. 489). For some reasons why we should read less, or at least take less notice, see my [1997].

property are clues to it because conventions bestow that property on objects that provide those clues.

Rey believes that cars exist and so is naturally concerned to distinguish them from SLEs. So what is the difference? Cars have "elaborate internal structure" and "it's by virtue of their actual structure that they move as they do. SLEs, I've argued, do not" (p. 570). This is, indeed, a real difference. Cars have to have a certain internal structure in order to function the way we intend them to but SLEs do not. Although typically SLEs are sounds and inscriptions, just about anything could be an SLE at the cost of some inconvenience. But similarly, just about anything could be Australian or a vote, a vast variety of small objects could be paperweights, almost any human could be a cleaner, and so on. The extent to which an object's having a relational property depends on its intrinsic properties varies greatly from relational property to relational property. But these differences among relational properties are *simply irrelevant* to the ontological issue that concerns us. Some things really are Australian, votes, paperweights, and cleaners, however little their intrinsic natures have to do with their so being.

Rey thinks that phonology also implies antirealism. He draws attention to the

> peculiarities of the relation between the acoustic stream and the intended phonemic sequence that render the identification of SLEs with physical phenomena at least extremely awkward. Most telling for our purposes is "displacement," whereby the "cues" for a particular phoneme are often presented not at the place at which the phoneme is "heard," but at the place of some earlier phoneme. (p. 556)

Later he emphasizes how our understanding of sounds depends on their context:

> pronunciation of phones in isolation does not correspond to what is pronounced in normal speech: the phones /k/, /a/, and /t/ produced separately and spliced together are not readily recognized as the word "cat"….And efficient "top-down" processes "fill in" phones that are demonstrably absent, as when mere silence between the "s" and the "l" in "slit" is heard as "split". (p. 562)

But, as I point out in a passage that Rey quotes, this "shows that there are many complicated ways in which sounds can instantiate a phoneme, including relations to other sounds; and that a sound may be able to instantiate more than one phoneme" (p. 186). But it no more shows that there are not really any phonemes than similar complications and variations in being Australian show that there are not really any Australian entities.

There is a related concern that is worth attention. A symbol can be complex in that the message it represents is determined by simple symbols in a structure. That is the case with sentences. But this does not require that each aspect of the message is "correlated" with a simple symbol. In particular, the structure of a sentence can be partly implicit; it can have constituents that are not overtly realized. One thinks im-

mediately of the theoretically interesting PRO, but ordinary ellipsis is an easier example:

(I) Mary went to visit the zoo, John, the museum.

How is it possible for syntax and meaning to be implicit? The simple answer is: *there is nothing to stop there being a convention of this sort*. Some parts of the acoustic stream or inscription that constitute a sentence are words, which have linguistic properties of their own. In virtue of those properties, and the physical arrangement of the parts that have them, larger parts of the stream or inscription have other linguistic properties; for example, the properties of being an NP or a sentence. There is nothing to prevent one of those other properties, arising from a particular arrangement, being one that the larger part has *as if* it had a part with a certain property even though it does not in fact have a part with that property. Thus,

(E) Mary went to visit the zoo and John went to visit the museum

has certain linguistic properties in virtue of explicitly containing 'went to visit' that the elliptical (I) has only in virtue of implicitly containing those words. Thus (I) is covered by a convention according to which it has a syntax and semantics as if it had the missing words. There is nothing in principle problematic about this. Indeed, think of the conventions for conveying complex messages with a simple symbol like a flag: a yellow flag on a ship's mast, meaning *This ship has yellow fever*, has the property of referring to yellow fever even though there is no one spot on the flag that does so. We can create conventions that make structure as implicit or explicit as practicality dictates.

