

surfaces for sensory experience in the project of providing a scientific account of meaning and our cognitive contact with the world. In the theory of meaning, the result is that sameness of meaning is understood in terms of responses to patterns of sensory stimulus. Once we give up the view that sensory experience is an epistemic foundation for knowledge of the world (a view that can be maintained only with the analytic-synthetic distinction), there ceases to be any need to give it a privileged place in the reconstruction of meaning on an empirical basis. Meaning must be understood from a third person stance (for '[l]anguage is a social art'), and this makes sensory experience seem peculiarly unsuited as a basis for understanding. Extruding it to its proximal surface cause is a conservative modification of the empiricist theory of meaning. Davidson takes over from Quine the third person stance, and the view that in matters of meaning and psychology the content of the concepts we use to deal with them are to be explicated in terms of their organization of a body of observational data. But the observational basis takes one more step toward objectivity in becoming the shared distal environment of speaker and interpreter. In this final outward step, the vestige in Quine's philosophy of the traditional role of sensory experience in empiricism, as the basis of meaning and knowledge of the external world, is removed. It is a strange journey empiricism has taken, from the beginning to the end of the twentieth century, one of those transformations of a philosophical view, through a series of internal changes, into something that seems to turn it on its head, and whose resemblance to its progenitor can be understood only by tracing the changes by which the transformation was accomplished.

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 doi:10.1093/mind/fzm405

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Ignorance of Language, by Michael Devitt. Oxford: Oxford University Press, 2006. Pp. viii + 304. H/b £30.00.

Devitt is an avowed friend of the generative enterprise; his brief is to offer an alternative to Chomsky's 'psychologistic' construal of the 'wonderfully successful' work of linguistics, much of it Chomsky's own, of course (p. 3). Hence, Devitt's title: '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). While I agree with many of Devitt's negative conclusions, his positive claims are way off target. To de-psychologise linguistics is a fool's errand, at least if linguistics is to remain an empirical discipline. (Due to space, an issue I shall sideline is whether Devitt can be so sanguine that current generative theory does not carry substantive commitments to a certain psychological design. This issue is

very much live in the debates over the foundations of the so-called Minimalist Program. Suffice to say that sanguinity is not appropriate.)

Devitt claims that ‘linguistics is not simply a branch of psychology. The grammar might *also* be true of a psychological reality, of course, but to show that requires an explicitly *psychological* assumption’ (p. 17). What linguistics is about is ‘linguistic reality’, where this is understood nominalistically as a set of possible occurrences of acoustic signals, orthographic marks, etc. Devitt is not so benighted to think that phonology or syntax is somehow reducible to acoustics or inscriptions. Linguistics is concerned with “‘high-level” relational properties’ (p. 27) of concreta that are ‘largely determined by the mind/brain’ (p. 26). So, linguistic reality ‘largely’ supervenes on psychological reality, but it is not psychological as such. Devitt sets out three general distinctions that animate his contemplated model. Encapsulated, his idea is that competence is distinct from its products and so the ‘structure rules’ governing the latter should be distinguished from the ‘processing rules’ governing the former, although the latter rules must ‘respect’ the former ones, where ‘respect’ does not amount to constitution; ‘respect’, for Devitt, is essentially an external relation (pp. 17–23). Devitt asks us to think of the dancing bees. Von Frisch gave us a theory of structure rules, but we still do not know what the processing rules are—*how* bees do their thing—but whatever the explanation will turn out to be, it will respect the discovered structure rules, assuming that they are a genuine discovery. The charge against Chomsky and many others, then, is that, one way or another, they illicitly take structure rules to be processing rules without any good ‘explicitly *psychological*’ evidence. It is difficult to think of anyone who answers to Devitt’s charge; Chomsky is certainly innocent.

