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LINGUISTIC INTUITIONS ARE NOT “THE VOICE OF COMPETENCE”

Philosophical Methodology: The Armchair or the Laboratory?

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1. Introduction

How should we go about finding the truth about a language? The received answer in linguistics gives a very large role to the intuitive linguistic judgments of competent speakers about grammaticality/acceptability,¹ ambiguity, coreference, and the like. Thus, Noam Chomsky claims that ‘linguistics ...is characterized by attention to certain kinds of evidence...largely, the judgments of native speakers’ (1986: 36). Carson Schütze remarks:

Throughout much of the history of linguistics, judgments of the grammaticality/acceptability of sentences (and other linguistic intuitions) have been the major source of evidence in constructing grammars (1996: xi).

Liliane Haegeman, in a popular textbook, goes even further, saying that ‘all the linguist has to go by...is the native speaker’s intuitions’ (1994: 8).² This raises a question: *Why* should we think that these intuitive judgments are good evidence for a syntactic theory of the speaker’s language, good evidence for its grammar? What could be their source that would make them reliable?³

(a) In a discussion of linguistic intuitions in *Ignorance of Language* (2006a: ch. 7; see also 2006b), I took the received Chomskian answer to be that these intuitions are “the voice of competence” (“VoC”). This is the view that 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: 96)

¹ Linguists tend recently to make much of the distinction between intuitions about grammaticality and acceptability. I argue that ordinary acceptability intuitions are evidence only insofar as they are grammaticality intuitions (2010b: 839-44); see Gross and Culbertson 2011 for a response.

² Despite the received view, John Collins (in the guise of “Ling”) talks dismissively of “the absurd idea that we are after speaker/hearers’ explicit propositional judgments on the linguistic status of strings” (2006: 480). For discussion, see Devitt 2010b: 838-9.

³ Mark Textor (2009) hankers after non-judgmental “linguistic seemings” as evidence; see also Gareth Fitzgerald (2010: 138); Barry Smith (2013). I argue (2010a) that there are no such seemings.

Competence not only plays the dominant role in *linguistic usage*, it also provides informational content to *metalinguistic intuitions*.⁴ Those intuitions are indeed, “noise” aside, the voice of competence. That is why they are reliable.

(b) I argued that VoC was wrong (pp. 100-19). Instead, I urged that intuitive judgments about language, like intuitive judgments in general, “are empirical theory-laden central-processor responses to phenomena, differing from many other such responses only in being fairly immediate and unreflective, based on little if any conscious reasoning” (p. 103). Although a speaker’s competence in a language obviously gives her ready access to the *data* of that language, the data that the intuitions *are about*, it does not give her ready access to the *truth* about the data; the competence does not provide the *informational content* of the intuition. In this respect my view is sharply different from VoC. And it is sharply different in another respect: it is *modest*, making do with cognitive states and processes we were already committed to. So, following Mark Textor (2009), let us call it “the Modest Explanation” (“ME”).⁵

Both (a) and (b) have been criticized. My main aim in this paper is to defend (a) and (b) from these criticisms. But first we should consider the methodological significance of this debate about linguistic intuitions.

2. Methodological Significance for Linguistics

It needs to be noted, first, that claims like those by Chomsky and Haegeman are exaggerations in two respects. (i) These claims are clearly intended to be statements about the evidential role of the intuitive judgments of *ordinary* native speakers, *folk* intuitions. Yet, as a matter of historical fact, linguists have relied much more on their own intuitions than on those of the folk. This has often been noted and has become the subject of much concern in recent years (Schütze 1996; Gordon and Hendrick 1997; Sorace and Keller 2005, Featherston 2007; Myers 2009). (ii) Furthermore, even though the debate about linguistic methodology is dominated by attention to the role of intuitions – far too much so, in my view (2006a, 98-100) - the role of usage as a source of evidence is often acknowledged.⁶ Thus evidence is found in the corpus, elicited production, reaction time studies, eye tracking, and electromagnetic brain potentials.⁷

So grammar construction is not solely reliant on native speakers’ intuitions for evidence. But the degree to which it should be so reliant clearly depends on whether VoC or (something

⁴ Some discussions of VoC are vitiated by a failure to keep these potential roles of competence sharply distinct; see particularly Fitzgerald 2010, discussed in my 2010b; also Collins 2006: 480; 2008a: 31; Textor 2009. It is trivial that competence (along with some other factors) is causally responsible for linguistic usage. But that is not what VoC is about. It is about competence as a source of metalinguistic intuitions.

⁵ My account of linguistic intuitions in *Ignorance* is misleading in two respects (and contains a minor misstatement); see my 2010a, pp. 254-5 for clarification.

⁶ It is acknowledged by Haegeman (1994: 10), despite her earlier claim about “all the linguist has to go by”. And it is acknowledged by Andrew Radford (1988: 24) after an extensive discussion of the evidential role of intuitions.

⁷ See Krifka 2012 for a helpful summary of the evidence that linguists use.

like) ME is right. Thus, if VoC is right and competence really does produce these intuitions, then of course the intuitions should be the pre-eminent source of evidence for grammars: “noise” aside, they must be true. On the other hand, if VoC is not right and hence, presumably, (something like) ME is right, then *intuitions should surely lose that pre-eminence: other evidence should come to the fore*. Indeed, the extent to which the folk are reliable about their language at all becomes an open question. At least that reliability needs to be thoroughly tested against other evidence.

