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### Meta-Linguistics: Methodology and Ontology in Devitt's Ignorance of Language

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## CRITICAL DISCUSSION

Michael Devitt, *Ignorance of Language*, Oxford: Clarendon Press, 2006, pp. xiii + 304, £30 (cloth).

### META-LINGUISTICS: METHODOLOGY AND ONTOLOGY IN DEVITT'S *IGNORANCE OF LANGUAGE*

Louise Antony

In *Ignorance of Language*, Michael Devitt attempts to overthrow one of the most powerful ideas in all of cognitive science: Chomsky's conception of human language as the overt manifestation of an internalized grammar, acquired with the aid of an innate faculty that constrains the possible forms of any such grammar. Devitt contends, on the contrary, that language may very well be acquired and used without speakers ever coming to know the grammars of the language they speak. He purports to have shown that, for all that we know, we could all be entirely 'ignorant' of language.

Devitt also disputes Chomsky's claim that linguistics is really a branch of psychology. Devitt's own view is that linguistics has a *sui generis* domain: the language itself (which I'll call 'Language') as opposed to the competence that produces it. It is the job of linguists to map this domain, to produce theoretically adequate descriptions of the various human Languages. Such descriptions can then be passed off to the psychologists, whose proper business it is to discover how human beings manage to produce sounds and inscriptions structured in the way the linguistic descriptions say they are structured. In Devitt's parlance, linguists should discover the 'structure rules' of Language, leaving it to the psychologists to discover how human beings manage to generate verbal material that 'respects' those rules.

Devitt considers himself to be a philosophical naturalist, someone who is, *inter alia*, committed to the continuity of philosophical and empirical investigation. It is characteristic of naturalistically inclined philosophers to reject aprioristic pronouncements about what can and cannot be true in realms where fruitful empirical research is being conducted. Yet he contends that relatively *a priori* considerations suffice to show that cognitivist (knowledge-based) theories of language acquisition and production are sufficiently implausible to warrant discarding them in favour of yet-to-be-articulated non-cognitivist alternatives. In Part I of the paper, I'll show that his arguments for

this contention fail: the methodological assumptions he needs are dubious, and run counter to the counsel of a naturalized approach to language and the mind. In arguing his positive case, Devitt once again relies on aprioristic methodology, this time to establish that Language is not only ontologically separate from the psychological competence that produces it, but that it can be individuated and studied in complete abstraction from that competence. In Part II, I'll argue that once again Devitt fails to make his case.

### I. Abduction and 'Plausibility'

In his arguments against cognitivist theories of language, Devitt relies very heavily on judgments about 'plausibility'. Early on in the discussion, he shares with us his judgments about the *a priori* likelihood of internally represented rules being implicated in the explanations of various cognitive achievements. For example, discussing the ability of the kingfisher to launch a predatory dive at precisely the right angle, Devitt announces that 'it is not plausible to suppose that the kingfisher represents any ... facts about refraction and angles'. Again, with regard to the dance of the honeybee, 'While there may be some plausibility to the idea that the bee represents its food source, there is little to the idea that it represents whatever rules may govern its dancing' [49].<sup>1</sup> I don't know where these judgments come from; nor do I understand why they should be given any particular weight. I'd like to see the models that have been proposed to explain the kingfisher's diving precision or the honeybee's dance—if the best models impute represented rules to the birds and the bees, then it hardly matters whether such a thing seemed 'plausible' to begin with. And indeed, the going models of insect behaviour are considerably more cognitivist than Devitt appears to recognize.

C. R. Gallistel discusses the problem of insect navigation in his essay 'Symbolic Processes in the Insect Brain' [Gallistel 1998], looking at research on ants, honeybees and locusts. Data drawn from extensive experiments testing the effect of a variety of variables on these insects' abilities to find their ways out to food or back to home base overwhelmingly support the hypothesis that these creatures engage in cognitively sophisticated computations. Locusts appear to be able to utilize motion parallax to compute distance. Ants appear to be able to keep track of their distance from their hives, and to use this information to get themselves home when they are displaced from their original trajectories. Bees and other insects appear to do dead reckoning.