Chomsky has *never* thought that linguistic theory is about psychological processing (a fact Devitt notes but does not heed (pp. 64–5)); indeed, since the mid-Sixties, he has often suggested that we have no coherent conception of mental causation (a fact Devitt neglects to note—see *Cartesian Linguistics*, New York: Harper and Row, 1966, Ch. 1; *Reflections on Language*, London: Fontana, 1975, pp. 16–17; *New Horizons in the Study of Language and Mind*, Cambridge: Cambridge University Press, 2000, pp. 72, 95). Adopting Devitt’s language, there is indeed a respect condition, but it is *internal*: whatever the brain does in terms of processing (which *no one* knows anything about), it respects the structures our true theories of syntax, phonology, etc. posit. Such respect, though, is not deferential to anything external, for one is here concerned simply with the conditions the mind/brain realises that enter into an explanation of the acquisition and maintenance of linguistic competence. Devitt takes a construal of such reasoning to be ‘not only fast but dirty’ (p. 9). Notwithstanding a forest of arguments and counter-arguments, Devitt’s hostility appears ultimately to rest upon an unargued for conception of psychology as solely concerned with processing (analogies with bees and blacksmiths *really* do not constitute arguments) and the metaphysical glibness of high-level relational properties. Let us take them in turn.

What Devitt regards as a fast and dirty move to the ‘psychological reality of grammar’ is in fact simply a demarcation of a domain of inquiry—linguistic cognition—and a proposal for its study—theories of the function computed. There is no argument on offer at all, not even a slow and clean one. Of course, if only certain kinds of data or experimental methods are germane to cognitive inquiry, or if such inquiry can only be an account of processing (something ‘explicitly *psychological*’), then the move *is* fast and dirty, if not completely bankrupt. Yet what is the rationale for such a restrictive conception of what is to count as significant inquiry into cognition? The assumption of the generative enterprise is that the best way, at present, of studying linguistic cognition is just to do linguistics as currently practised, which is not to exclude any future or extant avenues of inquiry, either in parallel or as integrated with the linguistic program. Proposing some other methodology on the model of the bees is fine and dandy, but why all the philosophy? If current linguistics is failing as an empirical account of an aspect of the human mind/brain, then let us have a detailed account of the flaws and their proposed amelioration by way of the bees. Devitt does not even attempt to meet such desiderata. (Chomsky has repeatedly made these points in the guise of a rejection of ‘methodological dualism’, the claim that special criteria, methods or restrictions are in play when we study ‘mental’ phenomena.)

Note, the linguist’s *decision* to study the mind/brain is not premised upon a rejection of the very idea that linguistic properties are realised by external concreta. For many purposes, it is convenient to talk as if they are, but there is no extant empirical inquiry that depends upon them being ‘out there’, whether dependent on us or not. Indeed, as far as I can see, by Devitt’s own merely ‘interpretive’ lights, it would appear that to read linguistic structures as properties of external concreta resolves no problem for linguistic theory nor offers any new line of inquiry; the empirical questions remain exactly as they were, but now set in the context of a free-spinning nebulous metaphysics. Inquiry into bees might well oblige us to make appeal to external concreta, but that would only really tell us that the bees’ internal mechanism is closely integrated (reflexively) with environmental variables (e.g. the location of nectar) in a way in which exercises of linguistic competence are not. (Devitt (pp. 138–9, 156–7) does make appeal to ‘theories’ of reference as establishing language-world links, but such theories have no empirical content and, anyhow, at best ‘fix’ reference rather than control our linguistic behaviour.) In the face of the last fifty years of generative linguistics, Devitt’s presumption that the study of insect *behaviour* offers a default model of how linguistics should proceed is truly bizarre. Devitt, it seems to me, must be in the thrall of ‘folk linguistics’.

Devitt, I know, takes exception to being lumped in with the folk and he does rightly recognise that ‘[l]inguistics, like other sciences, largely determines its own domain’ (p. 27). But if so, why should linguistics not be able to determine its domain as an aspect of human cognition? Why the *ex cathedra* stipulations of disciplinary boundaries and the bee model, which Devitt *explicitly* takes to