We should note further that if VoC is right, the frequent criticism of the common practice of relying for evidence largely on the intuitions of linguists rather than folk is appropriate. VoC gives no reason to prefer the intuitions of native speaking linguists to those of native speaking folk. Indeed, we should prefer those of the folk because those of the linguists may be prone to a sort of noise that lessens their credibility: theoretical bias. In contrast, ME *supports* the common, but criticized, practice. For, according to ME, intuitions are like ordinary “observation judgments” in being “theory laden”. The antipositivist revolution in the philosophy of science, led by Thomas Kuhn and Paul Feyerabend, drew our attention to the way in which even the most straightforward judgments arising from observational experiences may depend on a background. We would not make the judgments if we did not hold certain beliefs or theories, some involving the concepts deployed in the judgments. We would not make the judgments if we did not have certain predispositions, some innate but many acquired in training, to respond selectively to experiences.⁸ In light of this, when we do use intuitions as evidence, we should prefer those of the linguists to those of the folk because linguists have the better background theory and training; they are more expert (2006a: 111, 115).⁹

3. Methodological Significance for the Philosophy of Language

Just as linguists take native speaker’s intuitions to be the main source of evidence for syntactic theories, philosophers of language take them to be so for semantic theories, for example, for theories of reference.¹⁰ And philosophers, like linguists, have typically relied on their own intuitions rather than the folk’s.¹¹ However, there is a significant difference between the disciplines. Whereas linguists typically give some role to evidence other than these intuitions, philosophers seem not to: they do not acknowledge other evidence and their practice seems to involve only appeals to intuitions.¹²

This practice of relying on intuitions, just like the similar practice of linguists, raises a question: Why suppose that these intuitive judgments about the semantic properties of linguistic expressions are good evidence for a semantic theory? What could be their source that would

⁸ So “theory” in “theory-laden” has to be construed *very* broadly to cover not just theories proper but also these dispositions. For more on this theory ladenness, see Devitt 2011c: 19.

⁹ For an exchange on this issue, see Culbertson and Gross 2009, Devitt 2010b, Gross and Culbertson 2011.

¹⁰ For evidence of this, see Devitt 2012: 554-5.

¹¹ This practice has been challenged by “experimental philosophers”; see Machery et al 2004.

¹² I say “seems” because I think that, in fact, some evidence comes from observations of usage (2011c: 25; 2012: 563).

make them reliable? Philosophers seem to think that these intuitions are a priori, as Michael McKinsey points out (1987: 1). But appeals to the a priori are always dubious, in my view, and are particularly so about semantic properties (1994, 1996, 1998, 2011a). Might philosophers have a more respectable justification for their practice? Stephen Stich has an interesting suggestion: philosophers might be implicitly extending the linguists' VoC to semantics, in particular, to the theory of reference (1996: 40). Philosophers may think that a speaker's underlying competence provides her not only with syntactic intuitions but also with semantic ones.

ME is a rival to this VoC view of semantic intuitions just as it is to the VoC view of syntactic intuitions. And, the significance of this rivalry for the philosophy of language is analogous to that for linguistics. In particular, if VoC is not right and (something like) ME is, then, insofar as we use intuitions as evidence, we should prefer those of the more expert philosophers to those of the folk.¹³ *Much more importantly, we should be looking for other evidence for semantic theories.* And we should be using that other evidence to assess the reliability of intuitions.

This raises an interesting question: *What other evidence?* As noted, philosophers, unlike linguists, do not acknowledge any other evidence. I argue that they are very wrong not to. There is in fact lots of other evidence and philosophers should take ideas from linguists in trying to find it. In particular, philosophers should seek evidence in *usage*. They should seek direct evidence in linguistic reality itself rather than simply relying on the indirect evidence of intuitions about that reality (2011b,c).

A major source of such evidence is the corpus, the linguistic sounds and inscriptions that the folk have produced and are producing as they go about their lives. I illustrate what a rich source of evidence the corpus could be for the theory of reference with a vignette to be found, ironically, in an experiment aiming to test folk intuitions about reference in Kripke's famous Gödel case (Machery et al 2004). I point out that the experimenters' own uses of 'Gödel' in the vignette, are inconsistent with what (standard) description theories would predict (2011c: 27-8).

There are well-known difficulties in using the corpus as evidence. Fortunately, linguists have shown that we don't have to rely on the corpus: we can *induce* usage from competent speakers in experimental situations using the technique of "elicited production". Experimental situations "are designed to be uniquely felicitous for production of the target structure" (Thornton 1995, p. 140). I proposed an easier way of eliciting production: rather than *creating* situations in which we see what people say or understand, we can *describe* such situations and see what they say or understand about them (2006a: 99). I recently argued that this method can be a ready source of evidence for theories of reference and is the way forward in experimental semantics (2011b: 430-2; 2011c: 29-30). Wesley Buckwalter and I have begun conducting experiments of this sort (2011).

¹³ This supports "the Expertise Defense" of traditional philosophical methodology against the findings of Machery et al 2004; see Devitt 2011c: 14-26.

In sum, whether VoC or (something like) ME is correct is of great methodological significance for the study of language by both linguists and philosophers.

I turn now to my two main issues: (a) Do Chomskian linguists actually hold VoC? (b) Is VoC really false?

4. Is VoC the Received View in Chomskian Linguistics?

4.1 Background

I confidently attributed VoC to Chomskian linguistics at the beginning of *Ignorance* (2006a: 4) and later supported that attribution with five quotes and four further citations (p. 96). I shall discuss the five quotes in a moment (4.2), but I want to start the case for the attribution with an expanded version of the first of those quotes. This striking passage from Chomsky has always seemed to me to be as clear a statement of VoC as one could want:

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". (1986: 270)

I had made the attribution of VoC years earlier (Devitt and Sterelny 1989: 521) without hearing complaint. Stephen Stich has been making it for decades (see, e.g., 1996: 40). The VoC view of linguistic intuitions is the explicit inspiration for the “theory-theory” explanation of folk psychological judgments (as I noted, 2006a: 204n). It never occurred to me that the attribution of VoC would be controversial. Yet it has turned out to be. It has been controverted by three knowledgeable philosophers: John Collins (2008a: 17-19), Gareth Fitzgerald (2010), and Peter Ludlow (2011: 69-71). I have responded to Fitzgerald already (2010b: 845-7). In brief, despite resisting the attribution, Fitzgerald’s description of the “orthodox” Chomskian view is in fact VoC! And I would argue much the same about Collins. Ludlow’s discussion of the attribution is by far the most thorough to date and I shall respond to it here.