In his summary remarks, Gallistel acknowledges that these conclusions are surprising, and presumes that many will find them difficult to accept. He admits that

we do not know with any certainty how the nervous system implements even the operations assumed in neural net models, let alone the fuller set of operations taken for granted in any computational/symbolic model of the processes that mediate behaviors.

[Gallistel 1998: 46]

<sup>1</sup>Unattributed page numbers refer to *Ignorance of Language*.

Nonetheless, he finds the behavioural evidence in favour of these symbolic models to be compelling:

I have argued that it is difficult to imagine how any system that did not store and retrieve the values of variables and apply to them all of the operations in the system of arithmetic could do sun compass course holding, dead reckoning, and the determination of distance from parallax.

[*ibid.* 47]

The need to posit symbolic processing in insect brains in order to explain their observed behaviour therefore sets an agenda for neuroscience:

If we are persuaded by the behavioral evidence that the nervous system really does compute, then this establishes an agenda for behavioral neuroscience, namely to discover the processes that enable it to do so.

[*ibid.* 46]

In developing his *a priori* case against cognitivism, Devitt repeatedly invokes a methodological principle he calls ‘Pylyshyn’s Razor’:

[S]hould we have good reasons for supposing that a system is governed by a rule, we need further reasons for supposing that the rule governs by being represented and applied.

[52, his emphasis]

This would be all fine and good (leaving aside the question whether Pylyshyn would endorse the principle as stated), if Devitt applied it by *considering the reasons*—i.e., the evidence—that theorists give in support of the cognitivist models they develop. But whenever Devitt appeals to the principle, he does so by merely eyeballing the situation. Thus, he asserts, even if we have reason to think that a system ‘encodes a representation of structure rules’, it may ‘seem implausible’ to hold that these represented rules are also processing rules. Why implausible? Because that’s the way it *seems*.

The problem is that structure rules often *seem* to be *the wrong sort* of rules to be processing rules: they do not *seem* to be the sort of rules that would govern a process that the system actually goes through.

[53, first and third emphases mine]

Later on, this expression of incredulity is something that can be ‘noted’ and used to draw further conclusions: ‘earlier . . . , we noted the unlikelihood of a represented structure rule being a processing rule . . .’ [58]. Still later, in discussing Chomsky’s early theory, Devitt asks rhetorically, ‘Is it psychologically plausible that on the way to producing a passive sentence a speaker must produce an active-like D-structure which she then transforms into a passive S-structure by NP-movements?’ [65] No, the reader is expected to answer, it is not. Yet I found myself unable to render a verdict without seeing whatever evidence led Chomsky to posit such a procedure. On the next page,

there is a great deal more about what is ‘intuitive’, about where there is a ‘*prima facie* problem’ and, again, about what is ‘plausible’. I still want to know, as a naturalist, what the evidence says.

The initial plausibility of an empirical hypothesis has to count for something, or we’d never get science off the ground. But, when there is evidence available, consideration of such should obviate our initial assessments. It simply doesn’t matter how initially plausible a proposal was if it cannot deliver the goods. And if a proposal antecedently judged implausible delivers the goods, if the implausible details are really implicated in the observations, then so much the worse for our initial plausibility judgements.

