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METHODOLOGY IN THE PHILOSOPHY OF LINGUISTICS

Michael Devitt

1. Introduction

Both Louise Antony ('Meta-Linguistics: Methodology and Ontology in Devitt's *Ignorance of Language*') and Paul Pietroski ('Think of the Children')[1] are committed to a Chomskian perspective on linguistics. My book, *Ignorance of Language* [2006a], argues for fourteen theses, seven 'major conclusions' and seven 'tentative proposals', many of which are, or at least seem to be, decidedly unChomskian. Antony's paper is largely a trenchant criticism of my methodology. Pietroski's paper is also critical of that methodology. I aim to show, in part II, how misguided these criticisms are. But I also want to return the favour. So, throughout this paper, I aim to expose the methodological flaws of my critics.

There is a pattern to Chomskian criticisms of *Ignorance*. The pattern is one of misrepresentation,[2] *ex cathedra* pronouncements, relentlessly uncharitable readings, and a wearingly impolite tone. Papers by John Collins [2007, 2008a,b], Peter Slezak [2007], and Barry Smith [2006] are paradigms; for responses, see Devitt [2006c, 2007, 2008a,b].[3] Antony's paper is another paradigm. Pietroski's is unusual in its civility.

The main focus of these critics, including Antony and Pietroski, has been on the book's first major conclusion: 'Linguistics is not part of psychology' [40]. I later called this 'the linguistic conception' of linguistics/grammars. It stands in contrast to the Chomskian 'psychological conception' [2006c: 571]. I shall defend the linguistic conception in part I.

[1]Both papers are in this issue. Unidentified references to Antony and Pietroski are to these works. All unindentified references to my work are to my book.
[2]Non-Chomskians don't have this problem; thus, Elizabeth Camp [2007] gets the book pretty right and Mark Crimmins [2007] gets it dead right.
[3]Most of these papers arose from very productive conferences in the philosophy of linguistics held annually in Dubrovnik since 2005. As a result most have been published in the *Croatian Journal of Philosophy*, which has become a centre for the philosophy of linguistics.

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I. DEFENDING THE LINGUISTIC CONCEPTION

2. Sketch of the Argument in Ignorance of Language.

The argument for the linguistic and against the psychological conception of grammars is to be found in chapter 2 of the book. In an earlier defence of the book I summarized the argument [2006c: sec. 2]. I shall here present the argument more briefly but also more formally.

The argument rests on the application of the following three general distinctions to humans and their language. The distinctions are [17–23]:

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

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

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

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

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

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

I take the discussion sketched so far to establish:

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

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

The discussion then turns to linguistics:

> (B) These distinctions apply to humans and their languages.
>
> [23–30]

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

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

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

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

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

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

[5] I apply 'rule' to syntax not with its technical sense in linguistics but with a broader sense covering what linguists call 'principles' [3, n. 1].

do with linguistic facts. Some psychological facts *cause* linguistic facts [23–4], some '*respect*' them [25], some *partly constitute* them [39–40, 132–3, 155–7], some *provide evidence* for them [32–4], and some make them *theoretically interesting* [28–34, 134–5]. But psychological facts are not the *subject matter* of grammars. The dispute is not over whether linguistics relates to psychology but over the way it does.

3. Antony's Response

Antony has a very dim view of this argument. After accurately summarizing my argument for 'the claim that language is ontologically distinct from linguistic competence', she takes me simply to infer the linguistic conception:

> Thus, from the apparently innocent observation that the stuff we say is ontologically distinct from the faculties that enable us to say it, Devitt sees a straight logical line to a substantive conclusion about the delineation of theoretical domains in the natural world. This is breathtaking.
>
> [653]

And *this* is egregious! The 'straight logical line' is Antony's invention. She rightly thinks that establishing the linguistic conception requires more than establishing the ontological distinctness of linguistic reality from the mind: we need to show that this reality 'is a proper object of scientific investigation' [651]. And this is precisely what I attempt in an argument I introduce as follows: 'Here is a more disturbing doubt. I have talked of studying the nature of a sentence token . . . . *What is our theoretical interest in the token*?' [28; emphasis original]. This argument [28–34] lies between the pages she draws on to summarize my argument for the distinctness [17–26] and those she draws on to summarize my conclusion [35–8]. If she hadn't skipped the argument she could have saved her taken breath for a genuine issue.

