

Think of the Children

Paul M. Pietroski, University of Maryland

Often, the deepest disagreements are about starting points, and which considerations are relevant.

1. Introduction

Michael Devitt thinks that Noam Chomsky is fundamentally misguided about the nature of human language and the subject matter of linguistics.

Devitt lauds the research enterprise that Chomsky initiated, but rejects the following construal of the enterprise: it is a branch of psychology, perhaps someday biology, focused on a species-specific faculty that lets humans acquire languages of a distinctive sort; this faculty has various stable states, often called “I-languages,” corresponding to the highly constrained ways in which humans can naturally associate linguistic signals with meanings; and given our experience as children, we settle into one or more I-languages that are typically shared by local adults. I think that Devitt fails to engage with the best arguments for this conception of linguistics. He discusses language acquisition only briefly, in the final chapter. And he never considers, in any detail, “negative” facts of the sort that illustrate interesting ways in which humans *cannot* naturally associate linguistic signals with meanings.

Devitt offers an alternative conception with several connected components, only a few of which I can discuss here: linguistics is not part of psychology; correct grammars reflect aspects of a linguistic reality that is somehow independent of any Chomskyan mental faculty; there is little if any substance to this alleged faculty, because in so far as principles of universal grammar have any psychological reality, this is at least largely because such principles reflect prior rules of thought; so the intuitions that linguists routinely use as data, in arguing for or against hypothesized rules of grammar, are not the “voice” of a Chomskyan competence; there is no good reason for thinking that ordinary speakers mentally represent grammatical rules; but rules of grammar are “psychologically real,” at least in the minimal sense that human psychology must somehow “respect” such rules. Devitt endorses these claims with varying degrees of conviction. But on my reading, Devitt’s case for his own view relies on some unwarranted assumptions about linguistic competence and I-languages. Spelling this out will require discussion, in section two, of some data. This will limit my discussion of Devitt’s rich and stimulating text. But section three addresses what I take to be his central claims, regarding his disagreements with Chomsky.

1. Doubts about Method

In his first sentence, Devitt tells us that “The major concern of this book is to investigate the respect, if any, in which the rules or principles of a language are ‘psychologically real’ in its competent speaker/hearer (p1).” He goes on to consider many ways in which hypothesized rules might be psychologically real (Ψ -real). Since he asks us not to fuss about ‘rule’, I won’t. Devitt devotes considerable attention to what he calls the “Representational Thesis” (RT), according to which ordinary speakers mentally represent grammatical rules; where ‘represent’ is to be understood in the sense of a substantive Representational Theory of Mind. I share his skepticism about (RT). For present purposes, let me also grant that currently hypothesized grammatical rules are not Ψ -real in various other senses that Devitt discusses and rejects.

Devitt says that “Chomsky is naturally interpreted as urging RT,” citing various passages, while conceding that this “interpretation of Chomsky may not be right (p.6).” The exegetical issue turns in part, as Devitt notes, on Chomsky’s conception of representation. It also turns on whether the passages cited are intended to (i) import *theoretical* vocabulary that Chomsky goes on to use, or (ii) introduce readers to the general ideas, in an accessible idiom, before getting

down to work. But in any case, linguists can say that their proposed rules describe the nature and operation of the language faculty, in an idealized way that we hope to improve on as inquiry proceeds. (Compare Boyle's Law, the ideal gas law, van der Waal's equation, and statistical mechanics.) This is, I believe, Chomsky's view: the language faculty, which makes certain I-languages possible, imposes constraints on how children can associate (say) the sounds of English with meanings; generalizations that capture these constraints reflect substantive truths about the language faculty; but since our theoretical vocabulary remains inadequate, we are still trying to formulate the real underlying principles.

This is not a way of being Ψ -real that Devitt seriously considers. Of course, he wants to challenge the "faculty construal" of linguistics, which goes beyond the mere claim that human minds "respect" grammatical principles. But for just this reason, I don't find it helpful to enumerate ways of being Ψ -real. Grammatical rules might be "laws of the language faculty," even if such rules are not Ψ -real in *other* ways that philosophers might care about. Note that we do not ask, in the abstract, whether the ideal gas law is "chemically real." We think the "gas rule" is roughly right because it, or a close variant, is reducible to more basic principles.

