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Intuitions In Linguistics

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ABSTRACT

Linguists take the intuitive judgments of speakers to be good evidence for a grammar. Why? The Chomskian answer is that they are derived by a rational process from a representation of linguistic rules in the language faculty. The paper takes a different view. It argues for a naturalistic and non-Cartesian view of intuitions in general. They are empirical central-processor responses to phenomena differing from other such responses only in being immediate and fairly unreflective. Applying this to linguistic intuitions yields an explanation of their evidential role without any appeal to the representation of rules.

1 Introduction

The received view in linguistics is that the intuitions of competent speakers provide the main evidence for linguistic theories. As Noam Chomsky puts it, 'linguistics... is characterized by attention to certain kinds of evidence... largely, the judgments of native speakers' ([1986], p. 36). Indeed, the emphasis on these intuitions is sometimes so great as to imply that they are the *only* evidence the linguist has. Thus, Liliane Haegeman, in a popular textbook, says that 'all the linguist has to go by... is the native speaker's

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intuitions' ([1994], p. 8).[1] But this is not the approved position. Evidence is also to be found in linguistic usage, at least.

In the next section, I shall argue that speakers' intuitions are not the main evidence for linguistic theories. Still, I agree that they *are* evidence. The central concern of this paper is with the question: Why are they?[2]

Before considering this question, we need to be clear about what we mean by 'linguistic intuitions' here. We mean *fairly immediate unreflective judgments* about the syntactic and semantic properties of linguistic expressions, meta-linguistic judgments about acceptability, grammaticality, ambiguity, coreference/binding, and the like. These judgments are frequently expressed in utterances; for example, '"Visiting relatives can be boring" is ambiguous' or 'In "Tom thinks Dick loves himself" "himself" must refer to the same person as "Dick"'. Such meta-linguistic utterances are not to be confused, of course, with the vastly more numerous utterances we make about the nonlinguistic world. Nor are they to be confused with behavioral responses that are not meta-linguistic utterances; for example, looking baffled by an ungrammatical utterance or behaving in a way that clearly takes a pronoun to corefer with a certain name. The intuitions in question are *judgments about* linguistic performances not the performances themselves.

The standard answer in linguistics to our question starts from the assumption:

> **The Representational Thesis (RT)**: A speaker of a language stands in an unconscious or tacit propositional attitude to the rules or principles[3] of the language, which are represented in her language faculty.

Many linguists, including Chomsky, seem to believe RT.[4] It is the core of what Jerry Fodor ([1981]) calls 'the Right View' of what linguistics

[1] But two pages later Haegeman allows, somewhat grudgingly, an evidential role for usage. Andrew Radford opens a book ([1988]) with an extensive discussion of the evidential role of intuitions. The first mention of the use of the 'corpus of utterances' as data does not come until p. 24. Robert Fiengo starts an interesting paper on linguistic intuitions: 'Intuitions, with the contents that they have, are the data of Linguistics' ([2003], p. 253).

[2] The paper is based on chapter 7 of *Ignorance of Language* ([2006]).

[3] Recent versions of generative grammar prefer to talk of principles rather than rules. I shall simply talk briefly of rules.

[4] Some evidence that Chomsky takes knowledge of a language as a propositional attitude: He describes the knowledge as a 'system of beliefs' ([1969], pp. 60–1; see also [1980a], p. 225) and says that a child's acquisition of a language is the discovery of 'a deep and abstract theory — a generative grammar of his language' ([1965], p. 58). See also his more recent [1986], pp. 263–73, which includes the following: 'Knowledge of language involves (perhaps entails) standard examples of propositional knowledge' (p. 265); 'it is proper to say that a person knows that R, where R is a rule of his or her grammar' (p. 268). Chomsky mostly uses the term 'know' for the propositional attitude in question but, when the chips are down, he is prepared to settle for the technical term 'cognize' ([1975b], pp. 164–5, [1980a], pp. 69–70). Evidence that this attitude involves representation of the rules can be found in the following characteristically firm statement: 'there can be little doubt that knowing a language involves internal representation of a

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> 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. (Chomsky [1986], p. 270; see also Pateman [1987], p. 100; Dwyer and Pietroski [1996], p. 342).
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> we cognize the system of mentally represented rules from which [linguistic] facts follow. (Chomsky [1980b], p. 9; the facts are expressed in intuitive judgments).
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> 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. (J. A. Fodor [1981], pp. 200–1).
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> [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], p. 325).
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> Our ability to make linguistic judgments clearly follows from our knowing the languages that we know. (Larson and Segal [1995], p. 10; see also Pylyshyn [1984], p. 122; Baker [1995], p. 20).

So, on this explanation, linguistic competence alone provides information about linguistic facts; the intuitive judgments are, as I put it, the 'voice of

generative procedure' ([1991], p. 9; see also [1965], p. 25; [1975a], p. 304; [1980a], p. 201; [1980b], p. 9; [2000], p. 50).

