

Linguistic Intuitions Revisited

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ABSTRACT

Why are linguistic intuitions good evidence for a grammar? In ‘Intuitions in Linguistics’ ([2006a]) and *Ignorance of Language* ([2006b]), I looked critically at some Chomskian answers and proposed another one. In this article, I respond to Fitzgerald’s ‘Linguistic Intuitions’ ([2010]), a sweeping critique of my position, and to Culbertson and Gross’ ‘Are Linguists Better Subjects?’ ([2009]), a criticism of one consequence of the position. In rejecting these criticisms, I emphasize that the issue over linguistic intuitions concerns only metalinguistic ones. And I argue that my critics, like many others, make too much of the distinction between grammaticality and acceptability intuitions.

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Everyone agrees that a competent speaker/hearer’s intuitive judgments about her language can be a good evidence for a grammar of that language. But why would they be? What view of the source of these intuitions would make them good evidence? In ‘Intuitions in Linguistics’ ([2006a]) and *Ignorance of Language* ([2006b], Chapter 7), I looked critically at some Chomskian answers to these questions and proposed some other ones.

Section 1 of the present article starts with a summary of this discussion to serve as background to what follows. The rest of the section contains two developments of those earlier views. The first says a lot more about which linguistic intuitions are at issue and why they are interesting. The second says a lot more about the alleged distinction between acceptability and grammaticality intuitions. I think that these developments are important to my disagreement with the Chomskians. They have been stimulated by two interesting critical papers: ‘Linguistic Intuitions’ ([2010]) by Gareth Fitzgerald; and ‘Are Linguists Better Subjects?’ ([2009]) by Jennifer Culbertson and Steven Gross.¹ The developments include some preliminary responses to these criticisms. But my responses in earnest to Fitzgerald are in Section 2 and to Culbertson and Gross in Section 3.

1 Summary and Developments

1.1 Summary

In this section, I shall summarize my views on linguistic intuitions by answering three questions.

(A) What is the Chomskian view of these intuitive judgments? In answering we need to attribute a view that, were it true, would justify the linguists’ use of these intuitions as evidence for a grammar. My answer is that, according to the Chomskians, linguistic competence, all on its own,

provides information about the linguistic facts [...] So these judgments are not arrived at by the sort of empirical investigation that judgments about the world usually require. Rather, a speaker has a privileged access to facts about the language, facts captured by the intuitions, simply in virtue of being competent [...]. ([2006a], pp. 483–4, [2006b], p. 96)

On this view, the intuitive judgments are, as I like to put it, ‘the voice of competence’. So let us call the view ‘VoC’. According to VoC, competence provides *informational content* to intuitions. However, other parts of the mind are involved and so there can be ‘noise’. So intuitions are often not the *pure* voice of competence. Still, to the extent that the intuitions are the product of competence they must clearly be good evidence for a grammar.

I identify two versions of VoC. According to the ‘standard’ version, speakers derive their intuitive judgments from their representations of linguistic rules (and principles) by a causal and rational process like a deduction. I cite a lot of evidence in support of this interpretation of VoC ([2006a],

¹ All references to Fitzgerald are to his ([2010]); all references to Culbertson and Gross, to their ([2009]). Other papers critical of my view of linguistic intuitions are: (Collins [2006], [2007], [2008a]; Rattan [2006]; Smith [2006]; Mišćević [2006]; Pietroski [2008]). I have responded: ([2006c], pp. 590–7 and [2008c], pp. 680–1).

pp. 483–4, [2006b], pp. 96–7). Still, I accept that it may not be the right interpretation. If it is not, Chomskians must have a ‘nonstandard’ version of VoC: the intuitions must be provided somehow by embodied but *un*represented rules ([2006a], pp. 482–6, [2006b], pp. 96–8).

(B) Is VoC true? I argue that it is not. Instead, I urge the following theory of intuitive linguistic judgments, based on a theory of intuitive judgments in general:

Linguistic intuitions do not reflect information supplied by represented, or even unrepresented, rules in the language faculty. Rather, they are empirical central-processor responses to linguistic phenomena differing from other such responses only in being fairly immediate and unreflective. ([2006b], p. 10)²

Like all other such responses these intuitive judgments are theory-laden.³ On this modest view—modest because it makes do with cognitive states and processes we were already committed to—the importance of competence to intuitions is not as *a source of information* but as *a source of data*.⁴ A person’s competence provides evidence of what she would say and understand. Her central processor provides intuitive opinions by reflecting on such data. Thus, an ordinary person asked about a certain string of words may first simulate the behavior of attempting to produce or understand the string, thus exercising her linguistic competence. She will then go in for some quick central-processor reflection upon this experience, deploying her concept of *grammaticality*, *ambiguity*, or whatever from folk linguistics, to form a judgment.⁵ Her simple judgments, at least, are likely to be right and so it is appropriate for linguists to use them as evidence for a grammar. However, it follows from my theory-laden view of intuitions that the intuitions that should be most relied on are those of the linguists themselves because their intuitions are laden with the best theories ([2006a], pp. 496–502, [2006b], pp. 108–14, [2006c], pp. 594–5).

Why should this modest theory be preferred to VoC? (i) The standard version of VoC is committed to speakers’ representing linguistic rules. I argue at length that this commitment is unsupported and implausible ([2006b], pp. 87–121, 195–272). And, in the context of discussing linguistic intuitions, I offer several reasons why my theory should be preferred to the standard

² This is the first part of the book’s ‘third major conclusion’ ([2006b], p. 120).

³ This is not to say that they are the result of *theorizing* or should count as *theoretical* (cf. Mišćević [2006], p. 539; Devitt [2006c], p. 595). They are observational, if any judgments are, but are theory-laden in the way we have learnt all observational judgments about the world are.

⁴ The sense of ‘data’ here is the same as in ‘primary linguistic data’. So the data provided by competence are *linguistic expressions* (and the experiences of using them) *not any observational reports* about those expressions.

⁵ This simulation is not necessary in all cases; thus, word salads are likely to be immediately recognized as such in the same way that a person immediately recognizes many features of the world. I develop this point more in my response ([forthcoming]) to (Textor [2009]).

version ([2006a], pp. 488–91, 502–5, [2006b], pp. 100–3, 114–7). (ii) What about the nonstandard version? Many of the just-mentioned reasons would count against the nonstandard version too. But the main problem with the nonstandard version is that we do not have *any idea* how embodied but unrepresented rules might provide linguistic intuitions ([2006a], pp. 506–7, [2006b], p. 118, [2006c], p. 593).

An important background to this disagreement over intuitions is my rejection of the received Chomskian ‘psychological conception’ of a grammar in favor of a ‘linguistic conception’ ([2006b], Chapter 2; see also [2003] and Devitt and Sterelny [1989]).⁶ According to the psychological conception, a grammar is about a cognitive system and linguistics is part of psychology. According to the linguistic conception, a grammar is about a nonpsychological realm of linguistic expressions, physical entities forming symbolic or representational systems.

(C) How important are the intuitive judgments of ordinary speakers as evidence for a grammar? The received Chomskian view is that they are very important ([2006a], pp. 481–3, [2006b], pp. 95–6): they are thought to provide the main evidence, often near enough the only evidence. I argue that this exaggerates the importance of these intuitions⁷ and conclude that the main evidence for grammars is to be found ‘in a combination of the corpus, the evidence of what we would say and understand, and the intuitions of linguists’ ([2006a], p. 488, [2006b], p. 100).

The criticisms of Fitzgerald and Culbertson and Gross make it clear that I need to develop my view in two ways to sharpen my disagreement with Chomskians. First, in Section 1.2, I shall make clearer precisely what ‘intuitions’ are the subjects of discussion. This is very important to my discussion of Fitzgerald in Section 2. Second, in Section 1.3, I shall consider the significance of the alleged distinction between intuitions about grammaticality and intuitions about acceptability. Both Fitzgerald and Culbertson and Gross make much of this distinction. And it is very important to my discussion of Culbertson and Gross in Section 3.

1.2 ‘Intuitions’ in what sense?

Talk of ‘linguistic intuitions’ is vague. I am explicit that I am using it to apply only to intuitive *metalinguistic judgments*, judgments that expressions have certain properties; for example, being acceptable, being ambiguous, or being

⁶ This rejection has received a deal of criticism (some of it very harsh): (Antony [2008]; Collins [2007], [2008a], [2008b]; Dwyer and Pietroski [1996]; Laurence [2003]; Longworth [2009]; Matthews [2006]; Pietroski [2008]; Rattan [2006]; Rey [2006], [2008]; Slezak [2009]; Smith [2006]). (Devitt [2006c], [2008a], [2008b], [2008c], [2009]) are recent responses to some of these criticisms.