Here’s another reason to treat plausibility intuitions warily: we don’t really know where they came from. A good deal of the suspicion with which symbolic models are greeted in cognitive science may well be held over from the days when operationalism reigned and behaviourism was all there was to psychology. William Brewer [1974] has pointed out that behaviourists clung to their anti-cognitive scruples even as they reported baffling ‘anomalies’ that could be readily accommodated by cognitivist models. Brian McLaughlin and Theodore Warfield have pointed out, in a similar vein, that almost all claims about the superior processing speed of connectionist models over classical symbolic models were made in the absence of any actual comparison of the two model types. McLaughlin and Warfield [1994] did conduct such a comparison, and found many classical symbol models to be just as fast as connectionist competitors. Devitt would do well to take note of this particular result, since he argues that the speed of language processing counts against rule-based models: ‘Representing rules which have to be applied or consulted is an unlikely way to achieve this efficiency’ [210].

Gary Marcus makes the overall point about the unreliability of prior assessments of plausibility in his discussion of the debate between connectionists and defenders of symbolic models as to whether the neurological structure of the brain can support symbol-processing. He first explains why many people are enthusiastic about multilayer perceptrons:

The argument for eliminating symbol-manipulation . . . rests not so much on empirical arguments against symbol-manipulation in particular domains, but instead primarily on what one might think of as preliminary theoretical considerations.

[Marcus 2001: 27]

However, Marcus concludes:

None of the preliminary considerations that apparently favor multilayer perceptrons—biological plausibility, universal function approximation, a broad range of functions—is actually decisive. Instead, as is often the case in science, preliminary considerations do not suffice to settle scientific questions.

[*ibid.* 28]

For example, he points out that while connectionist networks are ‘biologically plausible’ in respect of their organization of layers of

interacting nodes, they turn out to be biologically *implausible* to the extent that they rely on back propagation, which actual neural networks cannot perform. Marcus cautions strongly against ruling out *any* architecture, including symbols systems, on grounds like these, given how little relevant information we have about neural implementation. '[T]here is no guarantee that the right answer to the question how cognition is implemented in the neural substrate will be one that appears to our contemporary eyes to be 'biologically plausible' [ibid. 30].

Most pertinently, Marcus advises against applying highly general methodological maxims in situations where models are undeveloped and data are sparse. The principle of parsimony, he points out, 'chooses only between models that adequately cover the data. Since we currently lack such models, applying parsimony is for now premature' [ibid. 31]. Moreover, it is often unclear what parsimony entails in a particular controversy. In the debate about cognitive architecture, we can use it to derive incompatible judgements, depending on how we look at the competing hypotheses. While multilayer perceptrons posit little in the way of innate structure (good) they are profligate with free parameters (bad). Marcus concludes: 'Because biological systems are clearly complex, constraining ourselves a priori to just a few mechanisms may not be wise' [loc. cit.].

Devitt should take particularly this observation to heart. Parsimony, as Marcus points out, is a tie-breaker: it tells us to choose the most economical one of the theories *that are adequate to the evidence*. It does not tell us in advance of an assessment of empirical adequacy which *kinds* of mechanisms ought to be preferred to which. Parsimony, after all, is traditionally at odds with conservatism—one wholly novel mechanism can sometimes wipe out a dozen small patches that have accreted over time. Yet Devitt's commitment to 'Pylyshyn's Razor' flouts this advice: it bids us give preference to mechanisms that do not include represented rules over those that do, in advance of knowing anything about the complexities and ineconomies that such alternatives might entail. In sum, 'Pylyshyn's Razor' is hardly a neutral methodological principle, and so cannot provide nontendentious support for Devitt's claim that some version of a 'brute causal' theory is the one to beat.

Devitt's confidence that he has established the *a priori* implausibility of cognitivist theories explains, I think, two very odd features of his discussion of the logic of abductive inference. The first is his characterization of the reasoning that he takes to be in the minds of defenders of cognitivism:

The abduction runs along the following lines. We can explain language comprehension if we see it as a rational process of testing hypotheses about a person's speech input. *And there is no other way to explain it.*

[198, his emphasis]

While it is true that many defenders of abductive arguments for nativism and cognitivism speak hyperbolically of being 'the only theory in town', no one seriously thinks that good abduction requires the elimination of all *possible* competitor theories. How could it? One doesn't know, after all, what they'll think up next. But since Devitt believes that cognitivist theories

have a black mark going in, he thinks that merely explaining the evidence would be insufficient warrant for a theory of that type. Rather, he thinks, one isn't warranted in accepting a cognitivist hypothesis unless and until it is demonstrated that no *non-cognitivist* theory could do the job as well.