Indeed, one might have thought that a philosopher interested in linguistic "reality" would attend to theoretical *developments* in linguistics that were prompted by recalcitrant facts and/or attempts to derive some principles from others. But Devitt explicitly prescind from considerations that have driven theory change.

Chomsky's revolution in linguistics has really been a series of revolutions moving from the early Standard Theory to Government and Binding and recently to Minimalism. The concern of this book is not with the linguistic issue of which rules of a language are right, it is with the place of the right rules, whichever they may be, in the mind. The concern is not with the linguistic details but with an issue raised by the whole enterprise. So, for the most part, I shall abstract from the differences between the members of this series (p. 15).

I find this puzzling. *Prima facie*, we should pose questions about the relation of linguistics to psychology in the context of our best current theories. And we should focus, in so far as possible, on grammatical principles formulated without unnecessary and distracting notation—especially if that notation was introduced decades ago, for purposes of initially *describing* grammars without yet worrying about *how* the formal notation was related to human psychology. Yet the Minimalist Program, geared in part to these very concerns, is never mentioned apart from the quote above.

Moreover, I don't see how anyone can—or why anyone would think they can—figure out how grammatical rules are (not) related to human minds, while abstracting away from the details that animate linguistic theory. The project seems to call for attention to actual theories, and reflection on how they can be improved, in light of the many considerations that linguists advance; see, e.g., Hornstein (1995, 2000), Lasnik (2000), Martin, et.al. (2000), and Uriagereka (1998) for some suggestions. Here, I can only offer reminders of some basic phenomena that motivated both Chomsky's research program and his mentalistic conception of language.

2. Negative Explananda and Intuitions

Language acquisition is not merely a process in which one acquires a capacity to associate boundlessly many signals with interpretations of some kind, in a way that allows for agreement across individuals about what each signal means. Competent speakers have acquired much more.

As Chomsky (1957) noted, we somehow know that (1) is weird, unlike (2).

(1) * I may have been there

(2) I may have been there

And meaningful strings of words often exhibit ambiguity, though only in constrained ways; see Chomsky (1965), Higginbotham (1985). To borrow a famous example, while (3) is ambiguous,

(3) The goose is ready to eat

(4) and (5) are each *unambiguous* in different ways.

(4) John is eager to please

(5) John is easy to please

The word-string in (3) can mean that the goose is ready to dine, or that it is *pret-a-manger*. But (4), paraphrased with (4a), *cannot* have the meaning indicated with (4b); and (5), paraphrased with (5b), *cannot* have the meaning indicated with (5a).

(4a) John is eager that he please us

(5a) #It is easy for John to please us

(4b) #John is eager that we please him

(5b) It is easy for us to please John

Mature speakers of English know such facts, at least upon querying. Human children, unlike bees and chimps, can acquire such knowledge about what signals can and cannot mean.

Speakers often agree about what a string of words cannot mean, even when the unavailable interpretation would be *more* reasonable than the mandatory interpretation. Note that (6) cannot be understood as the sensible yes/no question corresponding to the declarative (6a).

(6) Was the boy who fed waffles today fed the hungry kittens at noon

(6a) The boy who was fed waffles today fed the hungry kittens at noon

Rather, (6) is unambiguously the yes/no question corresponding to (6b).

(6b) The boy who fed waffles today was fed the hungry kittens at noon

Such facts bear on Devitt's development of Fodor's (1975, 1998) view, according to which (contextualized utterances of) English sentences get associated with sentences in a language of thought. Chomskyans can endorse the idea that principles of Universal Grammar help shape the character of distinctively human thought; see Pietroski (2005, 2006). But if the language faculty does not impose constraints of its own, and English expressions are devices for expressing *preexisting* expressions of Mentalese, one needs to say *why speakers cannot* associate word-strings like (4-6) with the coherent and expressible thoughts that cannot be expressed with these strings of English words. Unambiguity reveals constraints, which come from somewhere, on how sentences of a naturally acquired spoken/signed language can be related to sentences of Mentalese; see Higginbotham (1994), Pietroski (2000, 2005).