⁷ This is the second part of the book’s ‘third major conclusion’ ([2006b], p. 120).

coreferential. So I am applying it to judgments deploying *metalinguistic concepts* ([2006a], p. 482, [2006b], p. 95). And, for convenience, I also apply it to the linguistic expressions of these judgments. Now, of course, any such expression is itself an instance of linguistic usage. But, in the interests of emphasizing an important distinction, I am not applying ‘linguistic intuitions’ to vastly more numerous *other* instances of linguistic usage, the ordinary production (or not) of expressions to describe, query, etc. features of the nonlinguistic world, and the ordinary understanding (or not) of such expressions. These speedy automatic behaviors can, I emphasize, quite properly be called ‘intuitive’ and clearly provide evidence for a grammar ([2006a], pp. 482, 487; [2006b], pp. 95, 99). However, the behaviors are not *judgments about* language and so are not, in my view, properly called ‘linguistic intuitions’. In any case, I am not calling them that. Rather, I shall call them ‘*other* linguistic usage’. They are not the subject of my theses about linguistic intuitions. It is important to note this if we are to avoid merely verbal disagreements.

Suppose that we are constructing a grammar and become interested in the following strings:

- (1) Who do you wanna kiss you this time?
- (2) Visiting relatives can be boring.
- (3) Tom thinks that Dick loves himself.

We want to know whether (1) is grammatical, whether (2) is syntactically ambiguous, and whether in (3) ‘himself’ must corefer with ‘Dick’. We might simply ask some competent speakers about all this, using words that are as untechnical as possible. The responses to these questions would be examples of what I am calling ‘linguistic intuitions’. That is a very easy way to get evidence. But there are, of course, many ways involving *other* linguistic usage. Thus, we might look to the corpus: concerning (1), we look to see if ‘wanna’ is used this way; concerning (2), we look at the behavior and circumstances in which sentences like this are used for signs of the two interpretations; concerning (3), we look to the behavior and circumstances in which sentences like this are used for signs of what ‘himself’ refers to. Or we can conduct experiments using ‘the technique of elicited production’ to see what the subjects say or understand in appropriate circumstances (Thornton [1995], p. 140). Or we might simply describe some appropriate circumstances and ask speakers what they would say or understand. What we would observe in all these cases are instances of *other* linguistic usage. These instances can provide very good evidence but none of them involves what I am calling ‘linguistic intuitions’: they involve uses of language but not metalinguistic judgments about it.

Linguistic intuitions like ‘(1) is acceptable’ and ‘“Himself” must refer to Dick’ are, of course, themselves instances of linguistic usage and so could provide evidence in just the same way as instances of *other* linguistic usage do. But, in that way, ‘(1) is acceptable’ provides the same rather uninteresting linguistic evidence as any expression that is syntactically similar; for example, ‘Nana is lovable’. So, in that way, it does not provide any evidence at all *about* (1). Similarly, in that way, ‘“Himself” must refer to Dick’ provides the same rather uninteresting evidence as the syntactically similar ‘Caesar must march to Rome’ but not evidence *about* (3). In contrast, the evidence these utterances provide *qua* linguistic intuitions *is* evidence about (1) and (3), respectively. What makes these intuitions interesting to linguists as evidence is *what they say about* (1) and (3) not what syntactic structures they exemplify. So the interest arises from *their particular informational contents*: they name an expression and apply a metalinguistic concept to it.⁸ And what we want is a theory that explains why intuitions *with these contents* are evidentially interesting.⁹

The distinction is vital in assessing my dispute with Chomskians. It is trivial that a speaker’s competence (along with some other factors) is causally responsible for intuitive *other* linguistic usage: that is what makes the competence the competence it is. What VoC claims, and I deny in my answer to question (B), is that the competence is responsible for the information in intuitive metalinguistic judgments.

This distinction is also important in assessing my answer to question (C). That answer downplays the evidential role of speakers’ intuitions, claiming that they are ‘not the main evidence for grammars’ ([2006b], p. 120). What I am downplaying is the evidential role of intuitive metalinguistic judgments. I am certainly not downplaying the role of intuitive *other* linguistic usage. Indeed, I *emphasize* the evidential role of these behaviors ([2006a], pp. 486–8; [2006b], pp. 98–100).

Philosophers have objected, with surprising frequency, that I have not gone nearly far enough in downplaying the role of metalinguistic intuitions: they think that these intuitions really have *no* evidential role in linguistics. Fitzgerald shows some signs of this view ([2010], pp. 136–7). John Collins (in the guise of ‘Ling’) shows it unequivocally, talking dismissingly of ‘the absurd idea that we are after speaker/hearers’ explicit propositional judgments on the linguistic status of strings’ ([2006], p. 480). If this were right then my discussion of these intuitions would be misguided. But it is about

⁸ ‘Intuitions, *with the contents that they have*, are the data of Linguistics’ (Fiengo [2003], p. 253; emphasis added).

⁹ In contrasting judgments of acceptability and grammaticality, Carson Schütze runs together unhelpfully a judgment’s *being* a performance with its *being about* a performance ([1996], pp. 25–6).

as far from right as you can get. It is simply *indubitable* that metalinguistic intuitions have played a role in linguistics. I cited some evidence of this ([2006a], pp. 481–6, [2006b], pp. 95–7) but I might have cited much more. Consider, for example, Chomsky’s talk of the evidential role of ‘introspective judgments’ ([1964], p. 56). And consider this passage from *Aspects* where he talks of the evidential role not only of *other* linguistic usage *but also* of ‘introspective reports’, which could hardly be anything else but metalinguistic judgments:

Clearly, the actual data of linguistic performance will provide much evidence [...] along with introspective reports (by the native speaker, or the linguist who has learned the language). This is a position that is universally adopted in practice. (Chomsky [1965], p. 18; see also p. 20)¹⁰

Liliane Haegeman, in a popular textbook, puts even more weight on metalinguistic judgments: ‘*all* the linguist has to go by [...] is the native speaker’s intuitions about language’ ([1994], p. 8; emphasis added). Finally, Carson Schütze, in a book entitled ‘*The Empirical Base of Linguistics*’ and devoted to the evidential role of metalinguistic intuitions, remarks:

Throughout much of the history of linguistics, judgments of the grammaticality/acceptability of sentences [...] have been a major source of evidence in constructing grammars. ([1996], p. xi)

The failure to attend adequately to the distinction between metalinguistic judgments and *other* linguistic usage is the major failure in Fitzgerald’s defense of the Chomskian view of linguistic intuitions. My experience has made me think that he is not alone in this failure.¹¹

1.3 Grammaticality versus acceptability

I think that Fitzgerald, Culbertson and Gross, and many others, make far too much of the alleged distinction between a judgment of grammaticality and a judgment of acceptability. Some seem to have the idea that the fact that linguists, in recent times at least, tend to appeal to intuitions expressed using ‘acceptable’, ‘ok’, ‘sounds good’, and the like, rather than ‘grammatical’, helps somehow to sustain VoC over my modest theory-laden picture of intuitions. I detect three related strains of thought behind the emphasis on this distinction.

(I) There is the view that *speakers do not have any intuitions about grammaticality*. Citing (Newmeyer [1983]) and (Gombert [1992]), and on the basis of

¹⁰ See also Barry Smiths’ discussion of the importance of intuitive judgments ‘about the data’ ([2006], p. 451).

¹¹ See (Collins [2006], p. 480, [2008a], p. 31) and (Textor ([2009]) for example.

'Chomsky's definitions', Schütze claims that 'people are incapable of judging grammaticality—it is not accessible to their intuitions' ([1996], p. 26).¹² And Fitzgerald claims:

Acceptability and interpretability as data sources are to be distinguished from the theoretical notion of *grammaticality*, and what is generated by a *grammar*. Speakers have no intuitions about what a grammar mandates, in the theoretical sense of a grammar that concerns linguists. (p. 130)¹³

In the same vein Haegeman claims: 'The native speaker who judges a sentence cannot decide whether it is grammatical. He only has intuitions about acceptability' ([1994], p. 8). At first blush these claims seem obviously false. After all, most Anglophone high school students even have the *word* 'grammatical' and apply it occasionally. However, the assumption is that this word out of the mouths of the folk does not mean what the linguists mean by it; at least, the two words do not corefer. But why should we assume that? Certainly, not simply because the linguists have a vastly better *theory* deploying the term 'grammatical'. We should have learnt from the revolution in the theory of reference started by Saul Kripke ([1980]) that such a difference in theory is far from enough to establish a difference in meaning and reference. Thus, a botanist has a vastly better theory of elms than Hilary Putnam but that does not show that their uses of 'elm' do not corefer. Contemporary astronomers have a vastly better theory of the Earth than ancient ones but that does not show that their uses of 'The Earth' do not corefer. To suppose otherwise is to lapse into mindless meaning holism.¹⁴

If the folk's 'grammatical' corefers with the linguist's 'grammatical' then clearly the high school students have intuitions about grammaticality in the linguists' sense. So if those intuitions are likely to be true, which I argue that they are in simple cases, then they are evidence for a grammar (although far from the best or only evidence; see (C) above). But, despite the revolution, the folk's 'grammatical' may not corefer with the linguist's 'grammatical'. Such matters are notoriously difficult to determine. So, suppose it does not. Then the students will still have intuitions about grammaticality even if only in the folk sense. And they are still likely to be true. So they are still evidence for a grammar. For, a theory should always accommodate the truths of its predecessor. So, the grammar should accommodate the truths of folk linguistics.