The skipped argument is the first part of my discussion of this important issue but it contains a reference [30] to the second part [134–5]. Antony must have missed that part too.

So *why is* the task of studying the nature of linguistic expressions theoretically interesting and worthwhile? I summarize the first part of my answer as follows:

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

I then go on to develop this last reason in presenting the second part of the argument, concluding:

> Language is an extraordinarily effective way of making the thoughts of others accessible to us, thoughts that otherwise would be largely inaccessible; and of making our thoughts accessible to others, often in the hope of changing their thoughts and hence their behavior. So we have a great theoretical interest in explaining the properties of linguistic expressions, including their syntactic properties, that enable the expressions to play this striking role. And just as our interest in the properties of the bees' dance leads to an interest in the bees' competence to produce dances so also does our interest in linguistic expressions lead an interest in our competence to produce them. We have the following "direction of theoretical interest": from thoughts to language to linguistic competence.
>
> [134–5] [6]

Antony follows her groundless criticism with a discussion of my view of the linguistic reality that I think grammars describe [653–655]. She gets that wrong too, but I have no space to argue the matter.[7]

We shall see much more of Antony's methodology at work in section 6.

4. *Pietroski's Response*

If the psychological conception of grammars is to be saved, there must be something wrong either with the three distinctions or with 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. Pietroski's response to this argument in section 3 does not show any of this. Indeed, Pietroski's defence of the psychological conception is strangely unrelated to my actual argument. The considerations he adduces are largely, sometimes even entirely, irrelevant.

Pietroski rightly points out that on my linguistic conception we can theorize about linguistic expressions without positing a language faculty and taking it to be 'the object of theoretical inquiry' [666]. But he then mischaracterizes my argument as follows:

> Chomsky's mentalistic project turns out to be an optional *addition* to the project of theorizing about linguistic expressions, not a proposed *strategy* for theorizing about (*inter alia*) linguistic expressions, and certainly not the best strategy. Once we see that the mentalistic project is optional, Devitt says, we'll see that it isn't warranted by current evidence.
>
> [666]

To think clearly about this it is important to keep three things distinct, each of which might be described as 'mentalistic or psychological': a *conception* of

[6]This issue of theoretical interest is further explored in [2008a: sec. 4.1].
[7]For my views on this reality see [178–89; also 2006c: 597–604; 2008a: secs. 4.2, 5; 2008b].

linguistics/grammars, a *project* of investigating linguistic competence, and a *strategy* for investigating linguistic expressions. Pietroski seems to be suggesting that I am against all three, but I am actually only opposed to the psychological *conception*. I do see the psychological *project* as additional to the linguistic one, but this is not to say that it is 'optional' nor to disregard it in any other way. Indeed, it is the main concern of the book. However, I do think that positing a substantial language faculty to fulfil this project is not warranted by current evidence [173, 265]. What about the psychological *strategy*? The strategy that Pietroski has in mind pays a lot of attention to the evidence from language acquisition. I am all for this [32–3], whilst emphasizing the range of other available evidence about the nature of linguistic expressions [98–100].