I have to confess some sympathy with the view that one can, under certain conditions, be justified in rejecting an allegedly explanatory hypothesis even if one has no alternative to suggest. But what are those conditions? One, I suggest, is if the hypothesis involves processes or mechanisms that we have reason to believe are impossible, or if the hypothesis itself is unintelligible. Creationist hypotheses can be ruled out on this ground (or so I'm prepared to argue).<sup>2</sup> But Devitt never claims that representationalist theories are unintelligible, nor that the mechanisms they posit are impossible to implement. That's good, since such implementations actually exist—I'm typing my paper on one right now. Is there another condition that warrants discounting 'the only theory we've got'? There is, I think, but once again, it's not going to support Devitt. The condition is this: one can reject a proffered explanation for a given phenomenon, if the hypothesis in question *fails to explain the phenomenon*. I think, once again, that creationist 'explanations' fulfil this condition. Supernaturalistic histories of the world do not eliminate mysteries; rather, they introduce new ones, with the mere *stipulation* that the new ones explain the old. But once again, this is not what Devitt seems to have in mind. He never makes the claim that cognitivist theories fail to account for the data they purport to explain, only that it has not been shown that a more 'plausible' theory, without representations, could not do equally well.

This brings us to the second oddity in Devitt's account of abduction. Devitt observes, quite correctly, that the reason some people are suspicious of inference-to-the-best explanation is that it appears at times to be too loosely constrained, giving licence to just-so stories and flights of fancy. Fine—but in that case, one might have thought, the thing to do to legitimize abductions would be to pile on the data that must be explained. For theories of language, this would mean pulling in facts from domains as numerous and as diverse as possible: from psychological and behavioural evidence about acquisition, production and comprehension, from general work in psychological development, from clinical data on head trauma and stroke, and from experimental findings on neural architecture. A theory that could explain all that would be worth taking seriously.

But this is precisely *not* the strategy Devitt recommends. We are to constrain the class of candidate hypotheses, he tells us, not by garnering more and more diverse facts to be explained, but rather by first testing each candidate for 'plausibility relative to' what he calls 'background knowledge'. If we find that none of the explanations that proffer themselves seems plausible, relative to 'what else we know', then, Devitt counsels, 'we should *suspend judgement and keep looking*' [199, his emphasis]. His plan, therefore, is to produce a *preliminary* assessment of the 'likelihood of a future abduction supporting each of the various positions' on the psychological reality of linguistic rules. By 'preliminary', he means 'prior to any

<sup>2</sup>Although even here, I overstate the case. Intuitions of impossibility can be quite mistaken.

consideration of the psycholinguistic evidence from language use itself', as if to look at the primary source of evidence for a model would in some way be cheating [200].

I cannot find a charitable reading of this methodological advice. The kind of conservatism that Devitt appears to be recommending would have eliminated from further consideration virtually every major theoretical innovation in science. At the time of its proposal, how plausible was relativity theory or—for that matter—heliocentrism? If novel hypotheses are disqualified at the outset because of their novelty, how is science to ever discover fundamentally new structures and processes? Devitt avers that his methodology should 'not be confused with the extreme sceptical view that we should not embrace an abduction until *all possible* alternative explanations have been set aside. We need wait only to set aside alternatives that are likely, given what we already know' [199].<sup>3</sup> But while his position does not devolve into scepticism, it remains unclear how his method would permit a genuinely novel hypothesis to garner even provisional support.