In sum, moderately educated people will have intuitions about what is 'grammatical' and these intuitions are evidentially relevant to a grammar.

¹² The Chomsky passages cited by Schütze ([1996], pp. 21–4) do not seem to me to support this claim.

¹³ Another example: 'Speaker/hearers *have* no opinions on grammaticality/interpretability' (Collins [2008a], p. 31).

¹⁴ So Culbertson and Gross are right that I would not accept the fine-grained conception of grammaticality that they mention (p. 724n).

(II) A related strain of thought is widespread: *speakers' intuitions about what is 'acceptable', is 'ok', 'sounds good', and the like are not intuitions about grammaticality*. Culbertson and Gross exemplify this strain, and criticize my 'conflation of acceptability and grammaticality' (p. 725). The conflation here is innocent. For, I argue, these intuitions *are likely to be* intuitions about grammaticality ([2006a], p. 490, [2006b], p. 102). The ordinary evaluative terms like 'acceptable' involved in these intuitions are clearly context-relative. Whether to apply one to an expression, or to anything else, will depend on what we take to be the respect relevant to the evaluation. Thus, in saying that a certain expression or apparel is not acceptable, we might mean to convey that it is impolite or that it is unacceptable in church. For example, in our society it is unacceptable to identify a woman who is part of one's conversational group using 'she' even though doing so is perfectly good English. It is simply not polite. (A standard response in England is: 'Who's "she"? The cat's mother?') We might make these meanings of 'acceptable' explicit, but we might leave them implicit. Now intuitions about what the speaker thinks polite, acceptable in church, and the like are of no interest to a linguist constructing a grammar. What he should be interested in—although I don't claim *very* interested ((C) above)—is what the speaker thinks is *acceptable grammatically in her language*.

I had this to say about how the linguist might go about discovering this:

He may attempt to make this explicit; for example, 'Is this expression acceptable in your language?' Or it may be implicit; 'Is this expression acceptable?' asked in the right context by someone known to be a linguist. In these situations the speaker *may* naturally take 'acceptable' to express her notion of grammaticality (even if she lacks the term 'grammatical' for that notion). If she does take it that way, 'acceptable' in these situations acts as a synonym for 'grammatical'. (*ibid*)

I was assuming here, of course, that the speaker/hearer *has* a notion of grammaticality. There is evidence that, by middle childhood, people typically have objectified their language and acquired a concept of its grammaticality.¹⁵ Still, whatever steps the linguist takes along the suggested lines to elicit an intuition about grammaticality, he may have reasons for doubting that he has succeeded in doing so.¹⁶ In pondering this, the linguist has to consider *what else* would a competent hearer suppose that the linguist was seeking? On being asked whether an expression is acceptable (good, ok, etc.), the hearer has to determine the respect in which the expression is to be assessed for

¹⁵ See (Hakes [1980]), for example.

¹⁶ A further worry is that he may not have elicited intuitions about grammaticality in the speaker's actual language but rather in the prescribed language of the speaker's community. This raises another doubt about the evidential status of these intuitions. However, we should keep in mind that there is likely to be a high correlation between the speaker's language and the prescribed one.

acceptability. She is primarily concerned here not with the literal meaning of the linguist's question but with what he means by it (Grice). In an appropriate context, there may be nothing about an expression that the linguist could plausibly be seen as asking other than about its grammaticality.

I am claiming that the *content* of a speaker/hearer's acceptability intuition, and what she *means by* her expression of that intuition, are likely to concern grammaticality. It is important to see that this claim is not in the least undermined by the fact that the intuition, taken as about grammaticality, may be *wrong*. Thus, notoriously, a hearer is likely to find a grammatical sentence containing multiple center-embeddings unacceptable. Both VoC and my alternative explain this by appealing to the problems of processing such a sentence. So it is certainly true that it is *because of* these processing problems, not because of ungrammaticality, that the sentence is judged unacceptable. But this does not show that the judgment is not *about* ungrammaticality. The cause of the judgment is one thing, its content another.¹⁷ On my view, experiences of processing sentences usually provide good evidence of grammaticality but fail to do so with these cases of multiple center-embeddings. So in those cases, the experiences prompt a hearer to a false judgment of ungrammaticality.

Culbertson and Gross (p. 722) mention Chomsky's discussion in *Aspects* of the theory of performance ([1965], pp. 10–5). This influential discussion may help explain why linguists think that speakers' acceptability judgments are not grammaticality judgments. For the purposes of that discussion, Chomsky decides 'to use the term "acceptable" to refer to utterances that are perfectly natural and immediately comprehensible without paper-and-pencil analysis, and in no way bizarre or outlandish' (p. 10). For Chomsky, the notion of acceptability that he has thus defined is a technical one in 'the study of performance', whereas grammaticalness is one in 'the study of competence'. Despite this difference, the two notions are related: 'Grammaticalness is only one of many factors that interact to determine acceptability' (p. 11). Now, obviously, Chomsky's discussion of the role of acceptability in the theory of linguistic performance involves the technical notion he has just defined. Let us say that this notion is about 'C-acceptability'. C-acceptability is certainly not grammaticality. So, if the intuitions that linguists elicit from the folk using terms like 'acceptable', 'ok', and 'sounds good' were about C-acceptability then they would indeed not be about grammaticality. Perhaps linguists assume that these folk intuitions *are* about C-acceptability. But why assume that? We need an argument for this assumption. And it follows from my discussion that the assumption is very wrong. Ordinary folk, who have never even heard of *Aspects*, will understand these terms in the ordinary context-relative way I have illustrated. Indeed, how else

¹⁷ Ironically, Culbertson and Gross accuse me of confusing the cause and the content (p. 725).

would they understand them? It may be that the folk apply these terms to sentences in appropriate contexts *because* the sentences are C-acceptable—C-acceptability *explains* the application of the terms (see above)—*but C-acceptability is not the content of the terms*. Let us say that these terms, understood in the ordinary way, refer to ‘F-acceptability’. Then, my point is that *F-acceptability intuitions are not about C-acceptability*. And I have argued that, in appropriate circumstances, F-acceptability intuitions are likely to be about grammaticality. Chomsky’s discussion of C-acceptability has little bearing on that argument. His technical notion of C-acceptability may well be useful in the theory of performance but it is not what speakers mean when they use those ordinary context-relative evaluative terms.

This having been said, intuitions elicited in *other* terms might be about C-acceptability, or something close. *Precise words matter here*. Thus, instead of using those earlier terms to elicit intuitions, the linguist might ask whether a speaker ‘finds it easy to understand’ a given string. The speaker is being asked to reflect upon the processing of language. Various explanations of such intuitions are possible but it *may be* that the best explanation in a particular case is that the string is or is not grammatical. Where that is so, the intuition is clearly evidence for a grammar. Nonetheless, this is not an F-acceptability intuition and it is not *about* grammaticality. And it is not the sort of intuition that I am discussing in this section.

In light of all this, what justifies the linguist’s use of terms like ‘acceptable’ rather than ‘grammatical’ to elicit intuitions about grammaticality? Only a lack of confidence that the subject uses ‘grammatical’ to express her concept of grammaticality in her language. Perhaps she does not have the term at all or has not mastered its use for that purpose.

(III) The third and final strain of thought behind the emphasis on distinguishing grammaticality and acceptability is that *linguists are simply not interested in intuitions about grammaticality*. According to Culbertson and Gross, linguists do not ‘in fact use intuitive beliefs concerning grammaticality as evidence for their hypotheses—even if they sometimes use the label “grammaticality judgments” for what we label “acceptability judgments”’ (p. 724).¹⁸ In fact, linguists have *frequently and regularly* called what they use as evidence ‘grammaticality judgments’. Culbertson and Gross think that we should not take linguists at their word because the judgments that linguists *should* be using as evidence are the acceptability ones not the grammatical ones. I disagree. The grammaticality intuitions of the sufficiently expert should be used as evidence in linguistics just as analogous intuitions should be, and are, in other sciences ([2006a], pp. 491–6, [2006b], pp. 103–08). Indeed, on VoC, it is hard to see why intuitions about grammaticality should not be seen as

¹⁸ See also (Textor [2009]).

evidence just as intuitions about ambiguity and coreference are. I think that there has been a great deal of uncertainty, if not confusion, in linguistics over the distinction between grammaticality and acceptability ([2006a], pp. 488–91, [2006b], pp. 100–3).¹⁹ As a result, we are not in a position to be confident that linguists should not be taken at their word when they talk of ‘grammaticality judgments’. Furthermore, the F-acceptability intuitions, elicited from the folk by the likes of ‘sounds good’, that some linguists may call ‘acceptability judgments’ because they wrongly think of them as C-acceptability judgments, are, in the context, likely to *be* grammaticality judgments.