(I) Pietroski claims that the argument he has mischaracterized 'doesn't work' (666). Why not? The first of his three reasons is:

> Given *both* a theory of a competence *and* a theory of its 'outputs/products or inputs', we can and should '[d]istinguish the theory of a competence from the theory of its outputs/products or inputs' [2006: 17]. But this maxim cannot ensure a theory of either. *Prima facie*, there is no empirical theory of horseshoes, chess moves, or wffs to be had. Bee dances are interesting. But what is the analogue of a constraint on extraction from relative clauses? Devitt gives us no reason for thinking that acquiring and using a bee language is, in relevant respects, like acquiring and using English. (Though perhaps the best theories of bee dances will end up being theories of a mental module in bees.) [666]

I take it that these cryptic remarks are meant first to undermine the distinctions in general and then to undermine their application to language. They are not persuasive.

(i) Are there in fact theories of the products? I think that he is wrong about horseshoes, chess moves and wffs, but set them aside and focus on the bee's dance. Here we obviously have an empirical theory, that of von Frisch. Furthermore, the present consensus is that this theory is *true*.[8] Yet, as already noted, we do not have a theory of the bee's competence to dance. This provides very good evidence both for the distinction and that it is a worthwhile task to explain the product of a competence even if one cannot explain the competence.

(ii) Pietroski does not say that this task is not worthwhile but his parenthetical remark about a 'mental module' may suggest that it isn't: he wonders whether, *in the end*, the theory of the dance will collapse into the theory of the bee's competence. So, the idea may be that, in the end, there is no distinction, just a theory of competence. But why should we think that this is likely or even possible?

(iii) A helpful exchange with Pietroski provided some insight into a possible answer. Chomskians are rightly impressed with behaviourism's failure to explain even the walk that a rat takes to press a bar. So why

[8]Smith [2006: 441] is not part of the consensus. I have responded [2006c: 585–6].

should we think that it could explain the bee's dance? Surely any satisfactory explanation is likely to be fairly mentalistic. I agree, but this is beside the point because it misconceives the problem that von Frisch solved. This problem was not explaining the *cause of the bee's behaviour*, something that would surely call on (quasi-)mental states, but explaining the *nature of the representational system* that is a product of the behaviour. Of course, given the Respect Constraint, that system must be a key part of any explanation of the behaviour that we may someday come up with. Still, what von Frisch did was explain the representational system, not the cause of the dance.

(iv) So much for the criticisms of the distinctions in general. Finally, Pietroski questions the analogy between the bee's dance and language. Does the bee's dance have anything like the constraint on extraction from relative clauses? Is it acquired and used the way English is? These questions are beside the point. Any x that is analogous to y in some way will be disanalogous in countless others. To be a good analogy, x simply has to be analogous to y in the *relevant* way. The relevant way in this case is in illustrating distinctions 1 to 3. Nothing Pietroski says casts any doubt on that.

(II) The second reason that Pietroski gives for thinking the attributed argument does not work is foreshadowed in his earlier discussion of the 'negative facts' that he takes to be Chomsky's starting point [sec. 2]. The main theme of this discussion is that this starting point led to a successful focus on innate constraints and this provides important support for the psychological conception of grammars. Indeed, this provides 'the best arguments for the conception', arguments that I am alleged to have overlooked [657]. He now goes on:

> ... I don't see how such examples suggest that linguistic theories are really theories of expressions—as opposed to the faculty that generates expressions—given that the theories in question are offered in part to explain why humans *can't* generate expressions that would associate signals with meanings in various logically possible ways .... [u]ntil someone presents [a nonmentalistic alternative theory] ... why should we believe there *is* a good theory of linguistic expressions that *isn't* a Chomsky-style proposal that is simply coy about its commitment to a language faculty? ... One needs to say what *else* [other than the posited language faculty] might be responsible for the relevant *constraints on how* humans associate signals with meanings.
>
> [666]

(i) The fascinating issue of the innate constraints is beside the point of the distinctions I am emphasizing. Whatever the extent and nature of those constraints, the task of studying linguistic competence is one thing, the task of studying its linguistic output is another. It seems like a good bet that the bee's competence to produce representations in the particular system von Frisch describes is *entirely* innate, yet studying the competence and studying the system are still distinct tasks (though related by the Respect Constraint), as the current state of these studies vividly demonstrates. And the tasks would still be distinct if the dance were entirely learned, or partly innate and partly learned (like our languages).