With this in mind, let's return to cases. Idealizing, we can say that (3) exhibits two sentential meanings, while (4) exhibits one-but-not-two; 'Was a vet saw the dog that found' exhibits none. Endlessly many word-strings exhibit two-but-not-three sentential meanings. And so on. For example, while (7) is ambiguous, it cannot have the reading indicated in (7c).

(7) The doctor called the lawyer from Texas

(7a) The doctor called the lawyer, and the lawyer was from Texas

(7b) The doctor called the lawyer, and the call was from Texas

(7c) #The doctor called the lawyer, and the doctor was from Texas

By contrast, 'The doctor from Texas called the lawyer' has the reading indicated in (7c).

Scope constraints have been discussed extensively. Consider 'It is false that every boy is purple', which fails to have a second reading according to which for every boy, it is false that he is purple.

Devitt does not discuss such examples. But facts concerning unambiguity, which have long occupied center stage in Chomsky's work, are directly relevant to mentalistic conceptions of human language. So critics need to say what is wrong with Chomskyan diagnoses of such facts. If we don't have a language faculty that makes a *constrained* range of I-languages available, for acquisition and use, then how *do* we know so much about what previously

unencountered word-strings *cannot* mean? One cannot responsibly reject Chomsky's conception of linguistics without a plausible alternative diagnosis of the relevant negative facts.

Devitt does, however, have a chapter on intuitions. This is potentially relevant, since he rejects Chomsky's (1957, 1965, 1986) conception of how the available data is related to theory in linguistics. But let me state what I take to be a standard Chomskyan view, and locate the point at which I believe Devitt goes wrong in his characterization of the data.

The string of words in (1) sounds *worse*, to native speakers of English, than the string of words in (2). For practical purposes, that's data. But it won't matter if you adopt a demandingly observational conception, and conclude that what actual scientists call data are really tentatively inferred phenomena, which can be tentatively cited as evidence for other phenomena that scientists actually want to explain—like why water expands when it freezes, or why auxiliary verbs pattern as they do in human languages; see Bogen and Woodward (1988). The important point is that Chomskyans can and do offer specific versions of the following idea about *why* (1) sounds worse than (2): given rules that govern I-language analogs of what we call English, (1) cannot be classified as a grammatical sentence, though (2) can be.

There is ample room for precisification here. For one thing, grammaticality is probably a gradable notion, not a simple yes/no matter. (This is *one* reason why Chomsky [1965] dropped his earlier and explicitly simplifying analogy between grammaticality and well-formedness in invented languages.) But if only because grammaticality is employed as a theoretical notion, one would prefer not to use this notion in characterizing the data used to confirm linguistic theories. So linguists often use a notion like 'acceptability', shorn of any essential connection with grammar, in order to describe intuitions that may serve as relevant data. Moreover, it just seems unlikely that an ordinary speaker's intuition that (1) is worse—or if you prefer, the speaker's rapid judgment that (1) is "yucky"—is an intuition *of* ungrammaticality; though perhaps it is an intuition of yuckiness. A speaker may be able to tell, by introspection, that he cannot naturally associate a given word-string with a sentential meaning, yet be unable to tell *why* he is "blocked" in this way. Speakers typically do not know whether the "block" is grammatical at all, and this is unsurprising. (Subjects in a vision experiment may be unable to see any image emerging from a given stereogram, or be able to see a Necker cube in two ways, while being clueless about why.)