But first let me draw attention to others who seem to go along with the attribution. (i) Barry Smith, in a critical response to *Ignorance*, states what amounts to VoC: “Unconscious, information-bearing states of the language faculty give rise to conscious knowledge that is immediately reflected in the speaker’s intuitive linguistic judgements” (Smith 2006: 443; and see pp.451, 454). (ii) Similarly, Mark Textor, in talking of intuitions being “derived from mentally represented or tacitly known grammatical principles” like “a theorem [being] derived from already established truths” (2009: 396). (iii) We shall see in section 6 that Georges Rey embraces the attribution. (iv) Both Jaakko Hintikka (1999) and Timothy Williamson (2007) attribute VoC to the linguists in the course of critical looks at the use of intuitions in philosophy.¹⁴

¹⁴ Maynes and Gross 2013 contains a subtle discussion of the position of linguists on intuitions.

Here is a powerful reason for thinking that Chomskian linguists do hold VoC: *How else* can we explain the great evidential weight that linguists attach to intuitive judgments, particularly to the judgments of ordinary folk? The only explanation seems to be that linguists think that folk, simply in virtue of being native speakers of a language, have a privileged access to the truth about that language.

It would be nice, of course, to have some recent statements of VoC by linguists themselves. These are hard to find. The best I have come up with is talk of “the ‘true’ acceptability response generated by the cognitive system of language” (Sprouse and Almeida forthcoming: 3; see also: 17, 21)). Indeed, so far as I can see, linguists hardly ever discuss the source of intuitions *at all*, presumably feeling that they have better things to do, like constructing grammars. Linguists mostly seem to just *presuppose* VoC without even stating it explicitly. There seems to be little if any attention to the key epistemological question: *Why* are these metalinguistic intuitions good evidence in grammar construction? This is surprising given the importance attached to the intuitions as evidence. It is particularly surprising given the already-mentioned concern about relying on the intuitions of linguists rather than those of the folk (Schütze 1996). This concern is exemplified, for example, in the following recent papers: Sorace and Keller 2005, Featherston 2007, and Myers 2009. Yet none of the papers raises the key epistemological question about these intuitions.

VoC is not often stated. More interestingly, to my knowledge, it has never been stated in the sort of detail that could make it a real theory of the source of intuitions. Furthermore, again to my knowledge, no argument has ever been given for it (until Rey’s, considered in section 6).

What are we to make of this lack of interest in articulating, let alone arguing for, VoC? I think it may stem from the received Chomskian “psychological conception” according to which the grammar for a language is about a cognitive system in the language faculty of its speakers. It follows from this conception that the rules (and principles) of the true grammar are embodied in a speaker’s mind. A lot of work would still have to be done to get from this to an adequately detailed VoC: *How* do the embodied rules yield a speaker’s metalinguistic intuitions? Still, it may be tempting to think that the embodied rules *must* be responsible for her intuitions, even *sans* details. Tempting or not, VoC does still need the details. Aside from that, this route to VoC faces a serious problem, in my view: the psychological conception is false. I have argued against it and in favor of a “linguistic conception” according to which, a grammar is about a nonpsychological realm of linguistic expressions, physical entities forming a symbolic or representational system (2003; 2006a, ch. 2; Devitt and Sterelny 1989).¹⁵ It is then an open question whether competence in a language is constituted by the embodied rules of the language.

4.2 Ludlow

In his recent book, *The Philosophy of Generative Linguistics* (2011), Ludlow claims to reject VoC: “I want to stress (in partial agreement with Devitt) that such a view of linguistic

¹⁵ This rejection has received a deal of criticism (some of it very harsh): Antony 2008; Collins 2007, 2008a,b; Dwyer and Pietroski 1996; Laurence 2003; Longworth 2009; Matthews 2006, Pietroski 2008; Rattan 2006; Rey 2006a, 2008; Slezak 2009; Smith 2006, 2013. Devitt 2006c, 2008a,b,c, and 2009 are recent responses to some of these criticisms.

intuition is mistaken” (2011: 69). But he is very dubious, at least, of my attribution of VoC to linguists. He lists (p. 70) the five quotes that I gave in support of the attribution (2006a: 96) - but without the four further citations I also offered - and discusses each of them. He writes as if this discussion shows that the quotes do not in fact support my attribution of VoC. I have been alarmed to discover people who find Ludlow’s discussion convincing. Yet, with one exception, his discussion does not undermine my evidence at all. Indeed, it looks as if Ludlow does not really understand VoC. This may be because he never attends to my actual definition (sec. 1 above) but rather responds negatively to my (somewhat playful) *name* for the doctrine - “the voice of competence” - and my (somewhat provocative) *name* for the special sort of access to linguistic facts claimed by the doctrine - “Cartesian access” (2011: 70).

1. I shall start (though Ludlow does not) with my first Chomsky quote:

it seems reasonably clear, both in principle and in many specific cases, how unconscious knowledge issues in conscious knowledge... it follows by computations similar to straight deduction. (1986: 270)

This quote is a shortened version of the “striking passage” that I have just claimed to be as clear a statement of VoC as one could want (sec. 4.1). Ludlow responds:

The first Chomsky quote goes to the question of conscious knowledge, but I would take this to be knowledge of linguistic facts or phenomena (what I earlier called “S-facts” - not knowledge of the rules which give rise to the linguistic phenomena. This is entirely consistent with the picture I am advocating. (p. 68)

What Ludlow means by “S-facts” are “surface facts...like this: ‘Who did you hear the story that Bill hit’ is not acceptable” (p. 52). So judgments of these S-facts simply are the intuitive judgments that VoC is about. So Ludlow is in fact construing Chomsky as endorsing VoC! On Ludlow’s construal, the intuitive judgment - conscious knowledge of an S-fact - “follows by computations similar to straight deduction” from the unconscious knowledge of the linguistic rules and principles. So why does Ludlow resist the attribution of VoC to Chomsky? The reference to “knowledge of the rules” is revealing. It looks as if Ludlow takes VoC to require speakers to have conscious knowledge of the rules (see also pp. 66, 69). But this would be a preposterous thing to suppose and is a bad misunderstanding of VoC. As I point out, the explanation of intuitions offered by VoC “does not suppose that the speaker has Cartesian access to the linguistic rules, just to the linguistic facts captured by the intuitions” (2006a: 96n). According to VoC, the speaker has access to *the results* of a derivation from underlying rules not to the rules themselves.

2. Among the five quotes, the one that Ludlow discusses first is:

Our ability to make linguistic judgments clearly follows from our knowing the languages that we know. (Larson and Segal 1995: 10).