RT is certainly widespread. Consider the following, for example, for fairly explicit statements: (i) (Fodor, Bever, and Garrett [1974], p. 7; Bresnan and Kaplan [1982], p. xvii; Berwick and Weinberg [1984], p. 35) (ii) A recent encyclopedia article endorses the view (attributed to Chomsky) that 'the human language faculty is a system of knowledge.' 'This knowledge is formal, specific to the language capacity (as distinct from other cognitive capacities), and cerebrally represented. It constitutes a biological module, putatively distinct from other knowledge bases and mental processes' (Grodzinsky [2003], p. 741). (iii) In discussing the distinction between knowing how and knowing that, Jerry Fodor remarks: 'my linguist friends tell me that learning how to talk a first language requires quite a lot of learning that the language has the grammar that it does' ([1998], p. 125). (iv) Two philosophers influenced by Chomsky, Susan Dwyer and Paul Pietroski, base a theory of belief on the view that 'ordinary speakers believe the propositions expressed by certain sentences of linguistic theory' ([1996], p. 338). (v) Alex Barber takes Chomsky and others to hold that a linguistic theory is 'the explicit statement of certain aspects of the content of knowledge states possessed by ordinary speakers' ([2003b], p. 3).

5 'The Right View' is that linguistics is about the representations of grammatical rules in the mind/brain. I argue against this view in Devitt and Sterelny ([1989]) and, I think, much better in Devitt ([2003]) and better still in Devitt ([2006]): linguistics is not about this putative psychological reality but about a linguistic reality made up of the spoken, written, etc., symbols that speakers produce. Fodor contrasts the Right View with an instrumentalist view he calls 'the Wrong View.' According to this view, linguistic theories are about speakers' intuitions: the linguistic task is to systematize the intuitions. This view should not be confused with the one we are considering: that the intuitions are the main evidence for linguistic theories.

competence'. 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 by virtue of being competent and, thus, embodying representations of its rules in her language faculty, a module of the mind. I need a word for such special access to facts. I shall call it 'Cartesian'. We would like the details of this Cartesian explanation spelt out, of course. We would like to know about 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 Cartesian explanation may seem promising.[6]

Clearly, however, the goodness of the explanation depends on the likely truth of RT, a *very* powerful assumption about the mind. Now one might think that the standard explanation itself makes RT plausible for it can be used as the basis for the following abduction (inference to the best explanation): RT is the core of a good explanation of why the intuitions are evidence, and there is no other explanation; so RT is probably true. If the intuitions are really derived from representations of the grammatical rules then they must be true and, hence, good evidence for the nature of those rules. But if they are not so derived, how could they be good evidence? How could they have this evidential status unless they really were the voice of competence?[7]

Various things are required for this abduction to be good. One is that RT has to be independently plausible given what else we know. I think RT fails this test very badly, but I shall not be arguing that here.[8] I shall be arguing that there are reasons for doubting the standard Cartesian explanation quite apart from doubts about RT. More importantly, I shall be arguing that

[6] Note that the explanation does not suppose that the speaker has Cartesian access to the linguistic rules, just to the linguistic facts captured by the intuitions.

[7] Against the background of the standard Cartesian explanation, we can see the following as a statement of the argument:
Linguists normally take the intuitions of speaker/hearers to be the data to which structural descriptions are required to correspond. But this practice would be quite unwarranted unless it were assumed that speaker/hearers do have access to internal representations of sentences and that these provide a reliable source of information about the character of the abstract object (the language) which, on any view, the grammar is ultimately intended to describe. (Fodor, Fodor, and Garrett [1975], p. 244).
Graves et al. claim that the only plausible explanation of a speaker's explicit knowledge of grammatical facts is that she has tacitly deduced that knowledge from tacitly known principles of the language ([1973], pp. 324–9). The argument is implicit in Laurence [2003], pp. 89–91).

[8] In Devitt ([2006]), I argue that there is no significant evidence for RT and that, given what else we know, it is in fact implausible. In particular, a consideration of language use provides no persuasive evidence for RT. Not only is it not now part of a good explanation of language use, it is unlikely to be so in the future. My argument places a lot of weight on what we can learn from the general psychology of skills and their acquisition.

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another explanation of the evidential role of linguistic intuitions, an explanation that does not rest on RT, is better. So the abduction fails anyway.

What sense of 'represent' is playing a role in RT and the standard explanation? It is natural to take this sense as the very familiar one illustrated in the following claims: a portrait of Winston Churchill represents him; a recent sound /the President of the United States/ represents George W. Bush; an inscription, 'rabbit', represents rabbits; a certain road sign represents that the speed limit is 30 mph; the map on my desk represents the New York subway system; the number 11 is represented by '11' in the Arabic system, by '1011' in the binary system, and by 'XI' in the Roman system; and, most aptly, a (general-purpose) computer that has been loaded up with a program represents the rules of that program. Something that represents in this sense has a *semantic content*, a *meaning*. When all goes well, there will exist something that a representation refers to. But a representation can fail to refer; thus, nothing exists that 'James Bond' or 'phlogiston' refers to. Finally, representations in this sense are what various theories of reference—description, historical-causal, indicator, and teleological—are attempting to partly explain and what the popular 'Representational Theory of the Mind' claims to be involved in all propositional attitudes.