For the case at hand, the problem is evident as soon as we look at the details, at what Devitt considers us to 'already know' in the case of language and mind. As it turns out, the 'background knowledge' with which our proposed linguistic models must square turns out to centre on the 'fact' that language is a *skill*. It is supposed to be what we know about the acquisition and deployment of skills in general, and of cognitive skills in particular that makes cognitivist accounts of language implausible enough for us to suspend judgment, and to look instead for 'brute causal' alternatives.

There are two questions here: first, what is known 'already' about skills, and second, is it so clear that language is one? Let's look at the first question first. Here Devitt does a deft bit of bootstrapping:

... in discussing the dancing of the bee, the diving of the kingfisher, the ball catching and thinking of humans, I pointed out how *prima facie* implausible it is to think that such skills involve representation of the rules that govern them.  
[211]

But the reader will note that Devitt's 'pointing out' the implausibility of rule-based accounts consisted in his simply *asserting* that it is unlikely that rules would be part of the stories in these cases. (And leave aside the fact that the acquisition of other human cognitive skills, like reading and chess playing, typically *do* involve the explicit learning of rules.) Yet as I pointed out above, many cognitive ethologists have posited mechanisms of great complexity, involving computations over explicit representations of environmental variables, to account for, *inter alia*, navigational skills displayed by such lowly creatures as ants and bees. The main thing is that Devitt never *showed* that the skills he now cites *don't* involve internal rules. He never, indeed, showed that he or anyone knows the first thing about them. It would be one thing if it were *known* to be true that the various

<sup>3</sup>I was confused by this wording in the original. Devitt considerately provided the following gloss in personal correspondence: 'We need wait only until no better explanations are likely, given what we already know.'

animal activities Devitt cites were acquired and deployed without any rule-governed computations. If that were so, we would at least have extant, noncognitivist models to test in the arena of human language. But Devitt does not even *claim* that this is the situation we are in.

But in the end, this doesn't matter, because there's still the second question—is language a skill? Here Devitt must choose. He can either have it that it's an innocent commonplace that language is a skill, or he can have it that something substantively follows from classifying an activity as a skill. But not both. If he thinks that there is such a thing as a 'psychology of skills', and that this psychology tells us that skills do not involve rules and representations, that's fine. But in that case, the claim that language is a skill becomes a substantive hypothesis, in need of empirical defence. Or again, he can take it that it is *merely typical* of nonlinguistic skills that they do not involve rules—in which case, it becomes innocent again to call language a skill, since language could after all be an *atypical* skill. This wouldn't be surprising. After all, human beings are not typical animals. And one way in which we are not typical is precisely in possessing language. What he cannot get is what he wants: that it's a nontendentious observation that language is a skill, and yet that something interesting follows from that.

The absolute bottom line is this. In enjoining us to 'keep a totally open mind about how the organism manages to respect the structure rules of the language', Devitt would like us to believe that empirical enquiry about these matters has only just begun, that we are in danger of settling prematurely on one of many equally attractive and available options. But this is not so. There is a welter of data that cognitivist accounts of language can account for. Devitt's treatment of these data—in his chapters, late in the book, on language acquisition and language processing—shows no more than that the connection between data and cognitivist theory is non-deductive. Devitt never provides what would really be needed to dislodge cognitivism from the frontrunner position: an alternative theory that does at least as well.

## II. Language and Linguistic Competence

Devitt has a positive view to defend. Contrary to Chomsky's view that linguistics is a branch of psychology, Devitt believes that linguistics has a *sui generis* domain: the language itself, as opposed to the competence that produces it. It is the job of linguists to map this domain, to produce theoretically adequate descriptions of the various human languages. Such descriptions can then be passed off to the psychologists, whose proper business it is to discover how human beings manage to produce sounds and inscriptions structured in the way the linguistic descriptions say they are structured. In Devitt's parlance, linguists should discover the 'structure rules' of language, and leave it to the psychologists to discover how human beings manage to generate verbal material that 'respects' those rules.