Ludlow has this to say:

is there anyone who could possibly disagree with this quote? Surely the linguistic judgments that I make follow from my knowing the language that I know. If, for example, I knew Japanese I would have different judgments. (2011: 70)

I disagree with the Larson and Segal quote! And I do so because “follows from” implies VoC: it implies that Ludlow’s judgments are *deduced from* his knowledge of English; see the Chomsky quote above. And this is why VoC is mistaken. We should all accept, of course, that knowledge of English provides ample *data* for linguistic judgments about that language and not some other language like Japanese. But the data for judgments are not judgments (sec.1). So, once again, we have Ludlow endorsing a statement of VoC that he does not recognize as such. It really does look as if he misunderstands VoC.

3. Ludlow next considers my second Chomsky quote. Ludlow gives good reason for thinking that the quote does not support my attribution of VoC.¹⁶ It is the one exception I mentioned.

4. Now consider this quote from Jerry Fodor:

We can use intuitions to confirm grammars because grammars are internally represented and actually contribute to the etiology of the speaker/hearer’s intuitive judgments. (1981: 200-1)

Again we seem to have a quote that clearly expresses VoC. But not to Ludlow:

Likewise, when Fodor says that grammars contribute to the etiology of the judgments, I take this to mean that they contribute in this way: they give rise to the facts, and those facts are the objects of our linguistic judgments. (2011: 70)

This construal is obviously wrong. First, for someone like Fodor who believes in an internally represented grammar, it would be too obvious to be worth saying that it gives rise to the linguistic facts. Second, and more important, giving rise to those facts would not even appear to explain what Fodor claims to be explaining: why “we can use intuitions to confirm grammars”. It would not explain *why intuitions about those linguistic facts are likely to be true* and hence *why they are evidence for* grammars. Fodor clearly thinks that the explanation of this evidential role

¹⁶ Ludlow has considered my attribution of VoC before in a review of *Ignorance*. He there discusses, along much the same lines as in his book, this second Chomsky quote and the Larson and Segal quote. He does not discuss the other three more telling quotes. Nonetheless, he declares that *none* of my quotes “speak to the point in question” (2009: 400). This blatantly false pronouncement is all he offers in support of one of his three sweeping criticisms of my book: “Devitt is not particularly charitable in his interpretation of what linguists have to say about the nature of their enterprise” (p. 399). It is remarkable that Ludlow should make this criticism in a review that is about as uncharitable as it gets; plain nasty really. For comments on the review, see my “Responding to a Hatchet Job” at <http://devitt.commons.gc.cuny.edu/>

is that the internally represented grammar *gives rise to those intuitions*. If we simply take Fodor to mean what he says we have a commitment to VoC.

5. Finally, consider this quote:

[A speaker's judgments about the grammatical properties of sentences are the result of] a tacit deduction from tacitly known principles. (Graves et al. 1973: 325)

Ludlow has a truly remarkable response to this:

The passage from Graves et al. is completely misrepresented by Devitt's editorial addition. Graves et al. are not discussing linguistic *judgments*, but rather tacit knowledge of linguistic phenomena – of what I have called explanatory facts. (2011: 70)

First, Ludlow provides no evidence for this charge of complete misrepresentation: he simply makes a pronouncement. Second, the charge is quite false, as even the most cursory look at Graves et al would show. Ludlow's claim that Graves et al are discussing "tacit knowledge of... explanatory facts" is doubly wrong: (a), the knowledge discussed is explicit not tacit; and, (b), it is not of Ludlow's "explanatory" facts.

I'll start with (b). Ludlow contrasts "explanatory" facts with the above-mentioned "surface" facts. "Explanatory" facts "incorporate information about the explanations for...surface linguistic facts – for example this: 'Who did you hear the story that Bill hit' is unacceptable *because it violates subadjacency*" (2011: 52; emphasis added). Contrary to what Ludlow claims, in the passage I quote Graves et al are quite obviously discussing knowledge of "surface" not "explanatory" facts. Indeed, only a few lines before, Graves et al give six examples of the knowledge that concerns them. All are of "surface" facts. Here is one: "English speakers know that...

(4) John overestimated himself
...[is a] well-formed, grammatical sentence" (Graves et al 1973: 325).

So much for (b). Turn now to (a). Graves et al describe how the field linguist can deduce his judgments of "the grammatical properties and relations of novel sentences" from explicitly formulated principles (p. 324). Their idea then is that this "paradigm of explanation should be transferred" to ordinary speakers:

The untutored speaker has the same information, in *explicitly* knowing that certain sentences have certain grammatical properties. But we can assume that the speaker, unlike the linguist, does not explicitly know the principles from which it follows that these sentences have these grammatical properties. Why not then extend the paradigm of explanation to the speaker, by assuming that the speaker performs the same deduction as the linguist, only tacitly, and thus that the principles in the deduction are tacitly known? (p. 325; emphasis added)

So, Graves et al are indeed proposing that a speaker's *explicit* knowledge of Ludlow's *surface* facts is deduced from tacitly known principles. They are proposing VoC, just as I said. Indeed,

this is the most detailed presentation of VoC that I know of. (The difference between “explicit knowledge” and “judgment” is of no significance here, of course. Graves et al also talk of judgments in this context (pp. 324, 327-8), as is appropriate.) So, my “editorial addition” is in order and the misrepresentation is all Ludlow’s.

Ludlow’s persistent and extraordinary misrepresentations of the evidence leaves untouched my recent claim that evidence shows, “overwhelmingly”, that the orthodox Chomskian view of metalinguistic intuitions is VoC. Indeed, if Chomskians did not hold VoC, they would have no view of the source of linguistic intuitions (2010b: 847).

One is left wondering what Ludlow’s view of linguistic intuitions really is. His endorsement of the first Chomsky quote and the Larson and Segal quote suggests that he subscribes to VoC. Yet he resists the obvious VoC interpretation of both the Fodor quote and the Graves et al quote, which suggests that he doesn’t subscribe. Further investigation adds to the mystery. A few pages later he claims that “linguistic judgments are no different than judgments of experts with regard to a theoretical apparatus in the lab”, for example, chemical judgments (2011: 76). This is just the sort of comparison I make in urging ME (2006a: 103-11) and seems quite at odds with VoC. Yet a later discussion of a “hypothetical character” that he cutely names “Michael” suggests adherence to VoC. “Michael has a grammar as part of his cognitive architecture” but “is deeply confused” (2011: 92). The center of Michael’s alleged confusion seems to be that he does not believe that his acceptability judgments are the products of this cognitive grammar. In other words, Ludlow seems to think that to deny VoC is to be confused. All in all, I doubt that a clear and coherent view of the source of intuitive linguistic judgments can be found in Ludlow’s book. Certainly there is no argument for VoC.

