The Many Errors of Vyvyan Evans' *The Language Myth*

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Abstract

Vyvyan Evans' *The Language Myth* argues that Chomsky's program of Universal Grammar (UG) is "*completely* wrong," and it has attracted much recent discussion, some of it laudatory. We set out what we take to be its many serious errors, including: (i) a misunderstanding of the empirical character of the evidence that Chomsky and other generativists have adduced for UG, in English as well as in many other languages, coupled with a mistaken claim that the theory is unfalsifiable; (ii) a confusion of superficial *typological* universals, or features present *at the surface* of all of the world's languages, with UG features that are apparent only under analysis; and (iii) a failure to appreciate the significance of *Fine Thoughts* (the things one *cannot* say in natural languages, even though it would be clear what they would mean) as critical evidence of UG, and of the difficulties presented by them for the kinds of "language-as-use" and related empiricist theories that he favors. Indeed, Evans also (iv) fails to address the issues of *competence* and *constraints* that are raised by Fine Thoughts and that are a central concern of UG; and (v) conflates UG with a computational theory of mind, a Fodorean conception of modules and a Pinkerean interest in the Sapir-Whorf hypothesis.

There has been a flurry of controversy surrounding Vyvyan Evans' (2014) *The Language Myth*, some of it reaching a level of rhetorical rancor undesirable on a scientific topic.[3] It is receiving a great deal of attention inside and outside the academy, from highly favorable reviews in the *Times Higher Education* and *New Scientist*,[4] to recent multiple-author peer commentary in *Language*, the journal of the Linguistic Society of America (e.g. Deen 2016; Wijnen 2016). Quite apart from extended exchanges in the blogosphere (into which we won't enter), it has also been subject to a published exchange between Behme and Evans (2015) and the generative linguist David Adger (2015b, 2015a). Although, as will emerge, we share many of the views of its critics, and think many correct points were made in the *Language* commentaries, we think that the serious deficiencies of Evans' book have not been made sufficiently clear. To be sure, the book was intended to be a popular one: but all the more reason to explain clearly how badly it misrepresents generative grammar and its present scientific status. Moreover, the exchange between Adger and Behme and Evans was focused too much on secondary verbal issues[5] to the exclusion of a number of substantive errors in the book that we think are worth setting out as succinctly and objectively as we can.

Evans' book is a polemic against the conception of language provided by Noam Chomsky's theory of generative grammar and popularised by Steven Pinker in his book *The Language Instinct* (1994), which Evans takes as his main target. We should stress from the outset that, though we are sympathetic to Chomsky's general program, neither of us is uncritical of many of the things Chomsky and Pinker say on its behalf. One of us (NA) works primarily in an area, linguistic pragmatics, that Chomsky doubts will issue in any deep insights into language and its use; the other (GR) has been highly critical of what he takes to be Chomsky's confused ideas about the intentional character of his theory, as well as many of his views about the philosophy of mind.[6] We certainly are under no illusion

that anyone yet has anything like the settled truth about the character of linguistic competence, performance or acquisition. We regard Chomsky's proposals, as we think he and many other linguists regard them, as part of an extremely promising and exciting *research program*, open still to considerable revision and re-conceptualization.[7] However, we think Evans' criticisms and the research that he discusses fall far short of making the case for his wholesale rejection of this research program as "*completely* wrong" (p. 4, *sic*).

A core claim for Chomsky is that human infants are born already equipped with a *universal grammar* (*UG*), disposing them to acquire a rich adult grammar on the basis of the apparently rather brief and limited linguistic exposure they receive from the utterances of those around them. It is important to note that Evans takes issue not only with this proposal, but also with a number of logically unrelated claims made by Pinker, many of which are in fact not shared by Chomsky and are not in the least essential to the generativist program: e.g., an endorsement of a general computational theory of mind and a Language of Thought independent of natural language, and a rejection of the Sapir-Whorf hypothesis. We address here mainly the argument about UG, with some comments on some of the other questions only briefly at the end.