A string like 'The rat the cat the dog chased chased ate the cheese' sounds bad, even though this string can (so far as grammar is concerned) be classified as a sentence. Here, the yuckiness may be due to memory limitations on center embedding. Many factors, including pragmatic ones, can make some word-strings (as uttered in certain contexts) sound worse than others. Such asymmetries can be reported as data, often with a simplifying '*' on one of two strings that form a pair of superficially similar expressions chosen to highlight the contrast. On many pages, Devitt recognizes these points. Nonetheless, he says, "What a linguist is aiming to elicit from an ordinary speaker is, of course, an intuition about what is *acceptable grammatically in her language*; he wants the voice of her competence (p. 102, his emphasis)."

I disagree. One can grant that there are many kinds of (un)acceptability, without concluding anyone has *intuitions about* grammaticality. One can observe that speakers find a string weird—or that speakers cannot hear the string as having a certain meaning—while remaining agnostic about why this is so. When theorists are native speakers of the relevant languages, initial data collection is easy. (But observations easily made can be important. It's obvious that the moon and planets have trajectories like that of the sun, and *unlike* those of stars.) Devitt is led, by the conjunction of his various views, to conclude that the intuitions linguists regularly cite as data are theory-laden judgments about grammaticality—and hence, that trained

linguists are better sources of data than naive speakers. But this revisionary conclusion is unsupported by any independent evidence.

Devitt goes on to make useful remarks about what one shouldn't take intuitions to be. Many judgments that philosophers might call intuitions cannot play the evidential role that linguists assign to what they call intuitions. In particular, as Devitt says, we should distinguish *what* speakers intuit from their *intuiting* of it; see also Lycan (1986). Chomskyans can agree.

The important point, though, is that naive native speaker intuitions reveal the unambiguity of strings like (6) above, and thus help confirm hypotheses like the following: competent speakers can classify (6) as the sentence indicated roughly in (6A); but because of how I-languages are constrained, an auxiliary verb cannot be “displaced” from a relative clause like ‘who fed waffles’; so while we can imagine creatures who could also classify (6) as the sentence indicated roughly in (6B), this second way of associating the word-string with a coherent meaning is unavailable to humans.

- (6A) Was {[the boy (who fed waffles today)] [___ fed the hungry kittens at noon]}
 |_____|
 (6B) Was {[the boy (who ___ fed waffles today)] [fed the hungry kittens at noon]}
 |_____X_____|

In cases where the facts are unclear, and especially when studying children, it takes work to collect data that confirm suspicions of unambiguity. One needs to ensure that responses—ideally contextualized utterances of ‘yes’ vs. ‘no’, or ‘better’ vs. ‘worse’—form a significant data set. But as Devitt himself notes (p.99), psycholinguists have ways of doing just this. So do eye doctors.

Let me mention one last kind of example, characteristic of Chomsky's work in the 1980s. We know that (8) can have the indicated readings—with ‘her’ as a pronoun whose antecedent is ‘Pat’, or as a referentially independent deictic expression—but not the logically possible reading on which ‘her’ is referentially dependent on ‘Chris’.

- (8) Pat_i wants Chris_j to bathe her_{i/k/*j}

In (9), ‘herself’ is unambiguously dependent on the closer name, ‘Chris’.

- (9) Pat_i wants Chris_j to bathe herself_{j/*i/k*}

But in (10), ‘her’ can depend on ‘Chris’, though it cannot depend on the distant name ‘Pat’.

- (10) Pat_i wants to feed Chris_j and bathe her_{j/k/*i}

And in (11), ‘herself’ is unambiguously dependent on ‘Pat’.

- (11) Pat_i wants to feed Chris_i and bathe herself_{i/*j/k*}

Competent speakers, including very young ones, know such facts; see Crain and Pietroski (2000) for a review, in the context of criticizing empiricist conceptions of language acquisition.

Nativist accounts of “binding phenomena” posit a substantive language faculty that makes available only I-languages that respect certain locality constraints on antecedence, while *also* mandating a covert subject of ‘feed’ in (10) and (11), while *also* mandating certain general principles of composition. The idea is to defend each of these subhypotheses about the language faculty independently, and then explain the otherwise puzzling pattern illustrated with (8-11) as a consequence of multiple constraints on human I-languages. This neither requires nor forbids the additional hypothesis that children work out the implications for (10) and (11) in the way that linguists do. The basic Chomskyan claim, which is surely a psychological hypothesis, is that humans have a mental faculty that makes only certain languages available to us as resources for associating signals with meanings.