5. Summary of the Case Against VoC

So, what is wrong with VoC? Why should ME be preferred? Before answering we should distinguish two possible versions of VoC.

What I call the “standard” version of VoC, implied by most of the quotes, is based on the “representational thesis” that linguistic rules (and principles) are *represented* in the language faculty. Speakers are then thought to derive their intuitive judgments from these representations by a causal and rational process like a deduction. Despite the evidence that this is the right way to understand VoC, it is not certain that linguists really do see intuitions as having their source in *represented* rules. And that representational thesis is rejected by many Chomskian philosophers of linguistics (e.g., Smith 2006; Collins, 2006, 2007, 2008a; Pietroski 2008; Slezak 2009). So, perhaps what I call the “nonstandard” version of VoC is the right interpretation: the intuitions are provided somehow by embodied but *unrepresented* rules (2006a, 96-8).

I offer several objections that count against both versions. Very briefly, these are as follows. (i) If competence really spoke to us, why would it not use its own language and why would it say so little (pp. 100-03)? (ii) There would be a disanalogy between the intuitions provided by the language faculty and by perceptual modules (p. 114). (iii) There would be problems arising from the differences between the intuitions of the folk and the linguists (p. 115). (iv) If rules in the language faculty provided the linguistic intuitions they would surely also

govern language use and yet there is empirical evidence that they don't do both (pp. 115-16). Aside from these objections in common, there are further, more important, objections to each version in particular.

The first further objection to the standard version is the already-noted point that we need details to turn this sketch into a theory. Graves et al have provided the most details in extending the explanation of the field linguist to the untutored speaker. This is an ingenious idea but how does the explanation really work? The intuition that a linguistic expression has certain properties is in the central processor. How can representations of principles in a faculty of the mind that is inaccessible to the central processor be brought to bear on a representation of that expression to “deduce” that intuition in the central processor? But my main objection to the standard version is its extreme *immodesty*. This immodesty lies in its commitment to the representational thesis (pp. 116-17). A major conclusion of *Ignorance* is that there is no significant evidence that linguistic rules *are* represented in the minds of speakers and, given what else we know, it is implausible to suppose that they are (p. 272).

The further objection to the nonstandard version is simple and apparently overwhelming: we do not have *any idea* how embodied but unrepresented rules might provide linguistic intuitions (2006a: 118).¹⁷ Not only do we lack the details needed for a plausible explanation but attention to other similar 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.

Since writing *Ignorance*, I have become aware of a body of developmental literature that provides persuasive empirical evidence against VoC.¹⁸ The evidence suggests that the ability to speak a language and the ability to have intuitions about the language are quite distinct, the former being acquired in early childhood, the latter, in middle childhood as part of a *general* cognitive development. 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: 97). It looks as if VoC is false.

6. Rey's Defense of VoC

¹⁷ “not much is known about the mechanisms implicated specifically in the formation of linguistic intuitions” (Maynes and Gross 2013: 9)

¹⁸ See particularly, Hakes 1980, Ryan and Ledger 1984, Bialystok and Ryan 1985, Bialystok 1986.

Georges Rey, like Chomsky and Haegman (sec. 1), gives a great deal of evidential weight to linguistic intuitions: they “provide not only excellent evidence, but, by and large, the only serious evidence we have” (2006b: 563). Only VoC could underlie this extreme – and, I argue (2006a: 98-100), quite false – view and so it is no surprise that Rey subscribes to VoC. But, unlike anyone else I know of, Rey has taken up the challenge of arguing for it (2006b; 2013).¹⁹ The version of VoC that he proposes is a “nonstandard” one.

Rey thinks, contrary to what I have just claimed, that there is “a perfectly scientifically respectable model” of how embodied but unrepresented rules might provide linguistic intuitions (2013: ##). His proposal stems from the view that language processing generates “structural descriptions” (“SDs”), metalinguistic representations of the syntactic and semantic properties of the expressions being processed. These processes are not, of course, in the central processor: they might be described as “sub-personal”, “sub-doxastic”, or “non-conscious”. Let us just say, in a theory-neutral way, that these processes are in a “non-central language system”. Rey’s suggestion is that *the central processor has access to these SDs*. On the basis of the information they provide, the central processor forms the speaker’s intuitive judgments.

I will give Rey’s presentation of his view in a moment. But first some preliminaries.

6.1 Preliminaries

The view that language processing generates SDs is widespread and so it is surely reasonable for Rey to adopt it. Nonetheless, I think the view is dubious. I argue that the speedy automatic language processes are fairly brute-causal associationist ones that do not operate on SDs (2006a: 220-43). If that is right, there are no SDs in the language system for the central processor to access. My brute-causal view is explicitly “tentative” but I claim that it is better supported by current evidence than the widespread one. However, my earlier criticism of VoC rested nothing on that tentative view and I shall rest nothing on it here. I will go along with the widespread view for the sake of argument.

In filling out his proposal, Rey makes much of what he, along with many others, sees as an analogy between intuitions about language and intuitions about vision. Rey emphasizes something that I am happy to accept: that vision *processing* is analogous to language *processing*. And I accept that *certain* vision intuitions are analogous to *certain* language ones: intuitions about *what is seen* are analogous to intuitions about *what is said*. Just as the non-central vision system provides the central processor with “the immediate and main basis for judging what is seen” so too does the non-central language system provide the central processor with “the

¹⁹ Robert Matthews (forthcoming) is about to take up the challenge. Rey is also, so far as I know, the only person to mount a thorough defense of another common view in linguistics: antirealism about linguistic entities (2006a,b; 2008). According to this curious view, the sounds, inscriptions, etc., of a language do not really have the phonological, syntactic and semantic properties that we naturally suppose them to have. I have responded to Rey’s argument (2006a: 184-92; 2006c: 597-604; 2008a: 221-9). My “linguistic conception” of grammars, mentioned in section 4.1, presupposes linguistic realism.

immediate and main basis for judging what is said”.²⁰ But intuitions about what is said are not the metalinguistic intuitions that concern VoC; they are not intuitions about grammaticality/acceptability, ambiguity, co-reference, and the like. The vision analogy does nothing to support the view that the latter intuitions are provided by the language system, hence nothing to support VoC (2006a: 112-3, 2010b: 850-2, 854).