The central problem in Evans' discussion is his complete disregard of Chomsky's substantial empirical arguments for UG. Evans takes Chomsky to be engaged in some kind of *a priori*, armchair project, akin to Hegel's purported speculations about the planets (pp. 19, 22, 59, 97, 132),[8] a project "not based on actual findings" (p.14), and for which there is "scant empirical evidence" (p. 21). We suspect that he and others may have been misled by the patent reliance of generativist theories of grammar upon intuitive verdicts of speakers about sentences such as the ones in (1)–(19) below, which can superficially seem like the reliance of traditional philosophers on intuitive "armchair" verdicts about non-mental domains, such as ethics or, perhaps, astronomy. But the role of the intuitions in the two cases is significantly different. Unlike traditional philosophers, generativists do not take intuitions to afford any special *insight* into any realm of fact; rather the intuitions are regarded as presumably spontaneous reactions of native speakers that often reflect facts about their linguistic competence, much as people's reports of how things *look* often reflect facts about the structure of their visual system. The intuitive reactions are not themselves theoretical claims; rather they are *evidence* for such claims, evidence that is not the less legitimate for its relatively easy accessibility. One could of course apply for a grant to test whether these sentences and countless other examples really elicit the responses that generativists claim, but the data are for the most part so immediately obvious and uncontestable, that it would be largely a waste of time and money to do so.[9] Either way, these intuitions would certainly seem to suffice as "concrete findings [that] support the Chomskyan framework" of the sort Behme and Evans (2015: 157) seek[10].

Evans also implies that Chomsky wrongly ignores many linguistic phenomena other than syntax. Now Chomsky may well be mistaken about a number of things (as both of us think he is), but it's certainly not because he doesn't realize language is used to communicate (p. 5), bears important relations to thought (p. 6–7) and can be used for endless social effects, such as performatives (e.g., declaring a couple married, pp. 7, 79). The key insight of what Chomsky calls the "Galilean style" (Chomsky 2002: 98–100) is that abstracting away from some of the complexity of real-world phenomena is absolutely essential to theorizing. Galileo ignored air resistance in order to state clearly the laws of motion, which would have been a mess otherwise. In doing so, he was not claiming that air resistance isn't real, nor that it can be ignored for all purposes. Similarly, when Chomsky argues that syntax can be studied in abstraction from language use – his famous distinction between competence and performance — he is not claiming that social and contextual factors are irrelevant to usage: quite the contrary, since if they were irrelevant it wouldn't be necessary to abstract away from them!

A distinct, legitimate criticism of Chomsky is that he is too quick to dismiss the chances of theoretical insight into these other aspects of language and its use. While he might turn out to be right about this, it is a logically separate point that he has no need to insist upon[11]. Once physicists had developed separate theories of motion and air resistance, they could use them in combination to understand more complex cases where air resistance is not negligible. Obviously, to the extent that generativist syntactic theory can be used in combination with pragmatic theory or sociolinguistics to explain complex phenomena of language use that will only further corroborate it.[12]

One of Evans' most egregious errors is the repeated suggestion that the only evidence adduced by generativists is from English (pp. 15, 68, 77, 93), or that they regard themselves as "absolved from studying other languages" (p. 68). Chomsky of course realizes that there are thousands of languages in the world, many of which don't display the *superficial* syntactic appearances of English, and neither he nor any generativist that we know of has ever claimed that "the study of a single language can reveal [the] design [of UG]" (p. 15).[13] It is obviously tendentious to say (as Evans does) that the UG proposal is that all languages are English-like (pp. 15, 64): one could say with just as much – or little – justification that they are all like Warlpiri or Korean.

In fact, the truth is just about the opposite to what Evans implies[14]: comparative syntax has long been at the heart of the generative approach, particularly since the mid-seventies when Richard Kayne and Luigi Rizzi applied Chomsky's ideas to French and Italian. Generativists have studied and written extensively on not only many of the European Germanic, Romance, Slavic and Celtic languages, but also on many languages of Africa (from Amharic to Xhosa), the Americas (from Athabascan to Zoque), and Australia; on Asian languages from several families; on Uralic and Austronesian languages, language isolates, and a number of sign languages of the deaf (which, indeed, sometimes come "out of the blue," [pp. 91, 129–131] — but, significantly, respecting principles of UG!)[15] This work led directly to one of the key developments in generative grammar, the "Principles and Parameters" approach which proposes that UG is a combination of fixed principles and variable parameters — e.g., whether verb precedes object or *vice versa* — which are set during language acquisition on the basis of what the child hears.

Of course, appreciating the cross-linguistic evidence, using it to triangulate on UG, and to show how, despite the diversity of *superficial appearances*, all these languages plausibly do respect UG, requires careful *analyses* of the expressions in these languages — just as appreciating the underlying unities in any domain requires analyses beyond surface appearances (as in biology, chemistry, physics).