How does all this bear on VoC? The F-acceptability intuitions that linguists elicit from competent speakers are, for the most part, grammaticality intuitions. To the extent that they are, they are evidence for a grammar (although far from foolproof and not the most important evidence; see (C) above). To that extent, it can be no more plausible that they are the voice of competence than that intuitions explicitly about grammaticality are. To the extent that they are not intuitions about grammaticality, they are *evidentially irrelevant* to grammars.²⁰ This is so whether one adopts VoC or my modest view of the intuitions. Thus the key to the VoC view of why a speaker’s intuitions are evidence is that her underlying competence is responsible, noise aside, for their informational content. But what relevant information could competence be responsible for in an F-acceptability intuition other than information about grammaticality? In sum, the only thing of interest that a speaker could convey to a linguist in an F-acceptability judgment is an opinion about grammaticality.

Finally, we should not lose sight of the fact that metalinguistic intuitions other than ones about acceptability/grammaticality have played a role in constructing grammars; for example, intuitions about ambiguity and coreference. The discussion in this section does nothing to remove the attraction of my modest picture of these intuitions. Speakers clearly have the intuitions, linguistics are interested in them as evidence, and they are not plausibly seen as the voice of competence.

2 Fitzgerald

Gareth Fitzgerald ([2010]) has mounted a detailed critique of my view of linguistic intuitions and a defense of what he takes to be Chomskian orthodoxy. The thoroughness of this treatment makes this a very helpful article. I think it reveals the fundamental flaws of the orthodoxy.

¹⁹ This confusion is described and, to a degree, exemplified by Schütze ([1996]; see particularly pp. 20–7).

²⁰ In saying this I am not rejecting the Duhem–Quine point that anything *could be* evidence for grammars. I am simply claiming that, as a matter of fact, such intuitions *are not* evidence for grammars.

In Section 2.1, I shall discuss Fitzgerald's surprising rejection of my VoC interpretation of the orthodox Chomskian view. In Section 2.2, I shall summarize the main case against VoC. In Section 2.3, I shall consider his defense of what he takes to be that orthodox view. In Section 2.4, I shall mention two additional criticisms of VoC. In Section 2.5, I shall briefly address Fitzgerald's criticisms of my alternative view.

2.1 Do Chomskians subscribe to VoC?

I provide a great deal of evidence that the orthodox Chomskian view is VoC ([2006a], pp. 482–6, [2006b], pp. 96–7),²¹ including a shortened version of this striking passage:

it seems reasonably clear, both in principle and in many specific cases, how unconscious knowledge issues in conscious knowledge [...] a person has unconscious knowledge of the principles of binding theory, and from these and others discussed, it follows by computations similar to straight deduction that in [*I wonder who the men expected to see them*] the pronoun *them* may be referentially dependent on *the men* whereas in [*The men expected to see them*] it may not [...] That this is so is conscious knowledge'. (Chomsky [1986], p. 270)²²

Fitzgerald himself describes that orthodoxy as if it were VoC. Yet he denies that it *is* VoC.²³

In the abstract for his article, Fitzgerald characterizes the 'orthodox conception' as follows: 'linguistic intuitions are the upshot of a system of grammatical competence as it interacts with performance systems for perceiving and articulating language' (p. 123). In the text, the view is that 'these intuitions are yielded by special cognitive systems responsible for recognizing and shaping grammatical categories in speakers' utterances' (p. 125). Later the competence is alleged to be 'responsible' for the intuitions (p. 130); it 'shapes'

²¹ Consider this recent evidence also: 'Unconscious, information-bearing states of the language faculty gives rise to conscious knowledge that is immediately reflected in the speaker's intuitive linguistic judgements' (Smith [2006], p. 443; and see pp. 451, 454).

²² Mark Textor makes a similar claim: he talks of intuitions being 'derived from mentally represented or tacitly known grammatical principles' and likens this to 'a theorem [being] derived from already established truths' ([2009], p. 396).

²³ Fitzgerald tries to explain away the quoted passage from Chomsky (and it is the only item of evidence that he discusses). He starts in a way that fits VoC perfectly: the examples of 'conscious knowledge' in question, 'such as our knowledge that "John" needn't mean "him" in "John shaved him"' are alleged to be 'a consequence of the principles of UG'. However, he goes on. 'But none of this, Chomsky claims, neither the UG possessed nor the way its consequences are computed, is accessible to the speaker. So it is unclear why Chomsky would think that it might be voiced in their intuitions' (p. 145). The inaccessibility of the principles and computations is beside the point because VoC does not claim their accessibility. Quite the contrary ([2006a], p. 484 n. 6, [2006b], p. 96n). VoC claims access to *linguistic facts determined by the principles*. Smith ([2006], p. 443) and Collins ([2008a], p. 17n) also try to explain away this Chomsky quote. I argue that Smith fails ([2006c], p. 592) and I would argue that Collins does too. None of these authors addresses any of the other evidence I cite.

them (p. 144). Now, each of these intuitions has a certain informational content. I take Fitzgerald, in these passages, to be saying that this content is supplied somehow or other by the competence. And that just *is* the VoC explanation of intuitions. Indeed, *what else* could he be saying? Not that the *syntax* of an intuition is the ‘upshot’, etc. of the competence. That would be trivial. And if that were all there were to the ‘orthodox conception’ it would not have provided the theory we need. For, as noted in Section 1.2, the intuition’s syntax is not what makes it evidentially interesting. It is interesting because of its *content*. If Fitzgerald is describing anything worthwhile it must be VoC. Indeed, how else can we make sense of much of his discussion, particularly the analogy with vision (discussed in Sections 2.3 and 2.4)?

Despite this, Fitzgerald resists the attribution of VoC to Chomskians:

Devitt is absolutely correct to argue that intuitions are not the ‘voice of competence’. But then he is wrong to claim that this is a commitment of the orthodox model [. . .]. So there is no reason, on the orthodox model, to expect a speaker’s judgements to give ‘voice’ to their competence in the way Devitt suggests. (p. 144)

What is going on? It is hard to be certain but it rather looks as if Fitzgerald has misunderstood VoC. The explanation of linguistic intuitions provided by VoC, as I have characterized it, is extremely lacking in details. Indeed, that is one of my main criticisms of it; see Section 2.2 below. So far as I can see, Fitzgerald finds VoC wanting as an interpretation of Chomskian views by loading it with details it does not have and then rightly pointing out that Chomskians would find those details objectionable. These loaded details *may* include the following: taking VoC to allow no role for cognitive systems other than competence in producing intuitions (no ‘noise’); taking it to allow no evidential role for the intuitive behavior I have called ‘*other* linguistic usage’; taking it to require speakers to deploy the ‘theoretical concepts that animate grammatical theory’ (p. 137); taking it to require ‘direct access to the grammatical [. . .] principles’ (p. 143); taking it to imply that intuitions yield such comprehensive information as would make it unnecessary ‘to unearth a range of other evidence in theorizing about the competence system’ (p. 145). But none of these details is part of the doctrine VoC that I attributed to Chomskians. What I attributed—see Section 1.1 (A)—is simply that competence somehow or other provides information about grammatical facts, such facts as the acceptability and ambiguity of expressions (but not, note, information about grammatical *principles*). As I have just noted, this seems to be the very doctrine that Fitzgerald himself takes to be part of the Chomskian orthodoxy. Of course, this doctrine is so incomplete that it could be only the beginning of the needed explanation of metalinguistic intuitions. But if it were not the beginning of the Chomskian explanation we would not have the

faintest idea what that explanation might be. It sometimes looks as if ‘voice of competence’, my somewhat playful *name* for this doctrine, is the root cause of Fitzgerald’s unhappiness with my interpretation.²⁴ (But see also the ‘dark suspicion’ that ends Section 2.6).

The evidence that the orthodox Chomskian view of these metalinguistic intuitions is VoC is overwhelming. For, if the view were not that, there would be no view. So I will continue to call that view ‘VoC’.

Nonetheless, suppose that, despite all this evidence, VoC was not the orthodox Chomskian view. It is certainly taken by many to be so. And, as I note ([2006b], p. 204n) it is the main inspiration for the popular ‘theory-theory’ view of folk psychology. It is also the inspiration for the view of referential intuitions suggested (but not endorsed) by Stephen Stich ([1996], p. 40). So VoC would be worth refuting even if it were not the Chomskian view.

2.2 The main case against VoC

VoC should not be saddled with the details that Fitzgerald rightly finds objectionable, but it badly needs *some* details if it is to be an explanation that we should take seriously. It is not enough to say that the competence *somehow or other* provides information about such linguistic facts as the acceptability and ambiguity of expressions. We need to know *how* it does. The main problem with VoC is that the literature provides almost no answer.