(ii) Pietroski is simply conflating theories which my argument insists should be distinguished. Any theory of the nature of the innate constraints is of course a psychological one. I am all for such theories and discuss them at some length [ch. 12]. Any theory of the nature of linguistic competence is also a psychological one. Most of my book is about such theories. But none of these psychological theories are grammars, theories of a language. That's the point of the linguistic conception.

(iii) Any good theory of the innate constraints provides evidence for a grammar of a natural language:

> [E]vidence 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.
>
> [32–3]

But this does not make the theory of the language psychological. It is one thing for a theory to have psychological *evidence*, it is another for it to *be* psychological. Any theory can be supported by all sorts of evidence [Duhem-Quine], as I know Pietroski would agree.

(iv) I argue that the generative grammars produced by linguists *are* good theories of linguistic reality [30–5]. So we don't need a 'nonmentalistic alternative': we already have one. In arguing this I am not of course denying that there might be *other* theories of the psychological reality underlying language, nor that grammars are relevant to such theories: because of the Respect Constraint, grammars alone yield the minimal position (M) on psychological reality. Chomskians think that theories of psychological reality should posit a language faculty. I am indeed 'coy' about this commitment[9] and will briefly indicate why later [sec. 5: 'Innate Constraints'].

(III) Pietroski's third reason is that no theory of expressions is to be had. (i) He finds problems in my understanding of Chomsky's technical term 'I-language' and thinks that I take the term to refer to 'individual E-languages'. (ii) He wonders how we could characterize the expressions of a language 'independently of the language faculty'. (iii) He claims that I provide 'no help' with the problem of telling what the sentences of English are [667–8].

(i) Chomsky has, of course, every right to his technical term 'I-language' and I made some attempt to go along with it. I do not take the term to refer to individual E-languages, as Pietroski supposes, but still he is surely right that my usage diverges from Chomsky's. So let us rest nothing on this technical term. We can identify our disagreements without it.[10]

[9]Smith is more brutal, describing my lack of commitment as 'lily-livered' [2006: 451]. I have responded [2006c: 579 n. 7].

[10]So Pietroski's claim that 'Devitt's case for his own view relies on some unwarranted assumptions about . . . I-languages' [657] is false. So too is his similar view about linguistic competence, but I have no space to argue the matter.

(ii) I see a language as systematically related expressions with meanings constituted by word meanings and syntactic structures. These expressions are part of the 'external' physical world just as the bee's dance is. By 'I-language' I meant a language, but abstracting from any properties constituted by word–world connections. Chomsky seems dubious of such properties and so it seemed best to leave them mostly out of the picture. 'In effect, an I-language has only syntactic properties, in a broad sense of the term' [15]. Now, I take a *generative grammar* to characterize the expressions of an I-language in this sense, just as von Frisch characterized the 'expressions' of the bee's dance. So we don't need any *other* characterization. And the characterization *is* independent of the language faculty in that it says nothing about that faculty beyond the minimal position (M). This is not to say, of course, that linguistic facts are independent of psychological ones: 'psychological facts together with social and environmental facts determine linguistic facts'. But this determination does not make linguistic facts psychological [39–40; 2006c: 582–4].

(iii) I have no idea why Pietroski thinks that my book provides 'no help' on the evidential problem. I have a section discussing the evidence for a grammar—evidence from what we say and understand, from language acquisition, from intuitions, and so on [98–100]. What more does he want to know about how we tell what the sentences of English are?

In sum, Pietroski starts by mischaracterizing my argument for the linguistic conception. His reason (II) against this conception misses its target entirely. His reasons (I) and (III) largely miss it and inflict no damage.

II. METHODOLOGICAL MATTERS

5. *Pietroski*

I turn now to consider briefly the matters raised in sections 1 and 2 of Pietroski's paper.