In response, Rey claims that my discussion of the vision analogy

seriously misconstrues the projects of both the vision theorist and the Chomskyan linguist...neither the vision theorist nor the linguist confine the levels of representation deployed by those systems to merely [the tasks of delivering information to the central processor about what is seen or said]; nor are they committed to the *output* of these systems being couched as reports about the external world. Precisely what the output is of perceptual modules is, not surprisingly, an issue of subtle empirical detail,...it remains a vexing methodological problem with regard to both language and vision how to disconfound central from module-internal top-down processing,...The presumption of modularity theorists is that the output is fairly ‘shallow’,... (2006b: 563-4).

Rey’s first point reflects a misunderstanding. My discussion does not confine the level of representations deployed by those systems at all. Indeed, I clearly accept that the operations of these systems may involve all sorts of representations (2006a: 114). My claim is simply about what representations those systems *deliver to the central processor*.

Rey’s second point is more interesting. I do talk of the outputs of the vision and language “module”. This was a mistake. My argument that the vision analogy gives no support to VoC does not need such commitments to the architecture of the vision and language systems. In particular, the argument needs no commitment about the place of Fodorian modules within these systems, nor about how “shallow” the outputs of any such modules are, nor about any “top-down” processing the systems may engage in. The nature and workings of these systems are indeed “subtle empirical” matters. And our study of them is, as Fodor says, “in its infancy” (1998: 129). So it is wise to remain as noncommittal as possible about them. Fodor’s own discussion of them in his classic, *The Modularity of Mind* (1983), is explicitly tentative and speculative. Thus, in considering the outputs of his modules he is pulled two ways. On the one hand, his requirement that a module be “encapsulated” makes him think that its outputs are “shallow” (pp. 73-91). If they are shallow, I insist, they must be subject to further non-central processing to arrive at something suitable for delivery to the central processor (on which more below). On the other hand, Fodor’s criterion of “phenomenological accessibility” for outputs - “the visual processor...makes the deliverances of perception available as the premises of *conscious* decisions and inferences” (p. 136 n. 31) - pulls him away from shallowness (pp. 94-7, 136-7nn). My argument against VoC should have no commitment on such speculative matters.

²⁰ Rey (2006: 563-6; 2013: ##) is strangely bothered by my saying that it is the “task” of these systems to provide such information. Yet this is surely a harmless way of identifying what these systems do - their “functions” - *that makes us posit them in the first place*. It is quite compatible, of course, with these systems doing many other things as well. (For another example of such harmless talk, see Fodor on the “tasks” and “function” of the language parser; 1998: 131.)

Its only commitment concerns what these systems *make available to the central processor*, a “*bottom-up*” process. Whatever the outputs of one part of the non-central vision system to another part – say “early vision” to “higher-level vision” - what information does the system as a whole ultimately pass on to the central part of the mind that makes intuitive judgments?²¹ My argument assumes that the system provides a version of the scene that is the immediate and main basis for the conscious judging of what is seen, *and nothing else*. Now I take it that the only part of this that Rey could think a “subtle empirical” issue is “and nothing else”. Yet, so far as I can see, his discussion of vision, on which more below, provides no reason for thinking that the vision system does deliver anything else, in particular, no reason for thinking that it delivers *a description of the worldly vehicle* of the information, which would be an analogue of an SD. So I hold fast to my view that the vision analogy does not support VoC.

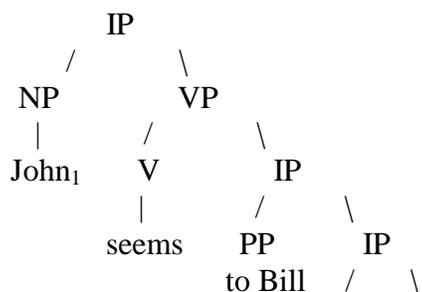
6.2 Rey’s Proposal

In presenting his proposed VoC, Rey allows “that linguists haven’t always been forthcoming or clear about just what sort of mechanism they presume underlies and ratifies native speaker intuitions” (2013: ##). This is too generous, given that linguists have said *almost nothing* about these mechanisms. And I think Rey may be too modest in thinking that his proposal is “at least implicit in several proposals that have been made” (p. ##).²² Here is his proposal:

linguistic intuitions...are presumed to be fairly directly caused by representations that are the output of...a language faculty...the faculty produces structural descriptions of various syntactic objects (phrases, sentences), and the intuitions are reliable insofar as those descriptions play a crucial role in the production of the intuitions....For example, it’s because

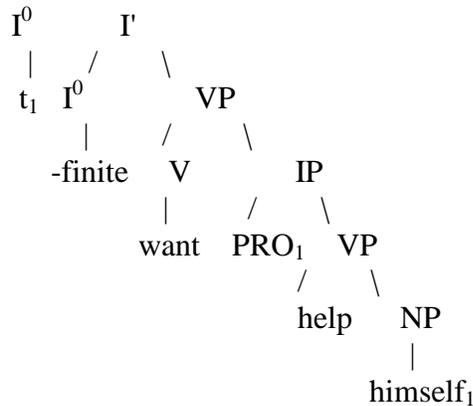
(1) John seems to Bill to want to help himself.

is produced by the language faculty with something like the following structural description:



²¹ So Rey’s point about the “shallow” outputs of early vision are not pertinent (2006a: 564; repeated in 2013).