Now one would have thought that Evans would consider some of Chomsky's arguments for what were, of course, originally and remain his extraordinary claims. But it's hard to find in Evans' pages any but the most perfunctory discussion of them or of the specific facts about language that lead Chomsky to his views.[16]

While generativists compile evidence from many languages, there certainly are countless relevant facts in English alone (and in any other language, as far as is known), and we draw attention here only to a very few striking ones that are critical for Evans' discussion. These are what Chomsky calls *Fine Thoughts*, or the surprising things one *can't* say, *even though it would be perfectly clear what a speaker would mean in uttering them* (Chomsky 2013: 41).[17] The best way to appreciate them is to pair them with closely analogous but innocuous sentences one *can* say, and then ask *why* you can say one but not the other. Here are some salient examples (standard generativist texts are crammed full of hundreds of others. Per usual, we will prefix the unacceptable examples with a star, and supply the standard labels for the different categories of example, indicating in parentheses the underlying issue they reveal):

I. Island constraints (One can't extract words from certain constituents, e.g., relative clauses in sentence subjects, preventing one asking about certain constituents of a sentence; see Müller [2011], and Sprouse and Hornstein [2013], for rich, recent discussions):

(1) **What do you wonder whether John bought?* vs. *I wondered whether John bought a car.*

(2) **What did you make the claim that John bought?* vs. *I made the claim that John bought a car.*

(3) **What do you worry that John buys another?* vs. *I worry if John buys another car.*

(4) **Who do you think pictures of pleased Bill?* vs. *Who do you think pictures of Sue pleased?* (cf. *Pictures of Sue pleased Bill.*)

II. Parasitic gaps (unpronounced elements, indicated by underscores, one of which depends on the other, preventing one from, e.g., answering certain questions without restoring a pronoun):

(5) **They played that song despite not liking __*
    vs. *Which song did they play __ despite not liking __?*

(6) **What did the attempt to repair __ ultimately damage the car?*
    vs. *What did the attempt to repair __ ultimately damage __?*

III. Binding phenomena (Each reflexive pronoun**,** e.g. *himself*, needs to stand in a certain structural relation of being *c-commanded* by an antecedent, which in languages like English must also be *local*; non-reflexive pronouns, e.g. *he*, must *not* have a locally c-commanding antecedent; indices indicate co-reference):

(7) **John$_i$ believes him$_i$ to be intelligent* (i.e. *him* can't refer to John);
    vs. *John believes he is intelligent* (where *he* can refer to John or someone else)

(8) **He$_i$ hopes John$_i$ will win* (i.e. *he* can't refer to John);
    vs. *John hopes he will win*. (where *he* can refer to John or someone else)

(9) **Bill$_i$ wants John to help himself$_i$*
    (i.e., *himself* can't refer to Bill; must refer to John)
    But: in *Bill seems to John to want to help himself*: *himself* must be Bill, not John.

In other languages there are similar (but not identical) facts. Consider two simple examples in Mandarin:

(11) **John$_i$ xihuan ta$_i$.* (i.e., *ta* cannot refer to John; fine if someone else)
     John like   him/her
      'John likes him'

(12) *John de mama xihuan taziji.* (*taziji* can only be John's mother, not John.)
     John 's mother like   him/herself.
     'John's mother likes herself'

The constraint here is like English — the reflexive must be c-commanded by the item that binds it — but unlike English reflexives, *taziji* is not marked for gender, so in (12) it can be understood as 'herself' and bound by *John de mama* ('John's mother').

IV. Constraints on contraction: (Can't contract at certain not easy to specify sites.)

(14) *Who do you want to dance with*?
     can be *contracted* to *Who do you wanna dance with?*

But:

(15) *Who do you want to dance?* can't be contracted to **Who do you wanna dance?*

(16) *You have a book in the car* can be contracted to *You've a book in the car.*

But:

(17) *I wouldn't let you have my book in the car*
     can't become: **I wouldn't let you've my book in the car.*

V. (Negative) polarity problems (Every *negative polarity item*, such as *any* or *ever*, must be c-commanded by a certain sort of "affective" constituent, e.g., "negation" and "factive"):

(18) *Did you see anyone?* cannot be answered by **Yes, I saw anyone.*

(19) **A man who has a dog is ever happy.* vs. *A man who has a dog is never happy.*

There are countless other examples, in English and all the other languages generative syntacticians have studied. Again, the deep puzzles they raise are (i) why the starred expressions seldom, if ever, even get uttered, since actual utterances of most them could easily be understood, and indeed would be much simpler than the grammatical forms by which speakers seem unconsciously constrained (why reply to "Did you see anyone?" with "I saw someone," when "I saw anyone" should more directly suffice?); and then (ii) how on earth kids come to learn such peculiar facts without significant effort or specific instruction.