map onto our folksy view of language (p. 31)? In short, I can see no argument here save for the folksy one that language is ‘out there’, how we process it is ‘in here’, so linguistics must be about what is out there, what our competence produces (think blacksmiths and horseshoes); we require an independent psychology to account for the processing. To repeat, Devitt has *no* empirical arguments based upon current linguistics to motivate his model (he does appeal to parameter setting, but the reasoning is garbled—see below). One cannot help but suspect that Devitt’s blitheness is due to a disquotational realism: *physics* is about physical reality, *chemistry* is about chemical reality, etc. so *linguistics* must be about linguistic reality (in the absence of something ‘explicitly *psychological*’). Who could disagree (p. 8)? The problem here, of course, is that our independent conceptions, if any, of the various realities constrain neither theory construction nor relevant data. Physics should no more answer to an inchoate conception of its proper domain than should linguistics. If it turns out that our best linguistics has no recourse to external concreta, then so be it. Running one’s semantics off disquotation is bad enough; running one’s philosophy of science off it is a disaster.

What, then, of “high-level” relational properties? One immediate problem is that Devitt makes no serious effort to say how such a notion might properly explicate the denizens of linguistic theory. Devitt (p. 155) does suggest that syntactic properties are ‘functional’ relations ‘*extrinsic*’ to tokens, but this increases the volume of fog. Devitt is happy to think of linguistic properties as being ‘largely determined by the mind/brain’ (p. 26). Why not say ‘more or less wholly determined’ and conclude that our theoretical interest in language should be focused on the mind/brain? Devitt (p. 40) has two quick replies to such a thought. Firstly, ‘in virtue’ of what do external concreta have their linguistic properties is a distinct question from whether external concreta *have* linguistic properties (again, think blacksmiths and horseshoes). Secondly, supervenience does not make for theoretical demarcation; for example, unemployment depends on physical facts, but political economy is not physics; likewise, linguistic facts depend on psychological ones, but the former are not the latter. Both thoughts are irrelevant.

Firstly, we talk of external concreta having linguistic properties, for sure, and perhaps we could devise a metaphysics about such talk, but all that is irrelevant to linguistics qua an empirical science. The only interesting question is whether linguistic properties so construed enter into theoretical explanation. Although it is potentially misleading, I should be happy to say that linguistics is *only* interested in the ‘in virtue’ question: in virtue of what do humans uniquely have the competence to pair sound with meaning over an infinite range? Answer: the language faculty that has such and such properties. Secondly, generativists should not make any appeal to supervenience to argue for cognitive internalism, a thesis that is wholly methodological, not metaphysical. We *decide* to study the mind/brain by whatever means prove fecund, just as we decide to study political economy without recourse to particle accelerators. We

should like different areas of inquiry to be ultimately integrated, but the metaphysics of supervenience offers no guidance how, if at all, such integration might go.

Devitt, for sure, has arguments, to which I shall presently turn, that external factors enjoy some theoretical significance, but, *pace* folksy intuitions, it looks as if external factors are just not salient to current scientific inquiry. In other words, let us grant that linguistic properties *might* arise from a relation between the mind/brain and external stuff, but all the action is at one end of the relation; the other end is noisy, variable and does not submit to independent inquiry. In that respect, we are quite unlike bees.

Take something simple like c-command, one of Devitt's favoured examples. In the sentence *Bob's brother loves himself* neither *Bob* nor *brother* c-command *himself*, which in part explains why English speakers construe the reflexive to be referentially dependent on the whole DP rather than *Bob* or *brother* alone. Presumably, what is supposed to make c-command 'high-level' is that there is no issue here of reducing it or any other posit of syntactic theory to the properties of what we may take to realise it. Presumably, what is supposed to make c-command 'relational' is that there is no *intrinsic* property of any token that determines that a c-command relation is determined. Thus, syntax can be realised by more or less anything one likes (for starters, consider the set of conceivable orthographies, hand gestures, and acoustic signals within the human frequency band). But the rub here is that there is no unity to this heterogeneity save for that provided by the human mind/brain (nothing c-commanded anything 200,000 years ago). Quite trivially, one should be a nominalist about that which we can *take to realise* c-command relations or any other syntactic property, which is to say no more than that there is no science to be had of such an irredeemably noisy domain. Nominalistic properties are simply ways of classifying things. We are only doing something worth calling 'science' when we are revealing underlying unity, even if we do not 'feign hypotheses' (cf. the discarding of *weight* in favour of *mass*). Look at it this way: absent the human mind/brain, that *Bob's brother* c-commands *himself* is no more interesting a property of our example sentence (understood as an inscription) than that the pairs <h, h> and <e, e> are cross-serial (in fact, of course, the sentence would have to be projected into a hierarchical structure for c-command to be realised, but let that *serious* complication pass). The only conceivable reason to pick out c-command is that it, as opposed to an indefinite number of other properties, enters into an *explanation* of human cognition (e.g. judgements of referential dependence). Generally, *only* 'high-level relational properties' of acoustic signals, etc. that enter into cognitive explanation are of interest to linguistics. In short, it is only the mind/brain that saves the putative linguistic reality from being pure noise, give or take one per cent. This conclusion, note, is not a *proof* that there is not a linguistic reality; it is only intended to demonstrate the irrelevance of the idea to current linguistic thought and any other conceivable science.