The first step in supplying some details is to distinguish two versions of VoC. According to one, a speaker’s competence in her language faculty embodies *representations* of linguistic rules and principles and she derives her intuitive judgments from these representations by a causal and rational process like a deduction. I call this ‘the standard version’ because it is suggested by much of the literature, including the striking passage from Chomsky quoted at the beginning of Section 2.1. Despite the evidence for this interpretation, I accept that it may not be the right one. If it is not, Chomskians must have a ‘non-standard’ version of VoC: the intuitions must be provided somehow by embodied but *unrepresented* rules ([2006a], pp. 484–6, [2006b], pp. 96–8).

Let us start with the standard version. We are given no details of the causal-rational route from an unconscious representation of rules in the language faculty to a conscious judgment about linguistic facts in the central processor. Still, the idea of one sort of representation leading to another is familiar and so this standard explanation may seem promising. I produce several reasons for thinking it is not promising at all. The most important of these is aimed at its highly *immodest* commitment to speakers representing

²⁴ I would make a similar response to Collins ([2008a], pp. 16–9) who also resists the attribution of VoC to Chomskians.

linguistic rules and principles. I argue at length that this commitment is unsupported and implausible ([2006b], pp. 87–121, 195–272). However, this commitment, hence the standard version, has no friends among my Chomskian critics (Smith [2006]; Collins [2006], [2007], [2008a]; Pietroski [2008]; Slezak [2009]) and, I strongly suspect, is not favored by Fitzgerald. So I shall set it aside and turn to the nonstandard version.

The main problem with this version is that we do not have *any idea* how a system of embodied but unrepresented rules might provide linguistic intuitions. Not only do we lack the details needed for a plausible explanation but attention to other such systems gives good reason to suppose that the linguistic system does not provide these intuitions and so we *could never* have the details:

The explanation would require a relatively direct cognitive path from the embodied rules of the language to beliefs about expressions of that language, a path that does not go via central-processor reflection on the data. What could that path be? [...] Consider some other examples. It is very likely that rules that are embodied but not represented govern our swimming, bicycle riding, catching, typing, and thinking. Yet there does not seem to be any direct path from these rules to relevant beliefs. Why suppose that there is such a path for linguistic beliefs? Why suppose that we can have privileged access to linguistic facts when we cannot to facts about these other activities? We do not have the beginnings of a positive answer to these questions and it seems unlikely that the future will bring answers. ([2006a], pp. 506–7, [2006b], p. 118)

This is my main case against the nonstandard version of VoC. The comparison of linguistic and visual intuitions in Section 2.4 reinforces the case.

2.3 Fitzgerald's defense of VoC

We should start by noting two things that Fitzgerald does *not* do. (A) He does not supply any of the details that VoC needs. He is content to observe that ‘we don’t know how conscious judgements are derived, or the mechanics of the role the linguistic systems play in issuing in these judgements’ (p. 144). (B) Fitzgerald continues: ‘Linguists infer that a structured grammatical competence system shapes these intuitions’ (*ibid*). And, of course, they do infer this; i.e., they infer VoC. But the issue is whether they are *right* to do so. In responding to Locke’s representative realism, Berkeley pointed out ([1710], Section 19) that for an inference like this—what we now call an ‘abduction’—to be good, the explanation it involves must have *an appropriate level of detail*. I have developed the point thus: ‘if the explanation posits *x* as the cause of *y*, it must say enough about *the mechanism by which x causes y* to not leave this mysterious; a wave of the hand is not sufficient’ ([2006b], p. 199). My main case against VoC is that it does not come close to providing an

appropriate level of detail and that a consideration of other systems suggest that such details *could not* be provided. Fitzgerald does not address this case.²⁵

It may be that he does not address it because he has misunderstood it. Referring to my discussion of the case he remarks:

To be clear, proponents of the orthodox model should agree with Devitt that there is no path from the encoding of the deep principles of grammatical competence in a speaker to their having correct beliefs about those rules. (p. 152)

But VoC does not assume that speakers have correct beliefs about linguistic *rules*. Indeed, it is obvious to everyone that they don't. VoC assumes rather that they often have correct beliefs about linguistic *facts*. These beliefs are the intuitions that VoC is supposed to explain, intuitions like the earlier ones about (1), (2), and (3) (Section 1.2). The main case is aimed at *that* explanation. We have good reason to believe that the underlying state that, according to VoC, provides the information about these linguistic facts, a state of embodied but unrepresented rules, is cognitively impenetrable.

So what *does* Fitzgerald do? First, he attempts to make us feel better about the lack of details. I shall discuss the attempt in this section. Second, he criticizes my alternative explanation. I shall discuss that briefly in Section 2.5.

Apart from my main case against VoC, I have four additional criticisms of the doctrine. These were offered originally against the standard version but they apply as much to the nonstandard one. One of these criticisms concerns an alleged analogy between linguistic intuitions and visual intuitions. Fitzgerald's lengthy discussion of this analogy is central to his attempt to make us feel better about VoC's lack of detail:

After all, there is currently no account of how the computations of the visual system issue in conscious intuitive judgements about the properties of a presented scene. So, it's not clear that there is a special problem with language. (p. 135)

I shall consider this alleged analogy in a moment. But first, Fitzgerald responds briefly to another of my four additional criticisms.

This criticism has two parts. (i) The speakers' intuitions used to test grammars are commonly ones of 'acceptability', 'goodness', and the like. Yet, the theory of the embodied principles and rules that are alleged to yield these intuitions is a theory of 'grammaticality'. 'If, in our intuitive judgments, competence is really speaking, why doesn't it *use its own language*?' ([2006a], p. 489, [2006b], p. 101). (ii) There are clearly lots of linguistic facts that ordinary speakers have few if any intuitions about: facts about heads, c-command, and so on. Why is that? 'If our competence [. . .] speaks to us at

²⁵ Neither does Smith ([2006], p. 446); cf. (Devitt [2006c], pp. 593–4).

all, how come it *says so little?* (*ibid*). We wonder what account of the causal route from embodied principles and rules to intuitions could account for (i) and (ii).

This is a preliminary criticism of VoC and I don't put much weight on it. Still, the criticism does make VoC's problem of supplying the needed details a little more acute. Fitzgerald's response to the criticism is not a success. (i) 'Grammatical competence does not "use its own voice" insofar as the properties of the sub-personal competence system are not available to mere personal-level reflection' (p. 143). But this does not explain why a sub-personal system that determines grammaticality yields intuitions expressed in terms of 'acceptability', 'goodness', and the like. (ii) 'The competence "says so little" because grammatical competence is only one factor involved in linguistic judgement which engages systems of linguistic performance and more besides' (*ibid*). The role of factors other than competence explains why *any* message provided by competence might get corrupted *en route* to a conscious intuition, but it does not explain why competence provides *so few*, possibly corrupted, messages to conscious intuition.

The comparison of linguistic intuitions with those yielded by the visual system is more interesting. Georges Rey ([2006], pp. 563–8), following in Chomsky's footsteps ([1965], pp. 8–9, [2000], p. 125; see also Collins [2007], p. 421), urges a defense of VoC based on this comparison. So does Fitzgerald:

just as reports of visual impressions constitute data for theories of the visual system that processes visual information, so, on the orthodox model, linguistic intuitions constitute data for theories of the grammatical competence that constrains the linguistic forms that a speaker finds acceptable and how they can be interpreted. (p. 134)

I rejected this analogy in responding to Rey. Indeed, I argued that the comparison of linguistic and visual intuitions counts *against* VoC. That is another of my additional criticisms; see Section 2.4.

First, I should allow that, had the analogy held, it would have provided support for VoC (hence, given their fondness for the analogy, support for attributing VoC to Chomskians). For, the visual module, all on its own, provides information about the visual facts. There is a relatively direct path from the visual module to the intuition that, say, a certain figure is a duck or a rabbit. This is not to say that the central processor is not involved in visual intuitions: it makes the judgment based on the information provided by the module. Still, the central processor's contribution seems small. And though I was right to say that these visual judgments are covered by my modest theory of intuitions ([2006a], p. 501, [2006b], p. 113)—they are empirical central-processor responses to phenomena differing from other such responses only in being fairly immediate and unreflective—I should have noted that they

are also covered by something like VoC. So the contrast I drew between the modest theory and VoC was overdrawn: the story of visual intuitions is compatible with the modest theory.

Turn now to my rejection of the analogy ([2006a], pp. 500–1, [2006b], pp. 112–3). There is indeed an analogy between intuitive judgments based on deliverances to the central processor by the language module and by the visual module. But the analogous intuitions are of *what is said*, on the linguistic side, and of *what is seen*, on the visual side. Thus, the language module may provide the basic information in language processing for my intuition that a speaker of (3) said that Tom thinks that Dick loves himself just as the visual module provides the basic information in visual processing for my intuition that I see a duck. But intuitions about *what is said* are not the intuitions that concern us. For, whether or not those intuitions should count as metalinguistic, they are not intuitions about *the syntactic and semantic properties of expressions*. They are not, for example, my intuition that, in (3), ‘himself’ must refer to the same person as ‘Dick’. The analogy with vision does nothing to support the VoC view that *this* intuition is provided by the language faculty.