²² He sees “intimations” of his view in Chomsky and Fodor. Furthermore, he claims that the view “is explicitly defended recently in Collins (2007: 421) [and] Fitzgerald (2010:§3)” (p. ## n. 17). There is nothing close to such a defense in either of the places cited: Collins does not even mention SDs; and Fitzgerald, rather than providing the details that a plausible VoC needs, tries to make us feel better about their absence, as I point out (2010b: 849-52).



that a speaker would spontaneously have the “intuition” that “himself” refers to John, not Bill...the structural descriptions...are activated by mechanisms of conscious attention which respond to the descriptions with whatever overt vocabulary is available, e.g., “No, himself can’t be John, but has to be Bill” (pp. ###)

There are two serious objections to this proposal. (I) *Why* should we suppose that the language system, in processing (1), makes this SD of (1) available to the central processor? (II) Even if it did, how would the SD’s information “fairly directly cause” the intuitions that are the concern of VoC? I shall consider these in turn. In so doing, I will adopt “the language-of-thought hypothesis”. This is not essential to the argument but it is very convenient for its presentation.

6.3 Serious Objection (I)

I assume that for a hearer to understand (1) is for her to come to have a mental representation of the message that *John seems to Bill to want to help himself* in the appropriate part of her central processor. She comes to have something that is a rough translation of (1). With this in mind, we can distinguish two possible versions of Rey’s proposal.

According to an “*online*” version, the language system’s contribution to the process of understanding *is* the deliverance to the hearer’s central processor of the above SD of (1); it delivers something that *describes* (1). The central processor uses this SD to come up with the required rough translation of (1), but it *can also* use it to form intuitions about (1); for example, the intuition that ‘himself’ co-refers with ‘John’. In contrast, according to an “*offline*” version, the language system contributes to understanding by itself delivering the rough translation of (1). So, on this version, the system delivers a representation of a worldly situation involving Bill and John whereas on the online version it delivers a representation of a representation of that situation.²³ Nonetheless, the central processor can access the SD offline and use it to form intuitions. So accessing the SD would be something *additional to* understanding, not essential to it.

²³ A common use/mention sloppiness in linguistics may make it easy to overlook this distinction: it is often unclear whether a discussion concerns SDs of expressions or the expressions themselves, as I note (2006a: 69-71). At one point Chomsky even claims the SDs *are* expressions of the language (1993: 1).

Each version of Rey's proposal is a very substantial one about the workings of the mind. Each needs evidence. I shall now try to demonstrate the *prima facie* implausibility of each. If this is right the evidential burden of the proposal is very great.

Consider the online version. I am going along with the widespread view that SDs play a rich causal role in the non-central language system. But playing that role in the process of understanding (1) does not entail, of course, that the system's final step is the deliverance of an SD of (1) to the central processor. Because of the causal role of SDs, language processing takes account of the syntactic and semantic information that SDs provide. As a result of what the system then delivers to the central processor, the mental representation involved in the hearer's final understanding of the utterance will have something like those syntactic and semantic properties and be a rough translation of (1). But this is not to say that it has those properties *because the system delivers an SD that describes those properties*. And it is highly implausible that it has those properties because of that.

The central processor is the home of a person's thoughts (propositional attitudes), including linguistic intuitions, that she can normally express in language. In the central processor she can move from one thought to another in practical and theoretical reasoning. Anything that is delivered to the central processor is, as Fodor says, "available to the cognitive processes that eventuate in the voluntary determination of overt behavior" (1983: 56), "available as the premises of *conscious* decisions and inferences" (p.136, n.31). *So if the SD of (1) is delivered to the central processor it must be available as a premise in an inference that leads to the final understanding of (1), an inference that takes the hearer from a description of (1) to a rough translation of (1)*. Here are three problems with this. (A) Phenomenology: ordinary hearers understanding (1) have no conscious awareness of its SD or of any inference from the SD to a translation of (1). (B) Given that it takes a few classes in syntax even to understand an SD, it is hard to see how ordinary hearers could use it as a premise even if they had access to it. (C) The speed of language processing is, as Fodor emphasizes, "mind-boggling": "the recovery of semantic content from a spoken sentence" is "very close to achieving the theoretical limit" (1983: 61). Given, as Fodor also notes, "the relative slowness of paradigmatic central processes" (p. 63), it is unlikely that such a significant part of understanding as moving from SD to translation is a central process

Consider now the offline version. According to this version, the deliverance of SDs to the central processor is not an essential part of understanding yet, nonetheless, these SDs can be made available offline and used to form metalinguistic intuitions. This version is spared problem (C): we seem to arrive at intuitions at a speed suitable for a central process. But the version still has a problem like (A): as we form our intuitions, it does not seem to us that Rey's "conscious attention" yields access to an SD like the one displayed. And the version has problem (B) arising from the difficulty in understanding an SD.

Finally, there is another problem for both versions. (D) Suppose that an SD of (1) *was* made available to the central processor and the hearer based her intuitive linguistic judgments on the rich information it contained. How come then that she does not have the intuition that, say, in (1) 'John' c-commands 'himself'? If her competence speaks to her in this way, "how come it says so little" (2006a: 101)? Presumably, this reticence would have to be explained as follows.

Online version: although the hearer accesses a full SD in understanding, she can use only part of its information in forming an intuition. Offline version: the hearer can access only part of the SD offline. What are we to make of these apparently arbitrary restrictions?

These problems are not decisive against Rey's proposal, of course, but they do put a heavy evidential burden on the proposal. Rey directs us to Fodor for the evidence:

Indeed, all the evidence suggests that speech perception involves not only understanding the *message* conveyed, but a highly modularized perception of *linguistic features of the speech vehicles* themselves (see Fodor *et al* 1974: 296-301, Fodor 1983:50-6, 86-93). (2013: ##)

Now I think that there is some truth in this claim. And I have no quarrel with much in the cited passages. Those passages contain lots of evidence and speculation about the nature of perceptual systems, particularly Fodor's modular "input systems", and about how much they contribute to what is ultimately perceived. But their evidential focus is not on what Rey needs to support VoC. He needs powerful evidence of one of the following: that these systems make their perceptual contributions by delivering online to a perceiver descriptions of the vehicles of these perceptual messages; or, that these descriptions in these systems can be accessed offline by perceivers. I don't find any evidence of *that*.