While Devitt does not explicitly address these cases, he does suggest—in defense of Cowie (1999)—that they fail to support a Chomsky-style nativism about language acquisition, even despite the absence of alternative accounts. Devitt seems to think that empiricists can reply by declaring nativist accounts “not good enough” to be explanations at all (p. 253). Chomskyan nativists often respond to critics by citing *lots* of negative facts that turn out to be systematically related, offering specific explanations of those facts in terms of a language faculty that makes sense of other facts (like creolization), arguing that the posited principles are independently plausible given the character of human language acquisition and the (experimentally discovered) age at which such principles are “respected,” and then noting that the critics offer no alternative accounts at any level of detail; see Crain, Gualmini, and Pietroski (2005) for a case study. Devitt calls this a “plonking” response. But he does not offer any alternative explanations for the facts.

In this context, it is especially important to distinguish (i) general questions about why and how children acquire the languages they do from (ii) more specific questions about the causal processes that children undergo, in response to their experience, given their innate endowment. Suppose, perhaps counterfactually, that no Chomsky-inspired response to (ii)—say, in terms of parameter setting—is sufficiently articulated and plausible to count as a significantly better theoretical alternative to empiricist speculations of the sort Cowie offers. It hardly follows that Chomsky-inspired responses to (i), in terms of proposed constraints on the space of options available to human children, are no better than such speculations. Empiricist critics owe an alternative account of how children can acquire the grammars they do. With regard to “explaining language acquisition,” the bulk of successful theorizing may lie with explaining why children never acquire certain coherent signal-meaning systems, and not with explaining how experience determines *which UG-compatible language* a child will acquire. And theorists who propose Chomskyan constraints in response to (i) are offering psychological hypotheses about human language acquisition; though of course, these hypotheses leave open many interesting questions about the actual causal role of experience.

3. Assumptions and Alleged Truisms

Devitt says at the outset that Chomsky’s program in linguistics “starts from the assumption that a person competent in a language *knows* that language” and then “defines the linguistic tasks in terms of this knowledge” (p.3, his emphasis). I disagree. If the program “starts” anywhere, it starts with various *observations* concerning examples like (1-11) above. And *pace* Devitt, Chomsky’s (1986) useful pretheoretic division of research questions—what do speakers know, how they acquire this knowledge, and how do they put it to use—is not a *definition* of “the linguistic tasks.” We discover explananda as inquiry proceeds. And the tasks will surely grow, as Chomsky and many others have stressed, if only because we must eventually explain how primate biology gave rise to the faculty that makes it possible to acquire the knowledge we use.

Of course, we can summarize a host of observations about examples like (1–11), by saying that children who acquire a language come to know it. But the epistemic locution is not required, even if ordinary English prefers it; as Chomsky regularly notes, one can speak of *having* a language, as many humans do. In any case, the starting point is not that speakers are somehow epistemically related to languages. This conjecture, like RT, is no part of any *theory* that Chomsky has advanced. On the contrary, the hope is to provide illuminating descriptions of the I-languages humans come to have, and then explain how certain primates come to have such languages.

I would prefer not to harp on this, since Devitt prefers to echew talk of knowledge in this domain, and speak in terms of *competence*. But Chomsky explicitly rejects the conception of

linguistic competence that Devitt finds truisitic. According to Devitt, who evidently follows Dummett (1978) in this respect, “To be competent in a language is to be able to produce and understand the expressions of that language (p.4).” I won’t review Chomsky’s (1980, 1986) reply to this proposed characterization of linguistic competence. And set aside the insistence on *production*, which invites thoughts of laryngitis and aphasia. Devitt’s conception of competence still seems unduly narrow. What about recognizing yuckiness and (un)ambiguity, entailments, and so on? On Devitt’s view, these are not manifestations of competence. But one needs a *very* good argument for limiting the notion of competence in this way, given the research program—based on a broader conception of competence—that helps explain why speakers recognize a lot of yuckiness, (un)ambiguity, and so on. More specifically, one needs alternative accounts of the relevant explananda, not a restatement of commonsense remarks about linguistic competence.