Methodology: Pietroski is puzzled that my discussion of psychological reality proceeds without attention to recent developments in linguistics, particularly to Minimalism [659]. I proceed in this way because the developments are not relevant to the fairly high-level conclusions and proposals that I am actually arguing for. We could roughly capture the question that I am addressing as follows: Given the sorts of generative grammars proposed by Chomskians and the evidence presented for them, what should we now conclude about psychological reality? Hence, differences between those grammars are not relevant. Of course, given that the psychological reality must 'respect' the linguistic rules, the latest view of those rules will be very relevant to a detailed theory of that reality. But we are very distant from such a theory and I am certainly not proposing one.

Innate Constraints: Pietroski thinks that Minimalism is particularly relevant to my conclusions and proposals because it presses attention on *how* and *why* language cannot have certain ambiguities, for example, thus focusing attention on innate constraints [sec. 2]. In (II) above, I have already

rejected the idea that this focus is relevant to my rejection of the psychological conception of grammars. I add now that it does not undermine my doubts about two Chomskian views of the psychological reality underlying language: that the structure rules posited by Universal Grammar ('UG-rules') are innately embodied; and that there is a substantial language faculty.

I agree that the focus on innate constraints is productive, and that it is very important to discover precisely what constraints there are on the languages that humans can acquire naturally. Furthermore, I am persuaded by the evidence—though probably not as persuaded as Pietroski—that humans *are* constrained to acquire only languages that are governed by (more or less) the UG-rules [248–52]. So, Pietroski has me wrong in thinking that, in my 'defence of Cowie', I doubt this aspect of Chomskian nativism and so need an empiricist alternative [663–4]. What I do doubt is the much stronger view that the UG-rules are actually innate in the mind. It remains an open question whether humans are constrained to acquire languages governed by the UG-rules because those very rules are innate or because of some other sort of innate mechanism. The present evidence justifies the view that there is *something* innate that is responsible for our acquiring only 'UG-languages' but does not justify any particular view of *what* is responsible: we have no theory of language acquisition detailed enough and successful enough to justify any conclusion about that [255–6]. Nonetheless, my sixth *tentative* proposal is that the UG-rules are largely, if not entirely, the innate structure rules of 'the language of thought', of 'Mentalese', and that this is responsible for the constraint [256–9]. This puts the innate constraints in the central processor and thus provides one of my reasons for being 'coy' about the language faculty [260–6]; the other reason comes from considering the effects of brain impairment on linguistic abilities [165–71]. My second tentative proposal is that there is little or nothing to the language faculty.

Finally, I argue [259–60] that if thought is not language-like, then innate UG-rules are not responsible for the innate constraints on language (see seventh tentative proposal). So it seems to me likely that the Chomskian view that the UG-rules are psychologically real depends on the controversial language-of-thought hypothesis.

Intuitions: I agree with much of what Pietroski has to say about intuitions. The crucial *dis*agreement is over their *source*. The Chomskian view is that the language faculty is the source: the intuitions are, as I like to put it, 'the voice of competence'. I reject this view, arguing that although linguistic competence supplies linguistic data, it does not supply *information about* the data. The information is the result of theory-laden central processor reflection on the data. Pietroski responds: 'But this revisionary conclusion is unsupported by any independent evidence' [662]. This brief pronouncement is *the full extent* of his discussion of this crucial disagreement! And the comment is baffling because chapter 7 (and [Devitt 2006b]) are largely devoted to providing evidence. Positively, I argue for my view by likening linguistic intuitions to intuitions in general [103–11].[11] Negatively, I argue

[11]Smith [2006: 443–6] and Miscevic 2006 criticize my view on intuitions. My response to Miscevic includes some development of the view [2006c: 594–5].

against the voice-of-competence view. The explicit version of that view claims that the language faculty contains a representation of the linguistic rules—the Representational Thesis (RT)—and that intuitions are derived from this by a causal and rational process like a deduction [96]. I argue that my explanation is better for a variety of reasons, not least that it is not committed to the implausible and unsupported RT [100–3, 112–7]. Pietroski has no more time for RT than I have [658]. So what is *his* version of the voice-of-competence view? *He does not say*. What he needs to explain is *how* the intuitions are caused by embodied but unrepresented rules. And the first problem is that, so far as I know, no such explanation has ever been attempted [96–8]. The second, and more serious, problem is that there are reasons for thinking that no such explanation will be forthcoming [117–9].