Devitt (pp. 184–9) has a response to a similar ‘noisy’ complaint from Georges Rey, but it compounds rather than resolves the present problem; for Devitt merely avers that while, say, *being Australian* (or a paperweight or whatever) is satisfied by a heterogeny, it remains a kosher property; indeed it does, but properties are cheap. The only issue of empirical interest is whether the relevant properties enter into kosher theoretical explanations, and it is the very heterogeny of *being Australian* that tells us that no science is to be had here, save, perhaps, for cultural studies. There must be a unity to *being a c-commander*, say, if that notion is to enter into an explanation of referential dependence, *inter alia*, across the board. Yet the only unity there is to *c-command* is to be found in cognitive states, not acoustics or inscriptions or hand gestures.

Devitt, for sure, takes up the challenge of showing how linguistics is indeed concerned with a linguistic reality. I shall focus on just two aspects of Devitt’s complex presentation.

Devitt’s chapter seven is dedicated to the status of linguistic intuitions. Here, Devitt rejects a ‘Cartesian’ conception of linguistic intuitions, under which they are the ‘voice of competence’, in favour of one where they are just like intuitions generally: unreflective, theory-laden judgements. They are *about* the products of competence rather than a transparent access to the competence itself. Devitt is interested in intuitions because he takes the Cartesian conception to be a key argument for a representational construal of the language faculty; thus, if that conception is wrong, or at least doubtful, then, again, we lose an argument for the psychological reality of language. While the Cartesian conception is a position in the field, its adoption is not mandatory for the ‘Chomskyan’, especially since the construal is not mandatory either. A far simpler methodological reflection is that, since we are interested in the mind/brain, an informant’s intuition is, *ceteris paribus*, a good source of data, just as it is when we are studying vision, theory of mind, or any other competence. We need no fancy philosophical argument for this claim, merely for *ceteris* to be *paribus*, that is, for there to be an absence of other conflicting data sources of equal or greater richness and availability and any other reason to think that the intuitive data are somehow systematically misleading. If *ceteris* were not so *paribus*, then a reliance on intuition would raise serious methodological concerns. It bears emphasis that this defence of intuitive data does not so much as suggest that other data sources should not be exploited as and when available, with the usual assessment of relevance and theoretical integration in play. It seems to me that this has been Chomsky’s model since the mid-Fifties. Although Devitt does not consider such a modest construal of the data issue, he may be read as offering a response to it (here I select from Devitt’s complex presentation).

Firstly, much of the data for linguistic hypotheses are not intuitions (pp. 98–100). Devitt’s case for this claim is very poor, as all the alternative data sources he cites, such as psycholinguistic evidence and experimental design, are clearly probing the speaker/hearer’s judgements. The only potentially relevant case

for his claim is corpus studies, but they only provide some indication of the character of the primary linguistic data; syntactic hypotheses are not claims about what people have said. Besides, *none* of the data sources Devitt mentions conflicts in the least with intuitive judgement.

Devitt also thinks that intuitions are limited in not being *about* heads, c-command, A-positions and the other exotica of linguistic theory (p. 101). But this is surely banal. Intuitions may serve as *evidence* for any aspect of linguistic theory. For example, ambiguity judgements are often employed to test for reconstruction effects involving A-argument chains. Referential construal judgements indicate headedness and c-command, and so on throughout the technology of linguistics. So, that intuitions are *about* X (whatever that might mean) does not mean that such intuitions might not serve as evidence for X, Y, and Z. In science, we care about evidence, not aboutness.