To be sure, a competent hearer can often determine what a speaker of the language conveyed in accordance with public conventions governing the use of relevant expressions. One can talk about a disposition-to-understand, in this neo-Gricean sense that Devitt favors. And this disposition, so defined, might be acquirable by beings who lack a human language faculty. But why think this disposition, so defined, is the core of human linguistic competence? Given enough data and time, even a Quinean Field Linguist might approximate human children with regard to how typical speech gets dealt with in context. But that is irrelevant; see also Marcus (1998). Devitt rejects Quine’s behaviorism. But there is still an attempt to limit the scope of linguistics, and restrict the notion of competence, in ways friendly to empiricist conceptions of language acquisition that remain unarticulated or obviously inadequate.

Devitt talks about the blacksmith’s “competence” to produce horseshoes, the chess player’s competence to move pieces, bee dances, and machines that generate well formed formulae of an invented calculus; cf. Wittgenstein (1953), Dummett (1975), Millikan (1989). Strikingly, Devitt says that for any competence,

the *very nature* of the competence is to produce its outputs: producing them is what makes it the competence it is. Thus, the blacksmith’s competence is (partly) the ability to produce horseshoes; the chess player’s, to produce chess moves, things governed by the structure rules of chess; the logic machine’s, to produce *wffs*, things governed by the formation rules; the bee’s, to produce dances, things governed by the dance rules (p. 22, his emphasis).

Likewise, he claims, the nature of linguistic competence is to produce/understand expressions. On Devitt’s view, we know this already. But working linguists don’t know what the nature of linguistic competence is, or *how* it is related to production and comprehension. The project is, in large part, to *discover* this by doing science.

It seems rash, and contrary to the rhetoric of naturalized epistemology, to draw conclusions about the nature of linguistic competence from general claims about the very nature of competences, especially when our best linguistic theories cast doubt on the idea that there are “outputs/products” such that linguistic competence is a capacity to produce them. But Devitt insists, on what seem to be *a priori* grounds, that the doubt cannot be cast.

According to him, even Chomskyans must grant that we can provide theories of the expressions said to be generated by the language faculty, without actually positing the faculty and taking it to be the object of theoretical inquiry—just as we can talk about the horseshoes produced by the blacksmith, without talking about the smith’s competence. Again, this is supposed to be a truism, despite its dramatic consequence: 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. But if I understand this argument, which is crucial to his larger project, it doesn't work.

Given *both* a theory of a competence *and* a theory of its "outputs/products or inputs," we can and should "Distinguish the theory of a competence from the theory of its outputs/products or inputs (p 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 analog 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.) In any case, 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. Proposals that account for facts of the sort illustrated with (1–11) are not mere claims about the expressions we can actually generate; they are claims about the psychology that lets us generate those expressions *but not others*. Critics of Chomsky (1957, 1965, 1980, 1986) need to engage with this point.

If we are given a theory of linguistic expressions that *isn't* a mentalistic Chomsky-style proposal in disguise, we can ask whether that theory is "good enough" to trigger Devitt's maxim. But until someone presents such an alternative, which will have to address examples like (1-11) above, 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 can define an enterprise that is unconcerned with negative facts and their source—just as one can define an enterprise of describing actual planetary motions without regard to terrestrial motions, the tides, or black holes. But scientific enterprises are not so restrictable.

One can't just cite the grammatical principles hypothesized by Chomskyans, and declare that these principles are somehow made true by facts independent of the posited human language faculty. One needs to say what *else* might be responsible for the relevant *constraints on how* humans associate signals with meanings. Moreover, despite Devitt's claims to the contrary, there are reasons for suspecting that there is no theory of *expressions* to be had—and hence, that Chomsky's project does not provide the outlines of such a theory. This will be my last point.