Devitt's view is developed throughout the book, but important elements of it appear either as 'Methodological Points', 'Distinctions', 'Major Conclusions', or 'Tentative Proposals', (helpfully collected together in

Devitt's glossary). His 'First Methodological Principle' ('MP1') provides a good point of entry.

(MP1): There is something theoretically interesting for a grammar to be true about other than the psychological reality of speakers: it can be true about a linguistic reality.

[35]

Note that this principle actually conjoins two distinct theses: first, that 'linguistic reality' is ontologically distinct from the human faculty or faculties that are connected to it, and second, that linguistic reality, so conceived, is a proper object of scientific investigation. The second does not follow from the first. It is not an *a priori* matter what topic areas constitute proper domains for scientific investigation. We need to know more about the nature of this 'linguistic reality' before we can be confident that it forms a scientific domain. Language, after all, could turn out to be less like electromagnetism and more like etiquette—less a mind-independent phenomenon with objective structure, and more a loosely organized, frequently changing set of informal rules, honoured with varying degrees of seriousness. I'll return to this consideration shortly.

Devitt defends the claim that language is ontologically distinct from linguistic competence by means of a series of analogies designed to establish a general distinction between *products* and the *processes* that produce them. To illustrate the distinction in its most basic form, Devitt discusses blacksmithing. A good blacksmith has the competence to produce (*inter alia*) horseshoes. Horseshoes are ontologically independent of the blacksmith's competence to produce them: the blacksmith's competence to produce a horseshoe is not itself a horseshoe. Horseshoes can be studied on their own without any attention to those skills of the blacksmith that enabled him to make them. Moreover, the good-making features of a horseshoe, the properties that determine it to be a good or a defective token of its type, are independent of any facts about the blacksmith's competence: it is not enough for a horseshoe to be the output of some blacksmith's skill to be counted a good horseshoe. Thus, there can be 'performance errors': 'sometimes what a blacksmith produces is not a good horseshoe' [18]. In sum, we get Devitt's first Distinction:

(D1) Distinguish the theory of a competence from the theory of its outputs/products or inputs.

[17]

Devitt's next step is to explain something about the kind of 'product' Language is. Viewed as an object in its own right, he says, Language appears to be structured in accordance with rules. To explicate the notion of a *rule-governed* product, Devitt appeals first to the game of chess, and then to the 'waggle dance' of the honeybee, performed by one bee in order to convey to its hivemates the location of a newly discovered nectar source. The choreography of the dance, Devitt explains, involves two parameters of variation, each of which carries information about a particular navigational

value: the angle of the bee's waggle signifies the direction of the nectar from the hive, and the number of jiggles in the waggle indicates its distance. The parameters of the choreography constitute the syntax of the dance, and the correspondence relations between the values of the parameters and physical magnitudes in the bee's environment constitute its semantics. The discovery of the structure and meaning of the dance, Devitt points out, was quite independent of our having any understanding of *how* the bee manages to produce it: indeed, while the dance was discovered in the 1960s, the bees' competence is still incompletely understood.

Human Language then, like the bee dance, is a rule-governed product. Human Languages are each governed by a proprietary and complex set of 'structure rules'. Structure rules specify which strings of elements (for Devitt, *concreta* like sounds or marks) constitute sentences in the Language, and speakers are more or less competent to the extent that their linguistic outputs conform to the structure rules of their language.

How does such conformity come to exist, to the extent that it does? Devitt agrees with the orthodox Chomskian that an explanation of speakers' competence will undoubtedly presume that the speaker in some way 'embodies rules that govern the process of producing the appropriate output when the competence is exercised'. He suggests that we refer to these embodied rules, whatever they are, and however they are embodied, as 'processing rules'. Where the Chomskian goes wrong, Devitt tells us, is in presuming that the processing rules *simply are* the structure rules. This presumption flouts his Second Distinction:

(D2) Distinguish the structure rules governing the outputs of a competence from the processing rules governing the exercise of the competence.