Devitt also seeks to establish the interest of 'linguistic reality' via a discussion of conventions. Devitt's reasoning here fares as well as his thoughts on intuitions.

Devitt's concern for conventions enters into the picture by way of a 'Gricean' account according to which linguistic competence is merely a means by which we clothe a more primitive conceptual structure, a *language of thought*. In a sense, we can see Devitt as squeezing the putative mentality of language between the high-level properties of the external linguistic reality and the internal structure of thought. Thus, there is a psychological reality to language, but it is simply a reflection of the structure of the *language of thought* (pp. 256–60). Conventions enter the picture as part of the Gricean story: conventional linguistic forms support the communicative intentions based in an agent's thought (p. 156).

The general relation between language and thought is as unsettled as any other area of cognitive science; Devitt offers very little assistance. For instance, he neglects the 'big fact' that linguistic structure does not match the apparent structure of 'thought', as witnessed by raising/control pairs, c-selection, lexicalisation, ambiguity, passivization, island phenomena, 'illusions', case, etc. One could take the measure of this fact as a measure of the independence of linguistic competence from 'thought'. (Devitt (p. 152–8) does discuss some issues in syntax, but only in a cursory manner and not in answer to the present complaint.) More generally, Devitt's discussion is too bound to the usual philosophical preoccupation with communication and meaning that the Gricean story enshrines. We should not expect empirical inquiry to involve such nebulous philosophical intuitions.

On conventions in particular, Devitt offers a flurry of counter-arguments to Chomsky's familiar doubts (pp. 178–84). The major flaw in Devitt's discussion is that at no point does he explain what notion of convention is relevant to language. He appears to think that mere regularity, give or take some mutual agreement, is enough for his purposes. I shall gladly leave it to others to speculate on the nature of conventions, but what remains clear is that convention

does not enter into any serious empirical inquiry into language. Devitt thinks otherwise given the common talk in linguistics of differences between languages, but this talk is easily paraphrased as referring to populations of humans with relevantly similar language faculties.

Similarly, *pace* Devitt, the familiar parameter setting model of language acquisition does not depend on conventionality. The story is simply that, noise apart, if someone grows up among speaker/hearers whose faculties were set to X, as it were, then the person will have their faculty set to X. No appeal to convention need be made, not even one of mere regularity. Of course, just how parameters do get set is a live issue involving numerous competing theories, but I know of no work which appeals to conventions, and nor does Devitt.

While I have been highly critical of Devitt's work, the book contains a wealth of careful distinctions and detailed arguments that should be of interest to anyone interested in the philosophies of language and mind. The book will infuriate many, but everyone should recognise it as an example of how serious philosophy of a very technical area may be conducted with thoroughness, lucidity, and elegance.

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 doi:10.1093/mind/fzm416

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Scepticism Comes Alive, by Bryan Frances. Oxford: Oxford University Press, 2005. Pp. xii + 209. H/b £35.00.

Consider the following scenario. Imagine that at university you are taught, as part of a physics class, some claim of theoretical physics, and as a result you form a belief in that claim, a belief which, as it happens, is true. Suppose further, however, that in later life it comes to your attention that there is in fact some significant disagreement amongst the experts as regards this claim. The size of the two competing camps need not be equally weighted; perhaps, for example, the dissenting scientists make up only a relatively small proportion of scientists who work on this area—twenty per cent say. Nevertheless, these dissenting scientists are generally regarded as first-rate (or at least no less first-rate, as a rule, than scientists from the opposing camp), and so their view, even when in the minority, cannot easily be dismissed as merely a 'fringe' theory. Finally, suppose that although you know of the debate and its general contours (both scientific and sociological), and may even have become a physicist, you have no expertise on the debate's issues. Here is the question: what is the epistemic status of your true belief in the claim that you were taught, given that this new information has come to light? I take it that the natural answer to this question is to say that whatever the positive epistemic support you have