This is the natural interpretation of 'represent' in this context, because it nicely accommodates the linguists' talk of 'beliefs', 'theories', 'propositional knowledge' and, particularly, of intuitions being 'deduced' from representations. And it is a relatively clear interpretation. So it is the interpretation that I shall adopt throughout this paper in understanding RT and the standard explanation. Still, the interpretation may not be right for all linguists who talk in this way.[9] 'Represent' (and its cognates) is used so widely and loosely in cognitive science that it is hard to be confident about what it means on any one occasion. This is not the place to try to analyze these uses and come up with other interpretations. However, I take it that linguists who talk in this way but reject my interpretation, and hence the standard explanation (as I am understanding it), will nonetheless hold that the rules are *embodied somehow* in a speaker without being represented (in the above sense). And these linguists are still committed to the Cartesian view that intuitions are the voice of competence, the view that speakers, simply by virtue of being competent, have information about the linguistic facts. How could this be so if the rules are not represented? The linguists need what I shall call 'a nonstandard Cartesian explanation' of why linguistic intuitions are good

[9] On the problem of interpreting what Chomsky means by 'represent' and other apparently intentional expressions, see the fascinating exchange: (Rey [2003a]; Chomsky [2003]; Rey [2003b]). See also the interesting interpretation in (Collins [2004]).

evidence for linguistic theories, an explanation of how unrepresented rules provide the privileged access to linguistic facts.

In Sections 3 to 6, I shall argue that the standard Cartesian explanation is not the best: the evidential role of linguistic intuitions can be better explained otherwise. This other explanation does not suppose that the intuitions are the product of embodied linguistic rules, whether represented or not; they are not the voice of competence. Rather, they are opinions resulting from ordinary empirical investigation, theory-laden in the way all such opinions are. If this is right, then the abduction to RT fails. In Section 7, I shall consider the possibility of a nonstandard Cartesian explanation. As we have just noted, a linguist who rejects the view that linguistic rules are represented (in the above sense) in speakers needs such an explanation. So far as I know, none has ever been proposed. I shall argue that none is likely to be forthcoming. Finally, in Section 8, I shall look critically at the view that it is a task of linguistics to *explain* linguistic intuitions.

I turn now to consider briefly what sort of evidence we have for linguistic theories.

2 The evidence for linguistic theories

I began the paper by noting the received view in linguistics that the intuitions of competent speakers provide the main evidence for linguistic theories; indeed, the view is often that these intuitions are near enough the only evidence. These views greatly exaggerate the evidential role of the intuitions. As recent experimenters who did actually test the intuitions of naïve subjects remark, this testing is 'in contrast to common linguistic practice' (Gordon and Hendrick [1997], p. 326). I suggest that, as a matter of fact, only a small proportion of the evidence used in grammar construction consists in the canvassed opinions of the ordinary competent speaker.

So what else constitutes the evidence? (i) One possible source of direct evidence is 'the corpus', the linguistic sounds and inscriptions that the folk have produced and are producing as they go about their lives without any interference from linguists. We can observe people and seek answers to questions like: 'Do people ever say *x*?'; 'How do they respond to *y*?'; 'In what circumstances do they say *z*?' The role such observations have played, particularly in the beginning of linguistics, is insufficiently acknowledged. Linguists may well rely extensively on intuitions now that generative grammars are in an advanced stage (even though not complete, of course). But think back to the beginning, perhaps just to the dark days before there were any generative grammars. Surely a lot of the early knowledge about languages, still captured by generative grammars, was derived from simply observing linguistic usage, much as the field linguist does. Even now, it is hard to believe that *L*-speaking linguists surrounded by other *L*-speakers are uninfluenced by the

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data they are immersed in. One would expect them, given their training, to be peculiarly sensitive to these data. Anecdotal evidence suggests that they are indeed so sensitive; that they are continually on the watch and noting linguistic oddities. Consider the theory of 'wanna' constructions, for example. We can surely be confident that the linguists' observations of the use of 'wanna' have played a considerable role in building this theory.

(ii) Interference by linguists can yield further direct evidence: we can contrive situations and see what subjects say or understand. Consider, for example, this description of 'the technique of *elicited production*':

> This technique involves children in a game, typically one in which children pose questions to a puppet. The game orchestrates experimental situations that are designed to be uniquely felicitous for production of the target structure. In this way, children are called on to produce structures that might otherwise not appear in their spontaneous speech. (Thornton [1995], p. 140).

Although this sort of technique is frequently used on children,[10] analogous ones are doubtless seldom used on adults, because contriving these situations is likely to be laborious. But, clearly, much evidence could be gathered in this way.

We might aptly describe a person's responses in these contrived situations as 'intuitive', but those responses are very different from the intuitions that are the main concern of this paper. They differ in not being judgments about the syntactic and semantic properties of expressions and, hence, not needing to deploy linguistic concepts.

(iii) Another sort of interference can yield evidence that is less direct: we can *describe* situations and ask people what they would say or understand in these situations. This evidence is less direct because it depends on people's reflections on these situations. Once again, we might aptly describe a person's response as 'intuitive' but it is not an intuitive judgment about linguistic properties. Such responses are another source of evidence that is insufficiently acknowledged. Linguists ask themselves, and sometimes ordinary speakers, what they would say or understand in various situations.

(iv) Further, less direct evidence can be obtained from language acquisition: evidence about what sort of languages we could learn is evidence about what language we have learnt. This sort of evidence is, of course, frequently acknowledged.

(v) Finally, I suggest that a good deal of less direct evidence comes from the intuitive opinions *of linguists* about the languages they speak. As an entry in the *Encyclopedia of Cognitive Science* points out, partly because it is

[10] See (Crain et al. [2005], Section 4), for a nice summary.

sometimes 'difficult to replicate relatively agreed-upon judgments of linguists while testing naive subjects ... and partly for sheer convenience, linguists rely increasingly on other linguists for judgment data on some languages' (Schutze [2003], p. 913). The intuitions of ordinary speakers may seem to be more present than they really are because linguists take their own intuitions to be representative.