[18]

In Devitt's view, the only relation there needs to be between the structure rules and the processing rules is that the latter must 'respect' the former. Hence:

*The Respect Constraint:* A theory of a competence must posit processing rules that respect the structure rules of the outputs. Similarly, a theory of the outputs must posit structure rules that are respected by the competence and its processing rules.

According to Devitt, there is no immediate warrant for positing any tighter connection than this between the structure rules and the processing rules. To do so, he says, would require 'a powerful psychological assumption about competence': either Chomsky's assumption that 'the language has no objective existence apart from its mental representation', or 'one of similar strength' [35]. This assertion becomes Devitt's 'Second Methodological Point'.

(MP2) The view that grammar has any more to do with psychological reality than that it must posit structure rules that are respected by the competence requires a powerful psychological assumption about competence.

[274]

A Fourth Methodological Point asserts that the study of the structure rules of the language is not only independent of the study of linguistic competence, but somehow prior to it.

(MP4) A grammar as a theory of a language has a certain epistemic and explanatory priority over a theory of the psychological reality underlying language.  
[274]

All this together supports Devitt's First Major Conclusion:

(MC1) Linguistics is not part of psychology.

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. In so far as such divisions even make sense (if confirmation is holistic, then the division of nature into 'domains' is artificial and anthropocentric), they are surely provisional and empirical. Magnetism and lightning are conceptually distinct, but science revealed that they are merely phenomenally different manifestations of the same underlying phenomenon. Couldn't human linguistic behaviour therefore turn out to be a mere surface manifestation of an underlying psychological reality? Don't we need to look at the evidence to find out?

Never mind. The ontological distinctness of Language and linguistic competence has, in any case, none of the consequences Devitt thinks it does. As James Higginbotham [1983] pointed out some time ago, even if one thinks that linguistics is properly more concerned with the Language than with the competence that produces it, the drawing of such a distinction does not obviate the question *which* Language it is that a speaker is speaking. The answer to this question might well require investigating the details of human linguistic capacities. After all, no speaker produces or comprehends more than a small finite fragment of the total corpus that constitutes his or her Language. Thus if it's the speaker's Language as a whole that we're interested in characterizing, we'll need to know something that determines the *possible* as well as the *actual* inscriptions and vocables that constitute that Language—we'll need to know, in Devitt's parlance, the structure rules of the speaker's Language.

And what determines what those are? It may be, for all that Devitt has established, that Languages, even while distinct from speakers' competences, are nonetheless *individuated* by those competences. It may be, further, that the only fruitful way to study the Languages that people actually speak is through the competences that generate them. This could be true even if the structure rules of the Language were not embodied in, but

only ‘respected by’ the competences.<sup>4</sup> In short, there’s nothing about the ontological distinctness *per se* of linguistic product from linguistic process that guarantees that ‘linguistic reality’ is a space that can be investigated independently of human psychology.

Let’s look more closely at the nature of Language, on Devitt’s anti-psychologistic view. The linguistic product, Devitt says, consists of ‘physical sentence tokens’ [24]. But this only raises the question, what is a ‘physical sentence token’? Devitt does not want to say that *any* linguistic-like utterance or inscription—any ‘string’ whatsoever—is part of the Language; he wants to distinguish strings that are sentences from strings that are not. How is this distinction to be made? Well, Devitt says,

The outputs of a linguistic competence, physical sentence tokens, are governed by a system of rules, just like the outputs of the chess player, the logic machine, and the bee. Something counts as a sentence only if it has a place in the linguistic structure defined by these rules.

[24]

So all we need to know in order to know which strings are sentences and which are not is what the structure rules are. In the case of chess, of course, it’s easy to find out what the structure rules are—you look them up. But in the case of Language, the structure rules must be inferred from the structures of the sentences. There’s a circularity here, but Devitt is right to think this circularity is, on its own, no more worrying than the usual problems about idealization in science. The delineation of a science’s domain always interacts with the empirical investigation of that domain [27].