As Rey notes (##), Fodor holds that the language module "deliver[s] representations which specify, for example, morphemic constituency, syntactic structure and logical form" (Fodor 1983:93). This seems to be the online idea that an SD is delivered to the central processor in language processing. (i) But we need evidence that the language module has an SD-output that *specifies* those properties rather than an output that *has* those properties. Why would the final result of the module's analysis be a metalinguistic representation of (1) rather than a representation with similar properties to (1)? (ii) And if the module did output an SD, we would need evidence that the SD is delivered to the central processor rather than to some other part of the non-central language system. And the evidence would need to be powerful enough to overshadow problems (A) to (D). Given those problems, we should look rather for some other part of the non-central system to handle the transition from the module's SD-output to the rough translation of (1) that must feature in final understanding.

One wonders, of course, how *any* part of the language system might handle this transition from the metalinguistic level to the linguistic level. But it is a consequence of assuming that SDs play a causal role at all that this transition has to be made somewhere. (A) to (D) make it seem very implausible that the transition is in the central processor. So we should look for the transition being made elsewhere, if not in a Fodorian module, then in some other part of the language system.²⁴

²⁴ I speculate that the transition "would have to be some brute-causal process" and see this as a reason for supposing, contrary to the widespread view, that the automatic process of language understanding *as a whole* is brute-causal, with no role for SDs and hence no need for the transition (2006a: 224).

Elsewhere, Fodor is receptive to the idea of metalinguistic intuitions arising from offline access to SDs (1998: 127-41). But, once again, *no evidence is cited* that we have this access (beyond that we have intuitions allegedly based on those SDs).

Finally, I hasten to add that I am not resisting the familiar Fodorian claim that “you can’t help hearing an utterance of a sentence (in a language you know) as an utterance of a sentence” (1983: 52-3). Indeed, I emphasize that language processing is typically “automatic” (2006a: 209). And Rey has me wrong in supposing that I find remarks like the following “implausible”: “speech perception involves...a highly modularized perception of *linguistic features of the speech vehicles* themselves”; “we *hear* the utterances of a language we know in terms of [morphemic constituency, syntactic structure and logical form]” (##). In understanding (1), we hear it as having those linguistic features and not others *in that*, as a result of all the processing in the language system, we come up with a representation that *has* those features and not others; for example, it *has* a feature that takes ‘himself’ to co-refer with ‘John’ not ‘Bill. What I do find very implausible is that, in hearing (1) in this way, the central processor *thereby* has the informational basis for the intuitive judgment that ‘himself’ co-refers with ‘John’. We have been given no reason to believe that. Hearing an utterance in a certain way is one thing, judging that it has certain properties, another.

In sum, despite Rey’s bold claim about “all the evidence”, what he cites provides *no evidence at all* of what is needed: no evidence that the non-central language system provides SDs to the central processor. Still, we might sometime get some evidence. Is that *possibility* enough to sustain Rey’s claim that he has proposed “a perfectly scientifically respectable model of a VoC” (p.10)? I think not, given the second serious objection to Rey’s proposal.

6.4 Serious Objection (II)

Suppose that the language system did deliver a partial SD to the central processor, how would the SD’s information “fairly directly cause” the intuitions that are the concern of VoC? Rey’s example is of an intuition about co-reference. That was a wise choice. Deriving that intuition from the SD of (1) requires some grasp of theoretical syntax but still it would arguably be fairly direct.

But what about the most commonly used intuitions, those of grammaticality/acceptability? Consider the processing of an ungrammatical string. There would be two relevant possibilities: (i) the language system provides an SD of the string to the central processor; or, (ii), it does not (it “crashes”).²⁵ If (i), then that SD would *not* directly cause intuitions of grammaticality. For, that SD does not come with a sign saying “ungrammatical”. To judge that the SD is of an ungrammatical string, the subject would have to apply her theoretical knowledge of the language to the SD. That’s ME not VoC. If (ii), then *information* provided by SDs would have nothing to do with a subject’s grammaticality intuitions. Rather, *the presence or absence* of the SD would be the *data* for the central processor’s response. So, not VoC again. Indeed, what does talk of SDs add to the explanation? We might as well say that the intuitions arise from the subject’s central processor reflection on the data of trying to understand the string. In sum, either

²⁵ Maynes and Gross propose (ii) as a possible defense of VoC (2013: ##14).

way, competence would be providing the *data for* an intuitive judgment not the *content of* that judgment. That's ME.

Now consider ambiguity intuitions. Once again the information provided by SDs is irrelevant and VoC fails. The intuition will be a central processor judgment based on noting that the expression has *two* (or more) SDs. And talk of SDs again seems redundant: we might just as well say that the judgment is based on noting that the expression has two (or more) meanings. ME again.²⁶

In sum, even if the language system did provide SDs to the central processor, those SDs would mostly not provide the informational content of speakers' intuitions. So Rey has not provided a respectable model of VoC.

7. Conclusion

I have previously attributed to Chomskians the VoC view that a native speaker's linguistic competence provides the informational content of her metalinguistic intuitions. A main aim of this chapter has been to defend this attribution from Ludlow's criticisms. I have argued that these criticisms fail. The evidence that the orthodox Chomskian view of metalinguistic intuitions is VoC is overwhelming.

In previous criticisms of VoC I have claimed that we do not have any idea how embodied but unrepresented rules might provide linguistic intuitions. Rey has proposed a version of VoC to show that I am wrong. My second main aim has been to argue that his proposal fails to show this.

Instead of VoC I have urged ME, the view that linguistic intuitions are ordinary empirical theory-laden central-processor responses to linguistic phenomena. If this is right, it has serious methodological consequences. First, the evidential focus in linguistics should move away from the indirect evidence provided by intuitions to the more direct evidence provided by usage. Second, insofar as the evidence of intuitions is sought, there will seldom be good reason for preferring those of folk over those of experts about language. Finally, what goes for linguistics goes for the philosophy of language. But here the needed change is more drastic because philosophers do not seem even to acknowledge the evidence available from usage. Finally, the focus on usage should yield a place for experimental work in, for example, the theory of reference.²⁷

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²⁶ We should say much the same about the "ambiguity" of the Necker cube (Rey 2013: ##).

²⁷ I am indebted to Steven Gross for comments on a draft. I am indebted to Georges Rey not only for such comments but especially for very many fruitful discussions of the issue.

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