This short article is not the place to go into the (considerable) detail of generativist accounts of these phenomena[18]. We just want to point out the structure of the argument, namely *inference to the best explanation*: all of the above phenomena are so peculiar, so contrary to reasonable assumptions of use, and so effortlessly mastered by children without explicit instruction in a remarkably short period of time, that it's hard to think of any explanation other than that the children were somehow innately endowed with a system that respects the relevant constraints.[19] Other things equal, the theory that provides the best explanations is the one that we have the most reason to believe. To argue against generativist syntax, then, it is necessary to show that it does not explain *critical* data like the above (data predicted by UG and not its rivals), or that there is a better explanation available.[20] [21] Given how easy it would be to use the ungrammatical forms, it seems extremely unlikely that a theory based merely in language use (Evans' view: see below) could begin to explain them. However much generativist theories ignore what they regard as noisy performance data, Fine Thoughts are critical data for preferring them to use theories, and the burden is on a use theorist to indicate how they would explain them.

It is important to see that far from being "immune to counterevidence" (p. 159) as Evans repeatedly claims (pp. 64, 78–9, 159–60) — a claim in tension with his claim that it is "*completely*

wrong" (pp. 4, and cf. p. 69) — specific claims of UG could be (and sometimes are) easily refuted if enough of these acceptability judgments were to prove thoroughly unstable, or if an alternative theory were proposed that, by the usual standards of scientific explanation, accounted for these and other data more successfully.[22]

What are the prospects of the alternatives Evans proposes? He mentions a number of "learning abilities not foreseen by Chomsky" (p. 106), e.g., "general learning mechanisms" (p. 3), "pivot schemas" (p. 113), a "pattern-finding ability," which performs " 'statistical' analyses" (p. 119) and "cultural" (p. 120) and "interactional intelligence" (p. 235). The claim is that these suffice: no learning mechanisms specific to language acquisition are required.

Evans pairs this familiar (roughly) empiricist claim with the idea that what is acquired is a type of "construction" grammar and with the functionalist notion that words, idioms and grammatical constructions persist in the language to the extent that they serve as tools for communication and other social purposes (pp. 82–88 and chapter 8), a conception he calls, generally, "language-as-use."[23] Developing his suggestion of "frequency and repetition" (p. 125), he turns to proposals of Ronald Langacker regarding "form-meaning units," which, according to Evans, are

> formed by the infant generalizing across patterns of language use. For example, a speaker acquiring English will, as the result of frequent exposure, discover recurring words, phrases and sentences in the utterances he or she hears, together with the range of meanings associated with those units. With a lot of exposure, the child begins to abstract across different instances of use in order to draw out the common elements of the utterance[...] How frequently a child hears a particular expression in fact determines how well established the expression come to be in the child's developing mental grammar. (p.124–125; see also pp. 131–132).

As an example of this frequency effect he cites work that shows that the word *"lie* is [...] more firmly ingrained in our mental grammar than *falsehood"* (p. 125), and later on discusses how a child might generalize many expressions to the form "x causes y to receive z" (pp. 245–247).

Surprisingly, Evans seems totally oblivious of the fact that such "frequency" views are a species of *precisely* the kind of theory Chomsky famously devastated in his celebrated (1959) review of Skinner's *Verbal Behavior*. Although these newer approaches are far more sophisticated than Skinner's, they suffer from essentially the same problem that Chomsky raised against that approach. Like Skinner, Evans takes linguistics to be concerned with frequency features of speech that is *actually produced*, e.g., the child's exchanges with adults (e.g. pp. 116–117). But, as Chomsky emphasized in that review and in virtually all of his writings since, he takes linguistics to be concerned not with a speaker's *performance*, but with her underlying *competence*, which manifestly goes far beyond frequency features of interactions with care-givers. Moreover, appeals to such unconstrained processes as "analogy," "abstraction," "generalization," "pattern matching" are as vacuous in Evans' proposals as they were in Skinner's (to take a famous example of Chomsky's: how come the child doesn't generalize the syntactic behavior of "Bill is easy to please" to "Bill is eager to please," which certainly sounds "analogous," and to have a "pattern" that could be "matched" and "abstracted" for the purpose?)

But the most telling inadequacy of Evans' suggestions is that they nowhere even hint of how any of the oddities exhibited, e.g., by Fine Thoughts might be learned by any of the methods he mentions.[24] Although all of those methods may have a place in *creating* constructions, Evans doesn't notice the problem of how a child could learn how to *constrain* them. Again, the striking feature raised by the Fine Thought sentences is that they seem to be constructions that are perfectly

"analogous" to constructions a child may have heard, and, moreover we know perfectly well what would be meant by them; but English speakers rarely if ever in fact produce them, and find them unacceptable if asked.