Rey also draws attention to variability within a speech community:

> In additional to dialectical and regional differences, there are differences merely in pronunciation between people due to, e.g., age, gender, anatomy, speech impediments, personality, social class, and, even within a single person at certain stage of life, differences due to, e.g., social style (auctioneers, sports announcers, advertisers cramming in qualifications on the radio), auditory circumstances (singing, whispering in a small room, bellowing to a crowd), emotional intensity and relative inebriation. (p. 562)

But this variation, near enough without limit, is no problem for realism. It provides no reason to doubt that, as a result of conventions, all the sounds produced by these various people in various circumstances really are SLEs: they really have the phonological, syntactic and semantic properties we naturally suppose them to have. The variation does indeed show that there can be difficulties in answering the rather uninteresting question of when people speak "the same language". But it should not lead to Rey's worry whether linguistics should "be grounded by BBC newscasters" or whoever (p. 563). As I note,

> the point is not that linguistics should be focusing on expressions in, say, Italian rather than Romance, or in, say, English rather than *x*-English for various values of '*x*'. (And the point is certainly not about "who gets to own" a term like 'English'.) The point is that the primary focus should be on linguistic expressions that share meanings in the idiolects of a group of people. (p. 184)

That is my sixth major conclusion (p. 183).

In sum, these considerations that Rey raises against linguistic realism seem to have *no force at all*. In general, those who present considerations against the existence of SLEs need to show how those considerations, if valid, would not count equally against the existence of many other essentially relational entities, particularly social entities, that clearly do exist. Rey's considerations do not pass this test.

Still, after noting similar responses in *Ignorance*, Rey adds a new wrinkle to the debate: "It would have helped, however, if [Devitt] had supplied some suggestions about exactly how a relational story of SLEs might go" (p. 56). In a similar vein, he later makes a passing challenge to my analogy of language with the bee's dance (discussed in section 2 above). He notes the correlations between the dances and the food source but doubts that there are any "similarly good correlations between human language and reality". He claims that the burden is on me to show that there are (pp. 569).

These quick comments raise very large issues which I can address only briefly. First, although I would not talk of "correlations" I do indeed make the common assumption that there 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. And I, along with thousands of realists about linguistic reality, have attempted to give externalist theories of meaning to explain these links. In so doing, people have appealed to referential relations, syntactic properties (as described by the grammar!), truth conditions, assertion conditions, acceptance properties, and so forth.[28] One would have to be delusional, of course, to think that the task is near finished: finishing will require a complete syntax and semantics! Still, it does seem to me that a vast amount of progress has been made over the last century. In any case, linguistic realists have certainly accepted their burden.

Second, there is a far greater burden on skeptics like Rey to provide powerful reasons for doubting that there are these links to the world. For, the assumption that there are the links is widespread, well-supported, and seemingly very important. Rey claims that to sustain my linguistic realism I would have to show that SLEs "covary regularly enough with external phenomena" He goes on: "The only effort ever to show any such thing was Skinner's, an effort I trust Devitt does not think we need rehearse" (p. 569). And I trust that Rey does not think that the failure of behaviorism gives any significant support to skepticism about externalist theories of meaning. It is going to take an awful lot more than that to sustain skepticism.

We can get a feel for the dimensions of the skeptical task by considering the relation of language to thought, discussed in part IV of *Igno-*

[28] See Devitt [1981] and Devitt and Sterelny [1999] for my main attempts.

rance. We have very good reasons for believing in thoughts: in particular, we ascribe thoughts to people to explain and predict their behavior and to use their thoughts as guides to a reality largely external to people (pp. 125–7, drawing on Devitt [1996]). The irresistible folk idea that "language expresses thought" then leads to my "fourth major conclusion" that the psychological reality of language should be investigated from a perspective on thought (pp. 127–32). Further considerations, drawing on Grice, yield my "fifth major conclusion" that thought has a certain priority to language ontologically, explanatorily, temporally, and in theoretical interest (pp.132–41). So our interest in language should be accompanied by an interest in the nature of thoughts (ch. 9).[29] I go along with the common externalist view that *thoughts* are relational, involving links with the world. If so, then we have the basis for showing that *language* is also relational (pp. 155–6). Perhaps more importantly, if the doubters were right and language was not relational, then it would be hard to see how thoughts could be. And if thoughts were not mostly relational, it would be hard to see how they could serve the purposes—explaining behavior and guiding us to reality—for which we ascribe them. Rey's quick comments leave us wondering about the nature of thoughts and their relation to his intentional inexistents, and about why, on his view, thoughts would be of any theoretical interest.