There are surely many other ways to get evidence of linguistic reality apart from consulting intuitions, just as there are to get evidence of any other reality.

In sum, the main evidence for grammars is not found in the intuitions of ordinary speakers but rather in a combination of the corpus, the evidence of what we would say and understand, and the intuitions of linguists. Still, it cannot be denied that ordinary competent speakers do have largely reliable intuitions, which do play an evidential role in linguistics, even if that role is greatly exaggerated. So we still need to explain that role. And the abduction still stands: RT is the best explanation of this role.

I shall be developing the theory that linguistic intuitions are theory-laden empirical opinions. But I begin by noting a tension in linguistic discussions of these intuitions arising, it seems to me, from the attraction of this theory.

3 A tension in the linguists' view of intuitions

A simplistic version of the standard Cartesian explanation would be that since linguistic intuitions are derived from the rules of the language they must always be true and so are as good as any evidence could be. Although there are signs of this version in the literature,[11] it is not the approved version. The approved version allows for errors arising from 'noise'. So, just as there can be performance errors in using our competence to produce and understand the sentences of our language, there can be performance errors in producing judgments about such sentences. The explanation is Cartesian in supposing that we have a nonempirical privileged access to linguistic facts but not in supposing that this access yields infallible judgments.

This retreat from reliance on intuitions seems clearly appropriate on the standard view. Other retreats are not so clearly so. (i) Although there has been reliance on intuitions about *grammaticality* the tendency in recent times has been to emphasize ones about *acceptability, goodness*, and the like,[12] and to offer explanations of these intuitions that are often syntactic but sometimes semantic or pragmatic. Yet *grammaticality* is the notion from linguistic theory, and so if the intuitions are really derived from a

[11] (Fodor, Bever, and Garrett [1974], p. 82; Baker [1978], pp. 4–5.)

[12] For example, (Higginbotham [1987], p. 123; Radford [1988], p. 10; Hornstein [1989], p. 26, 38n; Haegeman [1994], p. 7; Baker [1995], pp. 8, 38) Dwyer and Pietroski do not exemplify this tendency ([1996], p. 346).

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Radford [1988], p. 10; Hornstein [1989], p. 26, 38n;
38) Dwyer and Pietroski do not exemplify this

representation of that theory, should not we be relying on intuitions about grammaticality? If, in our intuitive judgments, competence is really speaking, why does not it *use its own language*? What is the causal-rational route from an unconscious representation of something's 'grammaticality' to a conscious judgment of its 'acceptability'? (ii) Ordinary speakers have many intuitions about grammaticality, coreference, and ambiguity but few about transitivity, heads, A'-positions, c-command, cases, transformations, and so on. Why is that? Why doesn't linguistics have a much wider range of intuitions to rely on? Linguistic theory is very rich. If our competence consists in representing this theory and our competence speaks to us at all, how come it *says so little*? Once again, we wonder how the details of the causal-rational route could be spelt out to account for this.

The clue to what underlies these retreats from reliance on intuitions is to be found in many passages like the following:

> it is hardly surprising that informants should not be able to tell you whether a sentence is pragmatically, semantically, or syntactically ill-formed: for these very notions are terms borrowed from linguistic theory: and like all theoretical terms, they are meaningless to those not familiar with the theory. (Radford [1988], p. 13).

Such passages reflect a sensitivity to the highly theoretical nature of linguistic terms. And they reflect the attractive thought that these terms have their place in empirical theories that are hard-won by linguists, with the result that judgments involving them are not plausibly attributed to people *simply by virtue of being competent speakers.*[13] The retreat to acceptability may seem to escape this thought, because 'acceptable' is a very ordinary term and not in linguistic theory.

I see linguists pulling in two directions in their treatment of the intuitive judgments of speakers. On the one hand, the standard view is that speakers represent the true linguistic theory of their language and derive their intuitive judgments from these representations. So, those intuitive judgments, deploying terms drawn from that theory, should be the primary data for the linguist's theory. On the other hand, there is the attractive thought that all judgments deploying these terms are laden with an empirical linguistic theory. Where the judgments are those of the ordinary speaker, that theory will be folk linguistics. We do not generally take theory-laden folk judgments as primary data for a theory. So we should not do so in linguistics.

[13] In this respect it is interesting to note Chomsky's skepticism about 'contemporary philosophy of language' and its practice of 'exploring intuitions about the technical notions "denote", "refer", "true of", etc.' He claims that 'there can be no intuitions about these notions, just as there can be none about 'angular velocity' or 'protein'. These are technical terms of philosophical discourse with a stipulated sense that has no counterpart in ordinary language.' ([1995], p. 24; [2000], p. 130).

The emphasis on intuitions about acceptability (also goodness and the like) may seem to remove this tension, but it does not really. First, intuitions about acceptability are not the only ones playing an evidential role. There are also intuitions about coreference/binding, ambiguity, and so on, involving terms that are straightforwardly linguistic. Whatever we say about 'acceptable' is no help with the tension associated with these intuitions. Second, 'acceptable' (also 'good' and the like) is a highly context-relative term: it might mean *acceptable in polite society, acceptable in a philosophical argument*, and so on. What a linguist is aiming to elicit from an ordinary speaker is, of course, an intuition about what is *acceptable grammatically in her language*; he wants the voice of her competence. 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'. So we are still pulled towards seeing her intuitive responses as judgments laden with folk linguistics. So the tension remains.