The only explanation Evans seems to offer of why we know such constructions are unacceptable is provided in a single paragraph: "children don't produce outlandish grammatical constructions when learning to form questions [...]. All this shows is that children don't produce the interrogatives that don't exist in their language!" (pp. 103–104 and cf. p. 105). But this is not an answer to the question, but simply a statement of the problem: how in the world do they come to know that certain constructions "don't exist," indeed, are not allowed in their language?[25]

Like many, Evans believes that absence of evidence is evidence of absence (pp. 105, 126–128). But if this is taken as a claim about learning about an (un)acceptable grammar, it is manifestly not true. *There is a virtual infinity of perfectly grammatical sentences that will never appear in any actual corpus for any number of reasons other than being ungrammatical*: for example, pointless but perfectly grammatical sentences (a million conjunctions of "Cats meow"), or semantically or factually jejune or anomalous ones ("Colorless green ideas sleep furiously," "Kangaroos divided by encyclopedias will conquer fastidiousness"). How is any learner to distinguish non-occurrences due to ungrammaticality from those due to silliness or just style of speech (people don't talk any more like Henry James, if they ever did)? Pending a specification of how a "language-as-use" theory of the sort he has sketched could begin to account for Fine Thoughts, and how a child could acquire knowledge of the constraints they manifest, Evans has not begun even to suggest a serious rival theory of language or its acquisition.[26]

Evans also falls prey to a widespread misunderstanding about "linguistic universals". Drawing on work such as N. Evans and Levinson (2009) and Everett (2005)[27], Evans shares their conflation of UG with claims about *typological* universals (p. 70): putative features observed in all of the world's languages.[28] Like (N.) Evans and Levinson, (V.) Evans claims that there are no typological universals, and therefore that "the emerging facts of linguistic diversity have demonstrated that the Universal Grammar proposal is just plain wrong" (p. 69). He discusses some of their examples at length, unfortunately taking at face value their presentation of their data, which has been seriously challenged: e.g., he repeats Evans and Levinson's claim about Arrernte syllable structure (p. 71) ignoring Nevins' (2009) critique. And he reports Everett's claim that the Pirahã language lacks recursion without referring to work that casts doubt on that analysis of the data.[29]

But all of this is irrelevant to Evans' argument that there is no UG, because this argument is founded on a logical mistake, as Fitch et al. (and several others[30]) have pointed out:

> Modern linguistics asks questions about the biological capacity to acquire human language, a set that includes but is not limited to the huge variety that currently exists on our planet. The putative absence of obvious recursion in one of these languages is no more relevant to the human ability to master recursion than the existence of three-vowel languages calls into doubt the human ability to master a five- or ten-vowel language. (Fitch et al. 2005: 203)

That is, "our language faculty provides us with a toolkit for building languages, but not all languages use all the tools," (Fitch et al. 2005: 204), any more than all humans develop their innate capacity to swim.

Evans is aware of this point, but acknowledges it (p. 78) only after pages spent in discussion of typological universals (p. 64–78) and doesn't see that it vitiates his criticism. In his response he

makes another mistake (pre-emptively dealt with by Fitch et al. 2005): this view of UG as a toolkit is unfalsifiable, he claims, because it doesn't predict that there are typological universals (p. 78). But the UG proposal — that there is a species-wide, innate, domain-specific capacity for language — makes a number of other predictions, including:

> (i) that any developmentally normal infant can acquire any language — e.g., an adopted Pirahã child would acquire English or any other uncontroversially recursive language just the same as any other child;

> (ii) that language abilities can be selectively impaired (Evans even discusses this prediction [p. 144]);

> (iii) that no child can acquire an artificially designed "language" that contravenes UG — at least not using the language faculty rather than as an intellectual exercise;[31] and

> (iv) that we should expect there to be a *critical period* for language acquisition.

Should *none* of these predictions or any of the above unacceptability judgments hold up, then generativist theories would certainly be in trouble. But neither Evans nor anyone else has yet shown that they don't. For all the still outstanding challenges certainly facing generativist (as any other) theories, they do seem to be well-supported by the data that they uniquely explain.

There are numerous further problems with Evans' discussion, most of which have not been discussed in the various reviews and commentaries, which it's best simply to list:

> (1) It is simply not true that generativists think there is no such thing as learning (p. 99), much less have a "phobia" about it (p. 153): the original "Aspects" model of acquisition (Chomsky, 1965: 30) was essentially a theory-confirmation model, and some more recent generativist models rely heavily on statistical and probabilistic estimates.[32] What exactly is learned is obviously an empirical question, but it surely includes which *specific* phonological forms and grammars (e.g., parameter settings) are in use in the speaker's immediate environment, which may well need to be determined statistically; as well, of course, as which strings of speech sounds stand for which concepts;