All in all, the burden of Rey's linguistic antirealism is very great. He promises a longer work setting out the difficulties of actual and possible relational accounts of SLEs (p. 562), difficulties he briefly foreshadows (pp. 562–4). Perhaps this work will show that such accounts are hopeless, but I predict that it won't. Meanwhile, we have not yet been presented with any good reasons for abandoning linguistic realism. But, I think what is really driving Rey's position is not his argument against the existence of SLEs but his argument that even if SLEs did exist they would not be of any theoretical interest. This raises question (II), which we will consider in a moment.

Smith: Smith spends many pages trying to convince us that linguistic reality is somehow mysterious. I shall be brief in response because the discussion of Rey, and the earlier discussion of Smith in section 2, should remove any mystery Smith has managed to create.

Smith has an epistemic worry about linguistic entities:

> what sort of independent access can we have to [linguistic outputs]? Can we establish facts about a language independently of facts about its speakers? Devitt seems to imagine we can, making the easy assumption that what we are talking about is just there anyway, knowable without knowing anything about the speakers who produce it. (p. 434)

I don't make any such assumption. The linguistic properties of sounds, inscriptions, etc. that interest us are ones that enable them to play extraordinarily important roles in the lives of speakers. So, *of course*, we look to those roles to get evidence of what those properties are (pp. 98–100). Indeed, we look for evidence wherever we can (Duhem-Quine).

Smith has trouble "finding" linguistic facts. (i) "Where exactly do we find facts about the complex arrangements of expressions standing in relations of, say, c-command to one another? Not in the sounds, that's for sure" (p. 435). Some sounds do indeed stand in the relation of c-command to others. The sounds have those relations in virtue of complicated psychological, social, and environment facts (section 1). So that is where we would "find" the linguistic facts. But we don't need to find these underlying facts in order to discover that objects have linguistic properties any more than we need to find the facts underlying being, say, a Chomskian, in order to discover that someone is one. (ii) "Where in the physical world could we locate a physical sentence token…?" (p. 439). Well, we would locate it wherever it is in space-time just like any other social entity. And it has the properties that make it a *sentence* in virtue of certain relations it has to other parts of space-time. No mystery here.

(II) Why is it important to suppose that SLEs do exist? Now, as already indicated, the properties in virtue of which something is an SLE are mostly ones it has by convention: "almost all noninnate syntax and almost all the word meanings of anyone's idiolect are conventional" (p. 181). For brevity, I shall ignore any innate elements to meaning; also the properties of an SLE, such as indexical reference, that are fixed "pragmatically". So our question becomes: Why is it important that there should be conventional SLEs?

Collins: One reason, pointed out in discussing Collins' Ling in section 2, is to explain language acquisition. Another is to explain communication. Ling has a negative attitude to this:

> Many of you philosophers are obsessed with communication…Anyhow, it strikes me that communication between A and B doesn't involve a third thing P to which both stand in some relation. That is, there is no need for public meanings or conventions as third things to which people relate. (p. 488)

I think that this is all wrong. I think this *not* because, as Rey suggests (p. 561), I think that communication between A and B is *impossible* without a third thing P that has a shared conventional meaning. However, without that, communication is hard, "pretty much limited to conveying the crudest messages about food, drink, sex, and shelter" (p. 186). My point is rather that the *efficacy* of using a language to communicate a vast range of often subtle and sophisticated messages requires that there be those shared conventions:

> On hearing an utterance, a person who participates in the conventions it involves can, with the help of pragmatic abilities that determine indexical references and remove ambiguities, immediately grasp the thought or message that the utterance expresses. It is in virtue of those largely conventional properties that the utterance is such a quick and effective guide to the speaker's thought. (p. 182)

Rey: Indeed, how is communication going to work without shared conventions? According to Rey's *folie à deux* view it works as follows:

[29] This interest in thought, and LOTH, are partly responsible for my "second tentative proposal": "there is little or nothing to the language faculty" (p. 173).