In other linguistic contexts 'acceptable' (and 'good') is likely to be taken in different ways. Thus the question 'Is this expression acceptable in your community?' would invite the speaker to consider not only grammatical facts of her language but also pragmatic ones about etiquette, appropriateness, interest, and so on. And there is a considerable risk that the simple 'Is this expression acceptable?' and even 'Is this expression acceptable in your language?' will also bring in pragmatic considerations. Yet, clearly, the linguist is concerned with asking questions that minimize the intrusion of pragmatics into intuitions that he hopes are the voice of competence.[14] In any case, insofar as pragmatic considerations do intrude, the attractive thought encourages the view that the intuitions are still theory laden: they are laden not only with folk linguistics but also with pragmatic theories about what is good etiquette, socially appropriate, interesting enough to be worth saying, and so on. So there is still a tension.

The discussion in this section is the first step in undermining the standard Cartesian explanation. On the one hand attention to the language in which competence allegedly speaks and to how little it says raises concern about the details of the causal-rational route from representations in the language faculty to a judgment in the central processor. On the other hand, we

[14] This being so, one wonders why linguists would ever use such a vague, pragmatic, context-relative term as 'acceptable' to seek grammatical intuitions unless pragmatic factors are controlled for (as in 'minimal-pair' experiments; see Section 5).

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4 Intuition

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have begun to see why the thought that ordinary linguistic intuitions are laden with empirical folk linguistics is attractive.

In Section 4, I shall present a view of intuitions in general. In Section 5, I shall apply this view to linguistic intuitions, yielding a view of them along the lines of the attractive thought. This view removes the tension by abandoning the received idea that the intuitions are derived from a representation of the rules: competence has no voice. And it yields an explanation of the evidential role of linguistic intuitions which, I shall argue in Section 6, is better than the standard one.

4 Intuitions in general

Questions about the status of intuitions do not arise only in linguistics, of course; intuitions play a role in ordinary life and science and seem to dominate philosophy. What are we to say of them in general? In *Coming to Our Senses* ([1996], pp. 72–85) I argue for a naturalistic, and non-Cartesian, view of intuitions in general.[15] On this view, intuitive judgments 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. These intuitions are surely partly innate in origin[16] but are usually and largely the result of past reflection on a lifetime of worldly experience.[17]

A clarification. It may be that there are many unreflective empirical responses that we would not ordinarily call intuitions: one thinks immediately of perceptual judgments like 'That grass is brown' made on observing some scorched grass, or 'That person is angry' made on observing someone exhibiting many signs of rage. Perhaps we count something as an intuitive judgment only if it is *not really obvious*. I shall not be concerned with this. My claim is that intuitions are empirical unreflective judgments, *at least*. Should more be required to be an intuition, so be it.

In considering intuitions and their role in science, it is helpful to distinguish the most basic intuitions from richer ones. Suppose that we are investigating the nature of a kind *F*—for example, the kind gene, pain or echidna. The most basic intuitions are ones that identify *F*s and non-*F*s; for example, 'This is an echidna but that isn't.' It is important to note that to have even these

[15] And in ([1994], pp. 561–71.) See also (Kornblith [1998]).

[16] In calling the intuitions 'empirical' I am claiming simply that they must be justified 'by experience'. Should any justified belief be entirely innate, which I doubt, then beliefs of that sort must have been justified somehow by the experiences (broadly construed) of our distant ancestors, and we must have inherited that justification via natural selection.

[17] 'intuition is the condensation of vast prior analytic experience; it is analysis compressed and crystallized.... It is the product of analytic processes being condensed to such a degree that its internal structure may elude even the person benefiting from it...' (Goldberg [2005], p. 150).

most basic intuitions a person must have the appropriate concepts: you cannot identify an *F* if you do not have the concept of an *F*. The richer intuitions go on to tell us something about *F*s already identified; for example, 'Echidnas look like porcupines.' The richer ones may be much less dependable than the basic ones: a person may be good at recognizing *F*s without having anything reliable to say about them; this is very likely the situation of the folk with pains.

Identifying uncontroversial cases of *F*s and non-*F*s is only the first stage of an investigation into the nature of *F*s: the second stage is to examine these cases to see what is common and peculiar to *F*s. Sometimes we have a well-established theory to help with the first stage; thus we had Mendelian genetics to identify the genes that were examined by molecular genetics in the second stage. But sometimes we do not have such help: we start pretty much from scratch; we are at the stage of *proto*-science. At that stage, the most basic intuitions are particularly important. In the absence of reliable theory, we must start by consulting the people who are most expert about *F*s to see what *they* identify as *F*s and non-*F*s: we elicit their most basic intuitions about *being an F* in 'identification experiments'. We are then in a position to begin our investigation. Until recently, at least, this was our position with pains.

When we are starting from scratch, we need the basic intuitions, but we do not need the richer ones. This is not to say that we should not use them. They may well be a useful guide to what our investigation will discover about *F*s; they are 'a source of empirical hypotheses' (Gopnik and Schwitzgebel [1998], p. 78).