> (2) Evans (pp. 24, 28, 49) seems not to have understood recent discussions by Chomsky and various biologists (e.g., Fitch et al. 2005) of how "Merge" may offer a reply to the "Darwin Problem" of how the language faculty might have evolved (see Hornstein, 2013: 399–405, for useful discussion). He thinks that they claim that human language "is unrelated to any other form of animal communication" (p. 28). In fact, while Fitch et al. agree with Evans that it "exhibits a degree of sophistication that far exceeds any other form of animal communication" (p. 28), the whole point of their paper is to propose a small change that could have given rise to this spectacular ability by connecting up in a new way the pre-existing physical and cognitive equipment, much of it no doubt shared with other species;

> (3) Pinker (1994) discusses the Sapir-Whorf hypothesis, which Evans glosses as: "the distinctive nature of languages does influence the habitual nature of how speakers of different languages think" (p. 194). But, *pace* Evans' (ch. 7), this is completely independent of anything having to do with UG. Actually, given the weak notion of "influence," it's hard to see how this weak form of the hypothesis could fail to be true. Is it really surprising that differences in grammatical gender give rise to differences in the mental lives of Spaniards and Germans (pp. 218–219)? What would be astonishing is if they *didn't*! And no doubt interior

designers find that having names for many shades of color makes it easier to think and talk about them. Pinker's real target is a very strong form of the hypothesis according to which what thoughts one is *capable* of having is determined by what language(s) one speaks. This isn't remotely established by any of the studies Evans cites, as he seems to be aware (pp. 197–8). So what is the disagreement supposed to be, and why should the reader care?

(4) Despite common impressions to the contrary, Fodor's and Chomsky's notions of module are very different, and Evans' discussion is irrelevant to Chomsky's views here, since it only targets Fodor's notion: a unit that processes specific kinds of input in a restricted way in isolation from general intelligence ("informational encapsulation"), on an analogy with visual processing. For Chomsky, to postulate a module or mental organ (his preferred term) is to make a claim about the kinds of structures and properties that characterise an underlying competence, largely in abstraction from issues of perception and production. A mental organ needn't be physically distinct from other mental organs, and, unlike a Fodorian module, its use in processing need not be informationally encapsulated (see Berent 2016, for discussion). In fact, Chomsky has always argued that use of language (performance) draws on linguistic competence, but on much more besides. Generativist syntactic theory claims that our parsing of sentences is sensitive to constituent structure and c-command, but leaves how this sensitivity is implemented as a separate question for those working on the parser.

What's more, Evans' critique of Fodor-modules completely misses the mark. Modularity theses are not committed to one module being completely impervious to the actions of other processes (pp. 17, 134), but only to certain kinds of *information transfer*. Indeed, the McGurk effect, which Evans claims as a counterexample to Fodorian modularity, (p. 152) is actually an excellent example of it, specifically of the phonological system's encapsulation in relation to central knowledge, since the effect doesn't disappear in scientists thoroughly familiar with the "illusion."

(5) Whether the mind is to be understood as a computer is also an issue largely independent of UG. *Pace* Evans (pp. 17–18, 162, 168), both Chomsky (2000) and Fodor (2000) express serious scepticism about the claim. At best, certain *core perceptual*, *syntactic* and *phonological* procedures are to be understood computationally, but the character of processing *beyond them* in the mind as a whole remains for them largely an open question;

(6) As many have pointed out, computational models of an intelligent process break it down to smaller and smaller intelligent steps, which finally can become purely mechanical, often binary "yes"/"no" responses (see Fodor 1968; Dennett 1975). *Pace* Evans (pp. 171–172), these last do not require any intelligence whatsoever for their execution: mere binary causal responses of an electric circuit or the like will suffice;

(7) Evans virtually contradicts himself when he claims (p. 185, in contiguous sentences) both that Mentalese "symbols get locked to states of affairs in the world" *and* that "concepts in Mentalese bear no relation to the experiences and states they are locked to," and the Mentalese account of thought has "a hard time accounting for how concepts are grounded": *locking* (or other sorts of covariation of a symbol with worldly phenomena) just *is a relation* to the world, and a way of grounding its meaning![33]

There are more errors, but the above should suffice to establish that Evans' book does not constitute a serious discussion of Chomsky's and related proposals and the arguments for them, much less any sort of refutation of them. The surprisingly widespread view defended by Evans that Chomsky has been proved wrong is a serious misrepresentation of the state of the art in linguistics to the casual

reader (and some not so casual ones, see footnote 4 above).