Fitzgerald finds my argument ‘unconvincing for two reasons’ (p. 146). Both of these are beside the point.

First, and fundamentally, it is unclear why the only intuitive materials made available to judgement by the linguistic systems are intuitions about what is said rather than intuitions about the acceptability of linguistic forms and their possible structural interpretations. (p. 146)

But the analogy with vision is supposed to be *an argument for* believing that linguistic systems make intuitions about the acceptability available, i.e., *for* believing VoC: the visual system makes visual intuitions available and they are supposed to be analogous to intuitions about acceptability. But, I argue, they are not; they are analogous to intuitions about what is said. So we are left without an argument for VoC. In response, Fitzgerald might have tried to reinstate the analogy. Thus, he might have tried to argue that the visual module not only yields intuitions about what is said but *also* intuitions that are guides to the nature of the underlying competence, intuitions that are analogous to metalinguistic ones. But he did not try this and it would have been a mistake, as we shall soon see. Rather, Fitzgerald finds support for VoC in some examples that suggest that speakers have intuitions ‘of broad structural properties of pieces of language’ (*ibid*). But the existence of these intuitions is not in question. What is in question, what needs an argument, is that these intuitions *are made available by the underlying competence*. Fitzgerald leaves us without an argument.

His second reason for finding my argument unconvincing is no more successful. He points out that intuitions about what is said ‘are of obvious

relevance' even though the intuitions do not make explicit mention of theoretical properties (*ibid*). *Of course*, these intuitions are evidentially relevant although not, I suspect, nearly as relevant as what I am calling 'other linguistic usage'. The point of my argument is not to downplay intuitions about what is said but to show that the visual analogy does not support VoC. And VoC is not a theory of intuitions about what is said; it is a theory of intuitions deploying metalinguistic concepts like acceptability and ambiguity. (I know: I named it!) The issue is the source of these metalinguistic intuitions. Fitzgerald has changed the subject to one where we largely agree and missed the point of the disagreement.

In sum, Fitzgerald does not provide the details that VoC needs and does not respond to the main case against it. His defense of VoC rests almost entirely on the analogy of linguistic intuitions with visual intuitions. His response to my criticism of this analogy is beside the point. The defense fails.

2.4 Two additional criticisms of VoC

In the last section, I have argued that the comparison of linguistic and visual intuitions does not support VoC. We can go further: the comparison with vision undermines VoC. This is another of my additional criticisms ([2006a], p. 503, [2006b], p. 114). The visual module may well be governed by embodied but unrepresented rules. And the operation of those rules may yield information that guides the module in arriving at its message to the central processor of what is seen. Yet the central processor has direct access only to the message, not to any intermediate information used in arriving at the message. Information involved in Marr's 'primal', '2.5 D', and '3 D' sketch is cognitively inaccessible (Fodor [1983], p. 94).²⁶ Why suppose that the language module is any different? It is plausible, although I don't say right ([2006b], pp. 220–43), to think that the parser, in arriving at its message of what is said, uses syntactic and semantic information provided by the language module. But if the language module is indeed like the visual module all of this information should be cognitively inaccessible.

Finally, I draw attention to another of my criticisms of VoC, one that Fitzgerald does not address, and some further related evidence. The criticism draws on empirical evidence about implicit learning to argue against the standard version of VoC ([2006a], pp. 504–5, [2006b], pp. 115–6). The argument works just as well against the nonstandard version. 'Implicit learning', is learning that takes place 'largely without awareness of either the process or the products of learning' (Reber [2003], p. 486). Now, if implicit learning were

²⁶ As Fitzgerald says: 'Subjects of visual experiment do not give voice to the content and organization of their visual system' (p. 144). But instead of seeing this as evidence against VoC, he sees it as evidence against the *attribution* of VoC; see Section 1.3 above.

largely a matter of coming to embody certain rules that *both* govern the performance of a task and yield intuitions about the task, we would expect improvement in performance to be matched by improvement in intuitions. Yet that is not what we find at all: improvement in task performance is dissociated from improvement in the capacity to verbalize about the task (Broadbent *et al.* [1986], p. 34; Stanley *et al.* [1989], p. 569). So, on the assumption that language learning is an example of implicit learning we have good evidence against the VoC view that the embodied rules that largely govern linguistic performance also provide our intuitions about the language. Of course, one might resist this point by denying the assumption that language learning *is* implicit learning. Still, I argue ([2006b], pp. 210–20), the evidence that it is implicit learning is very persuasive.

I have since become aware of a body of literature that provides much more persuasive empirical evidence against VoC, without the implicit-learning assumption.²⁷ The evidence suggests that the ability to speak a language and the ability to have metalinguistic intuitions about the language are quite distinct.²⁸ Schütze ends a critical discussion of much of this evidence with the observation that ‘it is hard to dispute the general conclusion that metalinguistic behavior is not a direct reflection of linguistic competence’ ([1996], p. 95). In other words, it is hard to dispute that VoC is false.

I turn finally to Fitzgerald’s criticisms of my alternative.

2.5 Fitzgerald’s criticisms of my alternative

I think that Fitzgerald’s criticisms of my views are badly flawed but, with one exception, I shall be very brief about saying why. The exception is a criticism of particular interest because it rests on a failure to distinguish metalinguistic intuitions from *other* linguistic usage. This failure pervades Fitzgerald’s defense of Chomskian orthodoxy and is, I suspect, quite common in Chomskian circles.

So, let us consider the exception. On my view, linguistic intuitions are empirical theory-laden central-processor responses to linguistic phenomena. Fitzgerald responds to this with an interesting argument that my view ‘is inconsistent with the pre-doxastic nature of linguistic intuition’ (p. 148). Now, something *must* have gone wrong here because the intuitions in question are *judgments* (Section 1.2) and so must be beliefs. Of course, there is likely to be lots of pre-doxastic experience and other mental activity before the

²⁷ See particularly, (Hakes [1980]; Ryan and Ledger [1984]; Bialystok and Ryan [1985]; Bialystok [1986]). (I am indebted to Justyna Grudzinska for drawing this literature to my attention.)

²⁸ Pietroski finds my view of what it is to be competent in a language ‘unduly narrow’ in that it does not include ‘recognizing yuckiness and (un)ambiguity, entailments, and so on’ ([2008], p. 665). In rejecting VoC I am indeed denying that the competence to *use* a language is essentially conjoined with the competence to make intuitive judgments *about* the language.

judgment, on anyone's account, but the judgment itself is doxastic. Why does Fitzgerald think otherwise?

Once again he sees an analogy with vision. Take the two lines in the Muller–Lyer illusion as an example. It is a striking fact that the lines seem intuitively to differ in length no matter how well we know that they do not. This is the result of pre-doxastic 'mandatory' processes in the vision module. Fitzgerald claims that our linguistic intuitions are similarly mandatory and pre-doxastic: 'the intuitive takes speakers have on linguistic material are pre-doxastic in a way that compares with visual appearances' (p. 138).²⁹

I too would want to emphasize that processes in the vision module are pre-doxastic. However, this is not to say that any resulting judgment is pre-doxastic. Consider someone who confronts the Muller–Lyer lines knowing that they are of equal length. The confrontation will not make her believe that the two lines differ in length but she will, nonetheless, come to a belief. She will come to the belief that the two lines *seem* to differ in length. Still, let us not fuss over 'pre-doxastic'. The mandatory phenomena are very real in the case of vision. If the analogy with linguistic intuitions held it would certainly count against my central-processor account of them, as Fitzgerald suggests. It would give some support to the idea that just as the vision module mandates the visual intuition the language module (linguistic competence) mandates the linguistic one. But the analogy does not hold.

Fitzgerald thinks otherwise because he is, once again, conflating linguistic intuitions with *other* linguistic phenomena: 'We can't help but hear the sounds of our language as structured and meaningful, forming an intuitive take on their form and interpretation independently of our choosing to reflect upon them' (p. 148). Right.³⁰ But the fact that linguistic competence mandates the way we hear the sounds of our language does nothing to show that it mandates our metalinguistic intuitions about the language. And I am arguing that it doesn't mandate those intuitions.

Fitzgerald has earlier claimed that the impression that 'Many more people have been to France than I have' has a complete structure, and the impression that 'The horse raced past the barn fell' has not, 'can persist' despite our coming to believe otherwise (p. 139). Indeed, I think the impressions *can* persist, but they are surely not mandatory in the way visual illusions are. In any case, the explanation of this persistence, on anyone's view, is not that it is mandated by the language module. For the Chomskian, after all, the module

²⁹ Textor ([2009]) also hankers after pre-doxastic intuitions as evidence. He claims that there are non-judgmental 'linguistic seemings' in between linguistic performances and metalinguistic judgments, and that these seemings have 'epistemic authority' in grammar construction. I argue ([forthcoming]) that there are no such seemings. Nenad Mišević ([2006]) also posits 'in-between' intuitions but does not think that they are pre-doxastic.