We should trust a person's intuitions, whether basic ones or richer ones, to the degree that we have confidence in her empirically based expertise about the kinds under investigation. Sometimes the folk may be as expert as anyone: intuitions laden with 'folk theory' are the best we have to go on. Perhaps this is the case for a range of psychological kinds. For most kinds, it clearly is not: we should trust intuitions laden with established scientific theories. Consider, for example, a paleontologist in the field searching for fossils. She sees a bit of white stone sticking through gray rock, and thinks 'a pig's jawbone'. This intuitive judgment is quick and unreflective. She may be quite sure but unable to explain just how she knows.[18] We trust her judgment in a way that we would not trust folk judgments because we know that it is the result of years of study and experience of old bones; she has become a *reliable indicator* of the properties of fossils. Similarly we trust the intuitions of the physicist over those of the

[18] I owe this nice example to Kim Sterelny. Gladwell ([2005]) has other nice examples: of art experts correctly judging an allegedly sixth-century Greek marble statue to be a fake; of the tennis coach, Vic Braden, correctly judging a serve to be a fault before the ball hits the ground.

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folk about many aspects of the physical world where the folk have proved notoriously unreliable. And recent experiments have shown that we should have a similar attitude to many psychological intuitions. Thus, the cognitive psychologist, Edward Wisniewski, points out that 'researchers who study behavior and thought within an experimental framework develop *better* intuitions about these phenomena than those of the intuition researchers or lay people who do not study these phenomena within such a framework. The intuitions are better in the sense that they are more likely to be correct when subjected to experimental testing.' ([1998], p. 45).

Even where we are right to trust an intuition in the short run, nothing rests on it in the long run. We can look for more direct evidence in scientific tests. In such a scientific test, we examine the reality the intuition is *about*; for example, we examine the paleontologist's white stone. These scientific examinations of reality, not intuitions about reality, are the primary source of evidence. The examinations may lead us to revise some of our initial intuitions. They will surely show us that the intuitions are far from a complete account of the relevant bit of reality.

Intuitions often play a role in 'thought experiments'. Instead of real experiments that confront the expert with phenomena and ask her whether they are *F*s, we confront her with *descriptions* of phenomena and ask her whether she *would say* that they were *F*s.[19] These thought experiments provide valuable clues to what the expert would identify as an *F* or a non-*F*. They can do more: the descriptions that elicit the expert's response indicate the richer intuitions that, as we have already noted, can be a useful guide to the nature of *F*s. Some experiments may be difficult, perhaps impossible, to perform other than in thought. Valuable and useful as thought experiments may be in practice, they are dispensable in principle: we can make do with real experiments. And thought experiments call on the same empirically based beliefs about the world as real experiments, and their results have the same empirical status.

Aside. This account of thought experiments provides a naturalistic explanation of the characteristic 'armchair' method of philosophy. The traditional explanation of this method is that philosophers are conducting thought experiments that probe their *concepts* to yield a priori *rational* intuitions; they are doing 'conceptual analysis'.[20] The naturalistic explanation accepts that philosophers are conducting thought experiments but construes these

[19] There are other things we might ask, for example, 'What would happen?' but these are beside our concerns. Gendler ([2003]) is a nice summary of views about thought experiments.

[20] The best reason for being dubious of the traditional explanation is that we do not, I have argued ([1996], [1998]), have even the beginnings of an account of what a priori knowledge is. We are simply told what it isn't, namely empirical knowledge. Bealer ([1998]) and BonJour ([1998]) are vigorous defenders of rational intuitions; see also (Sosa [1998]). For an exchange on the subject, see (BonJour [2005a,b,c] and Devitt [2005b,c]).

differently. The philosophers are not probing concepts but rather *intuitions about kinds*. This is just as well because knowledge of concepts, being a species of semantic knowledge, is very hard to come by. In contrast, philosophers have acquired considerable knowledge of many kinds over a lifetime of acquaintance with them. The philosophers' intuitions that draw on this knowledge, draw on these theories of the world, are not a priori but empirical. The philosophers are conducting thought experiments of the sort described in the last paragraph, counting themselves as experts about the kind in question. Thus, in a famous example of the method, 'the analysis of knowledge', the philosopher, as expert as anyone in identifying cases of knowledge, confronts descriptions of epistemic situations and considers whether the situations are cases of knowledge. On the basis of these empirical intuitions about cases she constructs an empirical theory about the nature of knowledge. The naturalist does not deny armchair intuitions a role in philosophy but does deny that their role has to be seen as a priori: the intuitions reflect an empirically based expertise in the identification of kinds.

The view I have presented of the limited and theory-laden role of intuitions does not need to be modified for the situation where what we are investigating are the products of a human competence (which is the situation in the philosophy of language and linguistics, of course). This situation would arise if we were (for whatever reason) investigating the nature of horseshoes, chess moves, touch typing, or thinking. Someone who has the relevant competence has ready access to a great deal of data that are to be explained. She does not have to go out and look for data because her competence produces them. Not only that, she is surrounded by similarly competent people who also produce them. As a result, she is in a good position to go in for some central-processor reflection upon the data produced by herself and her associates. This reflection, often aided by appropriate education, can yield concepts and a theory about the data. And it can yield the capacity for sound intuitions, basic and richer, about the data. In brief, she can become an expert. But this is not to say that she *will* become an expert. A person can be competent and yet reflect little on the output of that competence. Or she can reflect a lot but make little progress. Bicycle riders typically fall into one of these two categories. It is a truism in sport that great players do not always make great coaches. The fact that they possess a competence to a superlative degree does not imply that they can articulate and communicate the elements of that competence. Knowledge-how may not lead to knowledge-that. In brief, a person competent in an activity may remain ignorant about it.[21] And even if she does become an expert, we should not

[21] 'Highly skilled performers are often unable to reflect on or talk about how they achieve their skilled performance.' (Carlson [2003], p. 38).