All of this is a pity. While we think Chomsky's research program is deep and important, and that Pinker's *Language Instinct* was an excellent popular summary of it in its day, neither has nearly got everything right, and certainly much has been learned in the twenty years since the publication of Pinker's book. Evans could also have written an interesting, popular book, arguing carefully that Chomsky or Pinker went too far in certain claims, or that particularly Pinker gave the impression that things are resolved where the jury was still out, or which subsequent research has re-opened. There certainly are plenty of important outstanding questions. To take a few: What is known and what is still to be discovered about the roles of general learning and innate, domain-specific abilities in language acquisition? Are all the above Island and Binding phenomena due to grammar, or are some due to features of memory and processing? To what extent is syntax, for example the Negative Polarity phenomenon, partly driven by semantics or even pragmatics? How does an infant determine which ambient utterances are relevant to fixing the grammar? Does biological evidence support Chomsky and colleagues' highly speculative proposals about the evolution of the language faculty (Fitch et al. 2005; Hauser et al. 2014)? There is a good popular book to be written in this area, but this isn't it.[34]

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3 While we agree with criticisms of Evans' tone (Ackerman & Malouf 2016: 190; Deen 2016: 198, 200; Goldberg 2016: 201; Hinzen 2016: 203; Wijnen 2016: 207) and, more seriously, his tendency to misdescribe or even caricature (whether intentionally or otherwise) his opponents' positions (Ackerman & Malouf 2016: 190–1; Adger 2015a: 77, 2015b: 161–2; Deen 2016: 198; Goldberg 2016: 201), we confine ourselves here to the content of Evans' claims.

4 See Aldridge (2014) and Anderson (2014).

5 In particular, both reply and response dwell too much on precisely what Chomsky has said on particular occasions, and specifically on how to define "recursion" and "instinct." But, as we will argue, the central question that Evans is concerned with, the existence of UG, does not turn on specific ways Chomsky has phrased most of the issues, and certainly not on sharp definitions of either of those terms. The particular form of recursion that theories of syntax need is an internal technical question (see the passages Adger cites) that doesn't affect the theoretical approach as a whole; and appeals to "instinct" can everywhere be replaced by the postulation of substantive domain-specific innate constraints on language acquisition.

6 See Rey (2003), presently being expanded into (ms.).

7 The avowedly programmatic character of much of Chomsky's recent work raises interesting questions about the use of bold hypotheses in science. We can only touch on the details of that larger debate here.

8 Actually, Evans falls prey here to a common misreading of Hegel; see Beaumont (1954).

9 For doubters, though, this has in fact been done, with a correspondence of roughly 95% between the linguists' and the subjects' responses; see, e.g. Maynes and Gross (2013: 722–724), and Sprouse et al. (2013). Which is not to say that worries can't be raised about specific data, as well as about the interesting effects of "satiation" by frequent repetition of examples (see Hofmeister et al. 2013). As in any science, (analyses of) data are as much up for careful discussion as is theory.

10 More specifically, Behme and Evans call for "concrete findings from developmental psychology and neurobiology". It's hard to see why the search for evidence should be restricted to only these two fields (if that is indeed the intended suggestion). In science (and rational enquiry more generally) one takes relevant evidence wherever one can find it.

11 Many other generativists are more ecumenical than Chomsky: e.g. Adger (2015a: 160).

12 Such work is indeed being done. See e.g. Adger and Smith (2005) and other papers in Cornips and Corrigan (2005); the papers in Åfarli and Mæhlum (2013); and Cornips (2014).

13 The quotation that Evans supplies in support of this (mis)reading of Chomsky is crucially taken out of context: see Adger (2015a: 161), commenting on the dialogue on this issue between Evans and David Pesetsky at:
https://www.facebook.com/vyv.evans/posts/545113908959149?comment_id=545115972292276&offset=150&total_comments=160

14 As Deen writes, "the suggestion that nativists are shielded from cross-linguistic facts is breathtaking in its misrepresentation" (Deen 2016: 199). Of course this is not to deny that a handful of languages, including English, some other modern European languages, and Japanese have received a lot of research attention (from generativists and others) for various institutional reasons, and that many other languages are (relatively or absolutely) understudied.

15 A list of generativist work on languages other than English would run to thousands of references. For overviews, see Roberts (1997), Baker (2001) and Cinque and Kayne (2005). Chomsky frequently draws on this comparative work in his own research: one well-known example is Chomsky (1981) which introduced the pro-drop parameter; his (1988) discusses hundreds of Spanish examples. The very first generativist work was his 1951 master's thesis (Chomsky 1979) – on Hebrew! We provide some Mandarin examples below. For the spontaneous emergence of a sign language after the Nicaraguan civil war see Kegl et al. (1999). On universals and sign languages, see Sandler (2010). *Pace* (pp. 74–5), "scrambling" languages" are also not news: see the papers in Karimi (2003).