³⁰ This plays an important role in my argument for the tentative proposal that language processing is a fairly brute-causal associationist process ([2006b], pp. 220–43).

embodies our *knowledge* of language and yet the impressions are false. I take it to be generally agreed that the impressions are to be explained by features of processing independent of competence. This is quite congenial to my alternative: these features result in misleading data being presented to the central processor for reflection.

Finally, I respond very briefly to some of Fitzgerald's other criticisms. (i) I think that speakers' intuitions do *not* have 'the central evidential role' in grammar construction (Section 1.2). So my model does not require a 'revision of existing methodology' (p. 150), just a revision in what linguists commonly *say about* that methodology. (ii) The intuitions of linguists that I think have an evidential role are not 'theoretical hunches' (p. 153) but rather theory-laden *observations* about linguistic expressions (like (1), (2), and (3) in Section 1.2). (iii) Fitzgerald claims that 'gathering evidence about what speakers would say or understand [...] *just is* part of probing [...] what linguists normally call linguistic intuition' (*ibid*). His point here is merely verbal, related to his persistent conflation of metalinguistic intuitions with *other* linguistic usage.

In sum, Fitzgerald's criticisms leave my alternative view of linguistic intuitions unscathed.

2.6 Conclusion

I have claimed that the received Chomskian view is that the informational content of a speaker's linguistic intuition is provided somehow or other by her linguistic competence. I name this view 'VoC'. My alternative view is that this content arises from an empirical theory-laden central-processor response to linguistic phenomena (Section 1.1). It is crucial to note that the intuitions in question in this disagreement are metalinguistic ones not intuitive '*other* linguistic usage' (Section 1.2).

In his response to my views, Fitzgerald characterizes the 'orthodox conception' of Chomskians as if it were VoC but then, surprisingly, goes on to deny that it is. This denial seems to arise from his loading VoC with details it does not have and then rightly pointing out that Chomskians would find those details objectionable. If VoC were not the orthodox conception there would be no orthodox conception (Section 2.1).

VoC should not be saddled with these objectionable details but it badly needs *some* details if it is to be an explanation that we should take seriously. We need to know *how* competence provides information about linguistic facts. The main case against VoC is that we do not have *any idea* how embodied but unrepresented rules might do this. Not only do we lack the details needed for a plausible explanation, but also a consideration of other systems suggests that we will never have them (Section 2.2).

In his defense of VoC, Fitzgerald makes no attempt to provide the details that VoC needs nor to respond to the main case against it. His defense is an attempt to make us feel better about the lack of details. It rests almost entirely on an alleged analogy between linguistic intuitions and visual intuitions, an analogy that I have criticized before. He finds my criticism ‘unconvincing’ but his reasons for doing so are beside the point. His defense fails (Section 2.3).

I went on to argue that not only does a comparison of linguistic and visual intuitions not support VoC, it undermines it. For, if the language module is indeed like the visual module the information it allegedly supplies to intuitions should be cognitively inaccessible. Further, I drew attention to a body of literature that provides persuasive empirical evidence against VoC (Section 2.4).

Finally, I have argued against one of Fitzgerald’s criticisms of my alternative view of linguistic intuitions and briefly indicated why others also fail (Section 2.5).

The most conspicuous and pervasive mistake in Fitzgerald’s discussion, whether in his defense of VoC or his criticism of my alternative doctrine, is inattention to the distinction between the metalinguistic intuitions that are the concern of those two doctrines and *other* linguistic usage which is not. A dark suspicion intrudes. I wonder whether this inattention has caused Fitzgerald, somehow or other, to overlook the need for a theory of the metalinguistic intuitions and so caused him not to offer one. Is this inattention why he resists the attribution of VoC and why his defense of VoC is so weak? Is it also why he sometimes writes as if linguists have *no interest* in these intuitions ([2010], pp. 136–7)? Yet, as I have emphasized (Section 1.2), linguists *obviously* do have an interest. And linguists are right to have an interest because the intuitions are often good evidence for a grammar. So we badly need a theory of the intuitions. We need an explanation of them that shows why, *because of their content*, they are often good evidence. If Chomskian orthodoxy is not offering us VoC as an explanation, it is not offering us any explanation.

3 Culbertson and Gross

In Section 1.1, I noted a consequence of my view of linguistic intuitions: linguists, having better theories than ordinary competent speakers, are likely to have better intuitions. So the intuitions of the linguists should be preferred as evidence. Jennifer Culbertson and Steven Gross (‘C&G’), have conducted an interesting experiment to show that I am wrong about this ([2009]). I shall argue that they do not succeed.

In Section 3.1, I shall consider the significance of their position on the alleged distinction between intuitions about acceptability and about grammaticality. In Section 3.2, I consider their experiment.

3.1 Acceptability versus grammaticality

C&G make a great deal of the difference between ‘acceptability’ judgments and ‘grammaticality’ judgments. So, what difference do they have in mind? The ‘acceptability’ intuitions that they seek in their experimental instructions are judgments ‘whether individual sentences sound good or not *to you* [...]. [whether] you would or could say [them] under appropriate circumstances’ (p. 734). They contrast these with ‘grammaticality’ intuitions which they define as judgments about ‘what sentences are permitted, or generated, by a grammar’ (p. 722; they mean by this, as they should: permitted, or generated by the *true* grammar of the language in question). They do not say why acceptability intuitions are good evidence for a grammar, perhaps favoring something like VoC as an explanation. I take it that they think that VoC does *not* explain grammaticality intuitions.

Now, on the strength of the discussion in Section 1.3, I claim that, with a possible exception to be discussed in Section 3.2, the acceptability judgments that C&G elicited are likely to *be* grammaticality judgments of the sort C&G have defined. Their talk of what ‘sounds good to you’ is a paradigm of the sort of talk that is likely, in the circumstances of their experiment, to elicit intuitions about grammaticality. Furthermore, this likelihood is increased by C&G’s laudable effort to prevent getting intuitions about a prescriptive grammar: they emphasize, with an example, that they ‘are interested in your linguistic intuitions, not in the rules of “proper English” that you might have been taught in school’ (*ibid*). The implied contrast between the rules of ‘proper’ English and the rules of the subject’s actual English seems to invite the subjects to respond by deploying their concept of grammaticality in their actual language (a concept which we can assume that they have; see Section 1.3).

In light of this, I think that C&G make far too much of the difference between acceptability and grammaticality judgments. This mistake has many consequences. I have noted one already: their incorrect view that linguists do not, and should not, use grammaticality judgments as evidence (Section 1.3). Here are two more.

The mistake causes them difficulties in understanding what I intend in claiming that the intuitions of the linguists are likely to be better than the folk’s (p. 725). There should not be any doubt about my intentions. The intuitions I have in mind that are likely to be better include acceptability intuitions of the sort C&G elicited, which are *implicitly* grammatical (the possible exception aside), intuitions that are *explicitly* grammatical, ambiguity intuitions, coreference intuitions, and so on. I think that my modest theory applies to them all.

Next, I think the mistake is responsible for an apparent inconsistency in the views of C&G. They see their experiment as showing that linguists' *acceptability* intuitions *are not* more reliable than the folk's. However, they are prepared to accept that linguists' *grammaticality* intuitions *are* more reliable. They simply find the latter claim unsurprising and uninteresting (p. 724). Here is an argument to show that if the linguists' grammaticality intuitions are more reliable, then so are their acceptability ones.

Judgments of the form 'This expression is (un)grammatical' are indubitably grammatical. Linguists and the many folk having the word 'grammatical' are able to make such judgments in the sort of immediate and unreflective way that counts as intuitive. Let G be whatever it is in a person that would cause such an intuitive grammaticality judgment. Now, as everyone agrees, many things could cause an acceptability intuition other than G: for example, whatever causes a judgment of truth value or of etiquette could play a role. Let X be one of these other possible causes, Y, another, and so on. The argument then runs as follows:

- (1) Linguists are more reliable than the folk in their grammaticality intuitions, judgments caused by G.
- (2) Acceptability intuitions are caused by one or more of G, X, Y, and
- (3) Linguists are at least as reliable as the folk in their intuitions about matters other than grammaticality, judgments caused by X, Y, or
- (4) So, linguists are more reliable than the folk in their acceptability intuitions.