> The speaker has the illusion of uttering an SLE that the hearer has the illusion of hearing, with, however, the happy result that the hearer is usually able to determine precisely what the speaker intended to utter. (p. 552)

How is this "happy result" achieved? How does the message understood come to match the message intended? I responded as follows:

> Clearly the superficial properties of the physical entity—the entity that is the SLE on my view—must provide the hearer with clues to the speaker's intentional object. How could the superficial properties do that? Not by being excellent clues to the conventional meaning of the physical entity, as I think they are, because that entity has no conventional meaning on Rey's view. We are still left with a miracle: the success of hearers at guessing speakers' intentional objects without having the benefit of conventions that relate physical entities to meanings. And we are left with the closely related miracle of all parties to a successful communication being under the *same* illusion about the linguistic properties of a physical entity that has none. (p. 188)

Rey has a reply to this:

> nothing in my view precludes SLEs having conventional meanings, since nothing in my view precludes intentional inexistents from having them. After all, *conventions can attach as much to intentional inexistents as to real objects*: Santa Claus is a conventional symbol of Xmas; Aphrodite a symbol of love; Hermes a symbol of efficient messaging. (p. 560)

This is ingenious but it simply won't do. Can Santa Claus *really* be a conventional symbol of Xmas given that he doesn't exist? Isn't it rather the case that the name 'Santa Claus' is the symbol? Set that worry aside and accept, in the spirit of Xmas, that an intentional inexistent can enter into a convention. How could such a convention be set up? Only, I suggest, via the name of the inexistent. Thus the convention for Santa Claus would have been established in the community by the regular association of the name 'Santa Claus', meaning Santa Claus, with Xmas. We can tell no such story for Rey's inexistent SLEs. The convention for an SLE links a sound with a meaning. There is nothing miraculous about establishing this where the SLE is an actual sound that is regularly associated with that meaning as a result of being used to express thoughts with that meaning. But it would be miraculous if it were established where the only thing that is regularly associated with that meaning is an *inexistent* sound. We have no name of this intentional inexistent via which we could establish the convention and there is no other way to establish it.

So, I don't think Rey is entitled to conventional meanings for his SLEs. Still he is right to hanker after them. For, 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. We need to explain, as Rey, puts it, a "stability" in interpreting messages, and conventions are what largely do the job.

For Rey, this stability is a stability of illusion. He makes a striking comparison of this with "the remarkable stability of…perceptual illusions both across people, and across a single person over time" (p. 560). He cites the example of Kanizsa triangles in particular. But perceptual illusions are bad analogies because their stability is to be explained largely by innate structures whereas the stability in interpreting the message in a certain sound could not be thus explained. Indeed, I suggest that it can only be explained as largely the result of learning conventions.

Rey thinks that we should not bother with whether SLEs exist because their existence is irrelevant to linguistic theory (p. 564). I responded to this thought as follows:

> The study of the (largely) conventional meanings of actual linguistic entities, meanings constituted by a (partly) conventional syntax and conventional word meanings, is the concern of linguistic theory. Our theoretical interest in language is in explaining the nature of these conventional meanings that enable language to play such an important role in our lives…It is in virtue of those meanings that a language is acquired. Linguistic properties do play causal roles. If there really weren't any linguistic entities, communication would be miraculous and language learning a mystery. (p. 189)

This still seems right to me.[30]

5. *Conclusion*

In this paper I have defended some central themes of *Ignorance of Language* from its Dubrovnik critics. First, I have rejected the psychological conception of linguistics in favor of the linguistic conception: the grammars proposed by linguists are not primarily about a mental state of competence—of "knowing a language"—but rather are about a linguistic reality of sounds, inscriptions, and the like that are the outputs of that competence. Second, in opposition to the received view that a competent speaker must have propositional knowledge of her language, I have argued that she could be totally ignorant of it: she need not know anything about its rules or principles nor even anything about its syntactic facts. Third, I have argued for linguistic realism: there really are sounds, inscriptions, and other parts of the physical world having the syntactic and semantic properties attributed to them by grammars.

[30] The first draft of this paper was delivered at a conference on the philosophy of linguistics in Dubrovnik in September 2006. I am very indebted to the participants for the spirited but helpful comments the draft received there and to John Collins, Georges Rey, and Guy Longworth for helpful written comments. And we are all indebted to Nenad Miščević, Barry Smith, Peter Ludlow and, especially, Dunja Jutronić, for organizing this delightful conference.

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