6. *Antony*

Part One of Antony's paper is a naturalistic sermon about my methodology. She accuses me repeatedly of arguing an '*a priori* case against cognitivism' [645]. I am alleged to rely 'very heavily on judgments about "plausibility"' [644] without regard to the actual evidence, 'merely eyeballing the situation' [645]. The accusations are baseless and the moralizing unseemly.

The accusations raise three questions. (1) *What* cognitivist theses am I against? (2) Do I rely heavily on *a priori* plausibility judgments in my argument against them? (3) Do I ignore the evidence?

(1) Antony's talk of my opposition to 'cognitivism' is vague and highly misleading. At times she writes as if I am opposed to cognitivism *in general*; see the discussion of Gallistel [644–5], for example. Such an allegation would be silly, and I assume that she does not really mean to make it. However, she clearly does take me to be opposed to 'cognitivist theories of language' [644], implying that I am opposed to all such theories. This is the first of a series of gross misrepresentations. I oppose *certain* cognitivist theories of language, but I *propose* others.[12] Here it is important to distinguish, as Antony does not, between 'high' cognitivism, which takes the rules of a language to be represented in the minds of its users—that's RT—and 'low' cognitivism which takes them to be simply embodied without being represented. I totally reject high cognitivism: that's my second major conclusion [84]. My attitude towards low cognitivism is more complex. Thus, my first tentative proposal—that 'a language is largely psychologically real in a speaker in that its rules are similar to the structure rules of her thought'—is cognitivist. However, my fourth tentative proposal—'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'—is noncognitivist [276]. Antony misses all these subtleties.

[12]Five of the book's fourteen theses are clearly committed to cognitivism. Seven oppose a cognitive theory of some sort.

(2) Antony cites my plausibility claims about the diving kingfisher and dancing bee as exemplifying the *a priori* nature of my case against 'cognitivism' [644]. The charge is spurious. Whatever the appropriateness of these claims—and others nearby about calculators, computers and other animals [47–51][13]—*they play near enough no role* in any of my arguments against *any* cognitive theory of language. Antony makes no attempt to show otherwise. The explicit purpose of that discussion is quite different: it is to emphasize the importance of the relatively uncontroversial distinction between represented and simply embodied rules. Indeed, serious argument against cognitivist theories does not begin for another two chapters.

The *only* truth underlying Antony's claim about the role I give to intuitions is in the argument for my third tentative proposal that 'the structure rules of a language are the wrong sort of rule to govern the process of language use' [207]. This intuition appears early in the places Antony notes [645]. I develop it in several later discussions and do give it some weight. I don't apologize for this. As Antony says, 'the initial plausibility of an empirical hypothesis has to count for something, or we'd never get science off the ground' [646]. That is what these discussions are attempting. But, as Antony insists—and so do I in passages ignored by Antony [ch. 7]—we cannot rest with intuitions. And I don't. That brings us to (3).

(3) Antony's insistent theme is that my alleged opposition to 'cognitivism' is entirely *a priori*, ignoring the evidence. Yet—nice irony!—she gives no evidence for this: *she does not cite a single piece of neglected evidence that bears on any of my fourteen theses*. Worse still, she totally ignores the considerable amount of evidence that I *do* cite in favour of my theses. Consider, for example, the evidence cited in my lengthy argument against RT [87–121, 195–272]; and in the case for my tentative proposal in favor of brute-causal language processing [220–43]; and in support of the just-mentioned intuition that the structure rules are not processing rules [207–8, 220–43]. It is particularly striking that Antony does not address *any* of the psychological evidence I cite on skills in general [210–20] nor on language use in particular [230–41], contenting herself with the *ex cathedra* pronouncement that my discussion of it 'shows no more than that the connection between data and cognitivist theory is non-deductive' [650].