C&G are prepared to accept premise 1. Conclusion 4 goes against what their experiment seeks to show. If they are to avoid this inconsistency they must reject premise 2 or 3. 3 is hard to reject. So the issue comes down to 2. Now C&G probably differ from me over the possible causes of acceptability intuitions. I reject VoC and so my position is that these intuitions are *never* caused by the underlying linguistic competence. C&G are at least dubious about my position. But this probable difference between us over possible causes is irrelevant to 2. For, the only way to avoid 2 is to deny that G, whatever it may be, is *ever* a cause of a person's acceptability intuitions. But this denial cannot be sustained. On the one hand, if G would cause a person's intuition that, say, a word salad was ungrammatical, then G would very likely cause her intuition that this salad was unacceptable. On the other hand, if G would cause her intuition that, say, 'It is raining' was grammatical, then it is very likely that, however acceptable this sentence was to her otherwise, she would not have the intuition that it was acceptable were it not for G. So G is a cause of acceptability judgments.

I turn now to C&G's experiment. I think that their mistaken view of the difference between acceptability and grammaticality judgments also misleads them about the significance of their experimental results for my position.

3.2 The experiment

C&G tested four groups with differing educations and backgrounds but all having at least a college-level education. The results were:

the judgments of three groups—the linguists (identified as LOTS), the subjects with at least one class in theoretical syntax (SOME), and the subjects with no syntax experience, but with cognitive science background (LITTLE)—showed equally high intra-group average correlation values. However, the subjects with no experience in cognitive science of any kind (NONE) were *not* well correlated with one another (p. 729)

As for inter-group correlations [...], the same three groups (LOTS, SOME, and LITTLE) were highly correlated with one another and more correlated with each other than with the NONE group. (pp. 730–1)

They argue, plausibly, that 'greater consistency indicates greater reliability' (pp. 731–2). So they see their results as posing two problems for my theory of linguistic intuitions:

- (1) the results suggest that, contrary to what Devitt's claim would predict, a subject's knowledge of and experience in linguistics do not significantly affect performance, given a minimum level of relevant experience. The judgments of professional syntacticians were highly correlated with those of relative beginners in cognitive science. (p. 732)
- (2) why is there a divide at all between group NONE subjects and the rest? (*ibid*)

I do find these experimental results a little surprising. Still, it is *not obvious* that they count against my view of linguistic intuitions. I shall respond to the two problems in turn.

Response to (1): (a) The results seem to show that the linguistic intuitions of LOTS, SOME, and LITTLE were equally good. But they could really show this, of course, only if the intuitions elicited from these three groups *are about the same thing*. I think there is a reason for doubting that they are. On the basis of the words in the experimental instructions, particularly 'sounds good', I claimed that the intuitions were likely to be about grammaticality. My claim was quite general, but I mentioned a possible exception. What I had particularly in mind were the intuitions of LOTS. In Section 1.3, I mentioned the technical notion of acceptability utilized by Chomsky in his theory of performance: according to this notion, acceptable utterances are ones, briefly,

'that are perfectly natural and immediately comprehensible' ([1965], p. 10). I introduced the term 'C-acceptability' for Chomsky's notion. Now a plausible hypothesis is that the linguists who constituted LOTS were so familiar with Chomsky's influential distinction between grammaticality and C-acceptability that they largely ignored the ordinary meaning of the instructions and took the experimenter to be seeking judgments of C-acceptability. In contrast, the members of LITTLE, who know nothing of Chomsky's distinction, had no choice but to be guided by the ordinary meaning and so are likely to have judged grammaticality. Yet, as Chomsky insists, C-acceptability is not grammaticality. So if the hypothesis about LOTS is right, the intuitions of LOTS and LITTLE were not about the same thing. And what about the intuitions of SOME, whose members have done at least one course in syntax and hence may also have been influenced by Chomsky's distinction? It is hard to know. So, we can't be confident that a comparison of LOTS, SOME, and LITTLE could show anything about my theory of linguistic intuitions.

(b) Even if we were to set aside this serious concern, the comparison might not count against the theory. To show this I need to clarify the theory. It is a consequence of my *general* theory-laden view of intuitions that 'we should trust a person's intuitions [...] to the degree that we have confidence in her empirically based expertise about the kinds under investigation' ([2006a], p. 492, [2006b], p. 104). Sometimes the folk are as trustworthy as anyone in an area but where there is a science in an area they are not. We should then prefer the intuitions of the scientists because they are the most expert; their intuitive observations are laden with better theories. So we should prefer the paleontologist's intuitions about old bones, the physicist's about certain physical facts, and the psychologist working in the relevant cognitive area ([2006a], pp. 492–3, [2006b], pp. 104–5). Notice, however, that it does not follow from this that the scientist in an area will have more reliable intuitions than others about *every* fact in the area. It does not follow, for example, that the paleontologist who is better than the folk at identifying something as a pig's jawbone will also be better at identifying something as a skull. Perhaps educated folk would do just as well because they have *enough* expertise. What does follow from my view is that the more expert a person is in an area, the better the person's theory, the wider her range of reliable intuitions in the area.

Applying my general view of intuitions to linguistics leads to the claim, quoted by C&G, that 'the intuitions that linguistics should mostly rely on are those of the linguists themselves because the linguists are the most expert' ([2006a], p. 499, [2006b], p. 111). But it is important to note that it does not lead to the claim that linguists have more reliable intuitions about

every linguistic fact. Indeed, the quoted claim is preceded by the following one about the linguistic intuitions of ordinary speakers:

we can often be confident that such intuitions of normal educated speakers are right. We often have good reason to suppose that these core judgments of folk linguistics, partly reflecting ‘the linguistic wisdom of the ages’, are good, though not of course infallible, evidence for linguistic theories. ([2006a], pp. 498–9, [2006b], p. 110)

We should prefer the linguists’ intuitions particularly ‘when we get beyond the simple cases to theoretically interesting ones like “The horse raced past the barn fell” and “Who do you wanna kiss you this time?”’ ([2006a], p. 499, [2006b], p. 111).³¹ The firm prediction of my view is that the more expert a person is in linguistics, the wider his range of reliable linguistic intuitions.³²

In light of this, even if the intuitions of LOTS, SOME, and LITTLE were all about grammaticality, they *might not* count against my theory. C&G, in considering the other problem, (2), wonder whether ‘subjects in group NONE have not passed a threshold for theoretical linguistic knowledge that the others have, and passing this threshold renders one so reliable an indicator of syntactic reality that further improvements in theoretical knowledge have little or no measurable effect’. They dismiss this idea as implausible. Noting the only difference between NONE and LITTLE, they ask rhetorically: ‘why should taking a course in some other area of cognitive science enable one to cross such a significant threshold concerning linguistic knowledge?’ (p. 732) It does indeed seem rather implausible that it should. Still, the idea that the *other* three well-educated groups have all passed that threshold for judging the grammaticality of the sentences/strings tested strikes me as a plausible response to (1). To get a significant difference among these groups, assuming that they were all judging grammaticality, we may have to test them with more difficult sentences/strings.

Response to (2): I think C&G may well be on the right lines in their answer to (2). They suggest that a lack of experience of these types of experiments may explain the different performance of NONE. ‘In particular, the lack of such experience might prevent the subject from interpreting the notion of acceptability as it is intended’ (p. 732). But, because of their mistaken view of the distinction between acceptability and grammaticality intuitions, C&G wrongly think that I must disagree with their suggestion. On my view, the small

³¹ The experiments I cite at this point, (Spencer [1973] and Gordon and Hendrick [1997]), though open to criticism as C&G point out (pp. 727–8), illustrate the sort of nonsimple cases where we might expect linguists’ intuitions to differ from those of the folk.

³² Note that this prediction is quite compatible with another one: that linguists with different theories about some theoretically interesting cases may have different intuitions about them. This is an unavoidable consequence of the theory-laden nature of intuitions in linguistics as everywhere else ([2006a], p. 504, [2006b], p. 115). And it is a reminder that we should not rest with mere intuitions as evidence.

educational difference between NONE and LITTLE should make no significant difference in their acceptability intuitions *insofar as those are grammaticality intuitions*. I emphasize that there is always a possibility that acceptability intuitions will *not* be grammaticality ones (Section 1.3 above). Given what C&G report of their informal conversations with NONE subjects, it seems plausible to think that the intuitions of those subjects were often not grammaticality ones. Indeed, what could the failure to interpret ‘the notion of acceptability as it is intended’ *be* other than the failure to interpret it as grammaticality? The only point of experiments like this, in my view, is to elicit grammaticality intuitions. It looks as if C&G succeeded with LITTLE but, surprisingly, failed with NONE. If so, that explains the difference.³³

I remain a bit puzzled by these experimental results. However, they are not a major blow to the view of linguistic intuitions that I have presented. And what is the alternative to a view along those lines? Not, I have argued, VoC.

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³³ Given the educational level of the subjects in this experiment, we can presume that they had not only a concept of grammaticality but also the word ‘grammatical’. So, in my view, it would have been better to ask the subjects whether sentences were ‘grammatical in *your* language’ rather than whether they ‘sound good or not *to you*’ (before going on to emphasize that the question is not about ‘the rules of “proper English”’). This would have lessened worries about how subjects interpreted the question and hence yielded a better test of my theory of linguistic intuitions.

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