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assume that her opinions carry special authority simply because she is competent; her competence does not give her Cartesian access to *the truth*. She is privileged in *her ready access to* data, not in *the conclusions she draws from* the data; conclusions of the competent, just like those of the incompetent, are empirical responses to the phenomena and open to question; they arise from the empirical observation of data.

Touch typing provides a nice example of reflecting on the output of one's own competence. Ask a touch typist whether a 'k' should be typed with a middle finger and, very likely, he will think to himself, 'How would I type a "k"?' He will attend as he goes through the actual or mental motions of doing so and respond immediately, 'Yes'. Consider also this report:

> If a skilled typist is asked to type the alphabet, he can do so in a few seconds and with very low probability of error. If, however, he is given a diagram of his keyboard and asked to fill in the letters in alphabet order, he finds the task difficult. It requires several minutes to perform and the likelihood of error is high. Moreover, the typist often reports that he can only obtain the visual location of some letters by trying to type the letter and then determining where his finger would be. (Posner [1973], p. 25).[22]

The only privilege enjoyed by the typist's judgment about which finger should be used to type a 'k', or about where a letter is placed on the keyboard diagram, is the privilege of being based on what is surely a good datum: on how he, a good touch typist, types.

Although these typist's judgments are slow relative to his typing, they would probably be fast enough for us to count them as intuitive. And they are likely to be sound, for it is fairly easy to think about typing. Contrast this with thinking about the outputs of another, much more important, human competence, the competence to think, to move in a somewhat rational way from one thought to another. We all have this competence to some degree or other. Most of us reflect a bit on this and have some intuitions about what follows from what. Still, these intuitions are likely to be sparse and many of them are surely not sound. Thinking about thinking is so hard.

Now it is, of course, possible that the typist has somewhere in his mind a prior representation of the keyboard, which controls his typing and leads to

[22] And consider this report (Sun et al. [2001]). Subjects were placed in front of a computer with the task of navigating a submarine through a minefield using sonar. After some episodes, 'subjects were asked to step through slow replays of selected episodes and to verbalize what they were thinking during the episode' (p. 219). The experimenters sum up the results as follows: 'The subject at first performed the task on an 'instinctual' basis, without conscious awareness of any particular rules or strategies. Gradually, through 'doing it' and then looking at the results, the subject was able to figure out the action rules explicitly. The segment suggested implicit procedural learning at the bottom level and the gradual explication of implicitly learned knowledge' (p. 226).

his sound judgment about how to type a 'k'. *But why believe this*? Set aside whether we need to posit this representation to explain his typing. We surely do not need the posit to explain his judgment. The more modest explanation I have just given, making do with cognitive states and processes that we are *already* committed to, seems perfectly adequate for the job. Positing the prior representation is explanatorily unnecessary. Finally, when we turn to the case of thinkers, such positing would seem worse than unnecessary. The idea would have to be that the thinker's mind contains a representation of the 'laws of thought', which controls her thinking and leads her to, say, the *modus ponens* intuition that '*q*' follows from 'if *p* then *q*' and '*p*'. But, as Lewis Carroll's famous dialogue between Achilles and the Tortoise demonstrates ([1895]), this view of thinking would lead to an infinite regress.[23] The modest explanation is the only plausible one: a person's thinking is governed by rules that she does not represent, and her few intuitive judgments about thinking are the result of reflecting on the performances of herself and others.

On the picture of intuitions I am presenting, what should we make of linguistic intuitions? And whose intuitions should we most trust?

5 Linguistic intuitions

The focus in *Coming to Our Senses* is on the explanation of meanings. I claim that the folk are as expert as anyone at identifying meanings, expressing the most basic linguistic intuition of all. They do this in the ubiquitous practice of ascribing thoughts and utterances to people; the folk say things like 'Ruth believes that Clinton is sexy' and 'Adam said that Bush does not speak a natural language'. The 'that'-clauses of these ascriptions specify meanings (or 'contents'). Because these folk ascriptions are generally successful at serving their purposes—particularly, the purposes of explaining behavior and guiding us to reality—we have reason to think that they are generally true. And note that, although part of what is ascribed is a meaning, no semantic term need be deployed: the 'that'-clauses above do not contain such terms and that is normal; a meaning is ascribed by using, in the 'that'-clause, a sentence with that very meaning, or something close. Not only are the folk good at identifying meanings but the poor state of semantic theory gives no reason to think that the theorists will do significantly better.

[23] Suppose that the inference from the two premises, (A) 'if p then q', and (B) 'p', to the conclusion, (Z) 'q' had to be accompanied by a third premise, (C) 'if (A) and (B) then (Z)', which captures the 'law of thought' that modus ponens is a good inference. Then in order to infer (Z) from these three premises, we would similarly need a fourth, (D) 'if (A) and (B) and (C) then (Z)'. And so on ad infinitum.

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[24] As Chomsky says, competent speakers 'c
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[25] This presupposes a realism about lingu
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theoretically speaking.' (Bob Matthews, i

The situation is different when it comes to deploying the vocabulary of the philosophy of language and linguistics to make intuitive semantic and syntactic judgments about an utterance that has the identified meaning. Let us now apply the modest explanation of intuitions in general to the special case of these linguistic intuitions.