My theses concern difficult matters, and there is surely evidence that I have overlooked. The constructive thing for Antony to have done would have been to produce some.

I introduce a principle, 'Pylyshyn's Razor': 'Representations are not to be multiplied beyond necessity' [51]. Antony alleges that I repeatedly invoke this in developing my '*a priori* case against cognitivism' [645]. Antony first misstates the Razor[14] and then misinterprets it. She sums up some sound advice from Gary Marcus as follows: 'Parsimony ... is a tie-breaker: it tells us to choose the most economical one of the theories *that are adequate to the*

[13]All these claims, bar one, are about the representation of rules and I think those are adequately supported on the spot or by later evidence [210–20]. The exception is a claim about the representation of angles, resulting from the final two words in the following passage quoted by Antony: 'It is not plausible to suppose that the kingfisher represents any ... facts about refraction and angles' [49]. This claim is inconsistent with others in the book (e.g. [21, n. 5]) and I should never have made it.

[14]She substitutes a corollary [52] of the Razor for the Razor itself. This is weird. The Razor is stated many times in the book and the substitution does not even serve her hostile purposes.

evidence [647]. And Pylyshyn's Razor tells us something similar: that we should choose an explanation that posits representations over one that doesn't '*only if the representations do explanatory work*' [51]. And that is indeed how I apply the Razor many times. Yet Antony claims, characteristically without citing a single piece of evidence from my *actual* application of the Razor, that the application 'flouts [Marcus'] advice' [647]. It doesn't.

Finally, Antony finds two 'odd features' in my discussion of abduction. First, in this discussion [198–201], I emphasize the virtues of suspending judgment in certain circumstances and keeping looking. Antony sees herself as disagreeing with me about the circumstances [647–8], and perhaps she does, but I shall set that aside. Second, she takes my discussion of abduction to give the 'methodological advice' that, when trying to 'legitimize abductions', we should *not* 'pile on the data that must be explained'. Rather, she claims, I recommend 'the kind of conservatism that ... would have eliminated from further consideration virtually every major theoretical innovation in science' [649]. This is preposterous. (She says she 'cannot find a charitable reading'!) Abduction is a method of *inference*. I am concerned with when, in general, it is good and with what good ones we now have about the psychological reality of language in particular. This is all about what we are *justified in believing*: And my conclusion is: not very much about psychological reality; we should mostly suspend judgment. This does, of course, have some bearing on methodology, but it is not 'methodological advice' about how to get 'theoretical innovation'. My concern is with 'the method of confirmation' not 'the method of discovery'. It is *obvious* that we will get interesting novel hypotheses about language use and acquisition by going beyond what is now justified. Indeed, chapters 8–12 are largely concerned with such hypotheses; my tentative proposals are tentative precisely *because* they are not now justified. My aim was to assess the *present* evidence for various hypotheses, and thus detect which ones seem likely to be involved in future abductions. Beyond that, my attention to abduction yields no advice at all for future research.

Antony's methodology is one of uncharitable misrepresentation. Unsurprisingly, her criticisms do not bear on my actual method.

7. *Conclusion*

Pietroski's criticisms are often misdirected and where they are not they are unsuccessful. Antony's paper is a shocking collection of flagrant misrepresentations. This is particularly remarkable in that I drew her attention to these very misrepresentations (even the misstatement of Pylyshyn's Razor!) when she sent me an earlier version of the paper. If it were not for her admirable work elsewhere I would be tempted to say of her what Mary McCarthy said of Lilian Hellman.[15]

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