Devitt recognizes the relevance of Chomsky's focus on I-languages. But when Chomsky expresses skepticism about E-languages, the main worry is not about Platonism or publicity. While the 'I-' connotes individuals and idiolects, Chomsky's primary point is different: a science of human language needs to characterize languages *intensionally*, as certain *ways of associating* signals with meanings, and not *extensionally* as sets of input/output pairs; cf. Lewis (1972). As Chomsky has long stressed, we have no theoretical grip on what expressions of a human language *are*, apart from our tenuous grip on the recursive procedures that humans somehow implement by virtue of having I-languages. And as examples like (12) reveal,

(12) The child seems sleeping

a string of words can be somehow weird yet still comprehensible. We know what (12) means—viz., that the child seems to be sleeping—and that (12) *doesn't* mean that the child seems sleepy; see Higginbotham (1985). By contrast, for most Americans, (13) initially sounds terrible.

(13) The lawn needs mowed

Though for some, it is a perfectly fine way of saying that the lawn needs to be mowed.

If a theorist says there are facts about what “the sentences of English are,” apart from facts concerning the I-languages of humans classified as “speakers of English” for political/geographic reasons, then the theorist needs to say what those sentences *are*—and how we can find out if (12) and (13) are examples; see Lasnik (2000). Devitt provides no help here. Nonetheless, he says,

The competence in the language is not the language any more than the blacksmith’s competence is a horseshoe, the chess player’s competence is chess, or the bee’s competence is its dance. The linguistic competence is in the mind/brain, the language is not (p. 23).

According to Devitt, even Chomsky ought to agree, because: “Chomsky assumes that competence consists in knowledge about the I-language” (*sic*, see above); and “this very I-language is, indeed *must be* at the appropriate level of abstraction, the output of that very competence (p. 34, his emphasis).” Now if ‘I-language’ meant what it means for Chomsky, this last claim would diverge crucially from talk of *expressions* being the outputs of linguistic competence. One would speak instead about a competence for generating *algorithms*, each of which associates signals with meanings in a constrained and distinctively human way. That is Chomsky’s picture. But as the quotes and surrounding discussion suggest, Devitt is thinking about I-languages as, in effect, individual E-languages: sets of sentences that may be—though typically are not—idiosyncratic. And one needs to characterize these sets, without appealing to a language faculty, in order to make Devitt’s case against Chomsky.

If my reading is correct, Devitt’s misunderstanding of ‘I-language’ is significant. For *if* I-languages were individual E-languages, then it might be possible to characterize them independently of the language faculty, not worry so much about negative facts, and offer theories of I-languages without commitment to a Chomskyan language faculty. But ‘I-language’ is a technical term, introduced by Chomsky to *distinguish* his view from views like Devitt’s. So it is hard to see how Devitt’s claims about I-languages could be correct. I don’t see how to recast these claims, while retaining the role they need to play in Devitt’s argument, without appealing to I-languages. So in the end, I’m not sure what the alternative to Chomsky’s construal of linguistic theory is really supposed to be, or why we should adopt it.

In the middle of Devitt’s book, not discussed here, he makes a good case that following his initial line of thought leads to an anti-Chomskyan conclusion that is nicely summarized early on.

[I]t is because of shared conventional meanings in a group that language can play its important role of making the thoughts of each member of the group accessible to the others. It is the task of linguistics to explain the nature of these conventional meanings....the primary concern in linguistics should not be with idiolects but with linguistic expressions that share meanings in idiolects (p. 12).

But constraints on I-languages are not, in any ordinary sense, conventional. And focusing on aspects of language that are not conventional has been a remarkably productive research strategy. There are no results suggesting that the program Devitt recommends would be as fruitful. History should also make us suspicious, when philosophers are led to revisionary claims about what linguists ought to be doing—and what competences are essentially—on the basis of arguments that abstract from details of the sort that occupy working linguists.

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