... guided by folk linguistics, we start with an intuitive idea of the domain of grammatical tokens to be studied. We do not include many items that seem “unacceptable” to speakers. As our linguistics goes scientific, we modify our view of the domain, accepting some strings that we had thought ungrammatical because they were, say, too hard to parse or “meaningless.” We may even reject some strings previously thought to be grammatical. Linguistics, like other sciences, largely determines its own domain.

[27]

But the matter is more complicated than Devitt acknowledges. The process he is describing actually involves two different kinds of idealization. One is a matter of idealizing away from speaker *error*, and the other is a matter of idealizing away from speaker *difference*. The first kind of idealization is akin

<sup>4</sup>Devitt in fact concedes that facts about speaker’s competences may constrain the theory of the structure rules of a language—thus, his Third Methodological Point:

(MP3): The Respect Constraint makes the justification of the grammar partly dependent on the justification of the theory of competence, and vice versa. Beyond that, however, the grammar and the theory of competence are independent of each other.

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But he’s silent on the matter of how this mutual evidential constraint works, much less how it works without flouting the ‘epistemic and explanatory priority’ of the language’s grammar that MP4 bids us respect.

to the Chomskian's competence/performance distinction. On the Chomskian picture, a speaker's actual output is always the causal product of the internalized grammar (the competence) together with many other factors, including the effects of the competence's interaction with other cognitive systems, like memory, and with the sensory interfaces. Devitt does not want to admit that there is an internalized system of linguistic knowledge, so he cannot appeal to the notion of competence, but it is open to him to say that the operation of the speaker's 'production rules' is subject to the same kinds of interaction effects as are posited by the Chomskian: memory limitations, pronunciation challenges, etc.

But now what about the second sort of idealization? Devitt wants to say that two speakers can speak the same Language despite *systematic* differences in the syntactic constructions they produce and understand, or in the meanings they attach to their words. So consider the following example. I use the expression 'begs the question' to characterize reasoning that presupposes the claim that is supposedly being defended. My students, on the other hand, use that same expression to mean 'urgently raises the question'. The difference between my use of the phrase 'begs the question' and my students' is not a matter of performance error, of memory limitation or pronunciation challenges or the like. And according to Devitt, the mere fact that our usages disagree is not enough to say that we speak different Languages; one or more of us could be making a linguistic error. But if the divergence is a matter of our differing levels of mastery of the same Language, what would make *this* true—what would make it the case that either I or they are speaking our Language ineptly? To put the question in its most general form, what determines which Language it is that any of us speaks?

This question, of course, does not arise for the Chomskian. Chomskian idiolects are individuated by the competences that produce them. But for Devitt, nothing internal to the speaker figures in the individuation of Languages. Production rules, remember, need not be identical to the structure rules for the Language one speaks—indeed, it is Devitt's chief burden to show that they are *not* identical. Rather, Devitt insists, production rules need only be 'similar to' the structure rules of the Language to which they relate the speaker. Production rules, in other words, can neither individuate Languages, nor determine which Language it is that a speaker speaks. But what then does determine what Language a speaker speaks? This is important, because it interacts with the question of which strings go into the corpus of sentences that the structure rules must describe, and hence with the question whether a candidate set of structure rules accurately describes the Language.

In sum, the defender of Language saddles him or herself with a menu of interconnected questions that must be answered, but with no clear way of tethering any of them to the empirical facts. In contrast, the Chomskian who accepts the process/product distinction as a friendly amendment, has clear answers to the ontological questions, and a straightforward research agenda as well. But if the individuation of Languages is tied to the individuation of competences, distinguishing the two is merely a piece of metaphysical

finesse. Devitt has yet to show that a science of Language is viable—or desirable.

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