16 Evans' book also lacks discussion of the wealth of empirical generativist work on language acquisition. Deen (2016: 198) finds just three such references in Evans' chapter "Is language innate?", none more recent than 1987, against about eighty references to language-as-use work on acquisition. See also Berent (2016: 196).

17 "Fine thoughts" is perhaps not the most pelucid phrase for the purpose. One of us (GR) prefers to call them "WhyNots?" –for "Why not *s, given you can say s′?"

18 One can get the flavor of such explanations from any standard textbook on generative syntax, such as Adger (2003); Haegeman (1994); or Radford (2009).

19 As many have observed, the term "innate" is not a happy one, being notoriously difficult to characterize across biological domains. For Chomsky's purposes, it suffices to rely on a notion relatively specific to cognitive abilities, that of a species-wide "*initial state*" of a system that develops into fairly *stable states* as a result of input to the system. See Gross and Rey (2012) for discussion.

20 Note that the situation has not been changed by the advent of the Minimalist Program (Chomsky 1995), which takes for granted such phenomena as those in I–V, and their approximate theoretical characterisation by earlier proposed rules, — e.g. that anaphors are locally c-commanded by their antecedents. What this research program attempts to do is to derive these rules from basic principles of biophysics and computation plus a few (i.e. 'minimal') assumptions about how lexical items are combined, and how the grammar system is connected to other mental systems (see Hornstein 2013: 399–400). Hornstein aside, generativists have not made this point as clearly as they should have, and there has been a great deal of confusion on this point from non-generativists (but not in Evans' book, which does not discuss Minimalism).

21 Most of these data have been ignored by anti-generativists. An exception is Goldberg (2006), which attempts a non-generativist 'construction grammar'/functionalist account of island constraints. This is — at best — a work in progress which has not yet managed to reproduce the coverage of the data of standard generativist accounts (see Lidz & Williams [2009: 184]; and other critical work cited there).

22 There is actually continual, and often substantial, revision of specific UG proposals in response to new evidence; and there are serious rivals to the derivational approaches favoured by Chomsky and many generativists, such as the representational/declarative approaches exemplified by HPSG (see Sag et al. [2003]), but these are still committed to there being some innate domain specific mechanism (UG in a broad sense).

23 For the record, "language-as-use" views are hardly "new" (p. 132): they have been influential in philosophy, psychology and linguistics at least since Wittgenstein's (1953: §43) famous enunciation of them. And despite the impression Evans gives, "language-as-use" is hardly an underdog to some 'Chomskyan mainstream', since there has never been a time when generativists have been the majority among linguists.

24 The role supposed to be played by 'cultural intelligence' is particularly unclear, as Wijner (2016: 208) points out.

25 As Hinzen (2016: 205) points out, "there is nothing 'outlandish' about [such sentences] if we subtract the grammatical knowledge that E[vans] wants to get rid of."

26 Note that Prinz's (2002: 210) view that Evans cites (p. 104), that "A failed prediction could be used as evidence that the rule underlying the prediction was wrong" does not itself present a serious alternative to UG: since a toddler can't test *all* hypotheses, she must restrict herself to the ones with high prior probabilities, and, for anything that's been said so far, those might well be the very ones compatible with the very UG grammars that nativists posit.

27 For more detailed discussion of this work, see Smith and Allott (2016: 188–195). The conflation has worked its way to philosophy: citing Everett's work, Pat Churchland recently writes: "Linguistic universals, long the darlings of theorists, took a drubbing as one by one they fell to the disconfirming data of field linguists" (2013: xiii).

28 On the distinction between UG universals and typological universals see Hornstein (2013: 393, fn. 3) and Adger (2015b: 77). As Adger points out, the typological universals that have been proposed are generally about surface features of languages, such as word order, while UG universals concern underlying (tree-)structure. The fundamental difference, though, is that UG universals are properties of the *language faculty*, *the innate capacity to acquire a language*, while typological features are properties of particular external languages that are (or could be) spoken.

29 For example, Nevins et al. (2009).

30 Including Hornstein (2013: 393, fn. 3); Adger (2015b: 77).

31 There is some experimental and neurolinguistic confirmation of this prediction. See Smith and Tsimpli (1995), Musso et al. (2003); and Chomsky (2015: 98–99).

32 See, e.g., Yang (2002), Lidz and Gagliardi (2015) and Deen (2016: 199).

33 A point also missed in the notorious "Chinese Room" argument on which Evans seems to rely (pp. 169–170). See Rey (2002) for discussion of the many problems with that argument.

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