The competent speaker has ready access to a great deal of linguistic data just as the competent typist has to a great deal of typing data and the competent thinker has to a great deal of thinking data: the competent speaker and her competent fellows produce linguistic data day in and day out.[24] So she is surrounded by tokens that may, *as a matter of fact*, be grammatical, be ambiguous, have to corefer with a certain noun phrase, and so on.[25] So she is in a position to have well-based opinions about language by reflecting on these tokens. This is not to say that she will reflect. Indeed, a totally uneducated person may reflect very little and, hence, have few if any intuitive judgments about her language. She may be ignorant of her language.[26] Still it is clear that the normal competent speaker with even a little education *does* reflect on linguistic reality just as she reflects on many other striking aspects of the world she lives in. And this education will usually provide the terms and concepts of folk linguistics, at least. As a result she is likely to be able to judge in a fairly immediate and unreflective way that a token *is* grammatical, *is* ambiguous, *does* have to corefer with a certain noun

[24] As Chomsky says, competent speakers 'can easily construct masses of relevant data and in fact are immersed in such data' ([1988], p. 46).

[25] This presupposes a realism about linguistic entities that are, according to my ([2003]), the subject matter of linguistics; see note 5. This realism is curiously denied by some. Consider the following, for example: 'The sound waves produced by a speaker, or the gestures of an ASL user, or the ink marks of a text, understood as environmental features, have no linguistic properties whatsoever, not lexical, not phonological, not syntactic, not semantic' (anonymous reviewer of a version of the present paper); see also (Rey [2006]). I think ([2006], Section 10.6) that this view is a mistaken reaction to two facts: first, that the properties by virtue of which something is a linguistic token are all relational; second, that tokens of the one linguistic expression can appear in a variety of physical forms, a variety of sounds, a variety of inscriptions, and so on. Yet something can really have a certain linguistic property just as something can really have a certain nationality even though neither have these properties intrinsically and even though things that have them can differ greatly in their physical forms.

[26] This point is nicely illustrated by the following report: 'As a graduate student I spent a summer in the Pyrenees (Andorra, Perpignon, etc.) doing field research on the phonology of various dialects of Catalan. Many of our native informants were illiterate peasants. I was forcefully struck how difficult it was to elicit linguistic judgments from them regarding their language, which of course they spoke perfectly well. Just getting the plurals of certain nouns was tough. These folks seemed to be very hard of hearing when it came to hearing the voice of competence! Their difficulty, it seemed, was that their native language was largely transparent to them—they had never thought of it as an object for observation and hence were largely unable to form even the most rudimentary judgments about its character. Catalan speakers with only a modicum of grade school education, by contrast, were good informants, presumably because they had learned through their grammar lessons to think of language as an object with various properties, even if they had no sophisticated knowledge of what those properties might be, theoretically speaking.' (Bob Matthews, in correspondence).

phrase, and so on. Such intuitive opinions are empirical central-processor responses to linguistic phenomena.[27] They have no special authority: although the speaker's competence gives her ready access to data it does not give her Cartesian access to the truth about the data.[28]

So, on this modest account, how does a normal competent speaker make a grammaticality judgment about a novel expression? As a result of education and reflection, she already has the folk linguistic concept of grammaticality in her language. And she appreciates the connection between this grammaticality and competence in the language: roughly, errors aside, competent speakers produce and understand grammatical sentences. She knows that she is a competent speaker and so uses herself as a guide to what the competent speaker *would do*. So she asks herself whether this expression is something she would say and what she would make of it if someone else said it. Her answer is the datum. Clearly her linguistic competence plays a central role in causing this datum about her behavior. *That* is its contribution to the judgment that she must then go on to make. She does some central-processor reflection upon the datum to decide whether to apply her concept of grammaticality to the expression, just as she might reflect upon any other relevant data supplied by the behavior of her fellow speakers. If the datum shows that she would have no problem producing or understanding the expression, she is likely to deem it grammatical. If the datum shows that she has a problem, she will diagnose the problem in light of her background theories, linguistic and others, perhaps judging the expression to be ungrammatical, perhaps judging it to be grammatical but infelicitous or whatever. Often these judgments will be immediate and unreflective enough to count as intuitions. Even when they do count, they are still laden with such background theory as she acquired in getting her concept of grammaticality.

What goes for intuitions about grammaticality will obviously go for intuitions about acceptability insofar as these are nothing but intuitions about grammaticality, insofar as 'acceptable' in the context is simply expressing the speaker's notion of grammaticality (and hence not expressing pragmatic notions like ones about etiquette).[29] And it will go for intuitions about ambiguity and coreference/binding. Furthermore, we can often be confident that such intuitions of normal educated speakers are right. We often have

[27] Ilkka Niiniluoto urges a similar view: 'Linguistic intuition is ... largely observational knowledge about language' ([1981], p. 182).

[28] I emphasize that this is a modest explanation of the origins of a speaker's intuitions about her language. It is emphatically not an explanation of the origins of her linguistic competence and is neutral about the extent to which that competence is innate.

[29] Much the same will go also for acceptability intuitions that are not of this sort and are partly pragmatic: they are central-processor responses to the data, laden with pragmatic theories as well as a linguistic one (Section 3). And these intuitions may well be reliable, albeit not nearly as useful because of the pragmatic intrusion.

good reason to suppose that these reflecting 'the linguistic wisdom of infallible, evidence for linguistic th

Finally, what about intuitions e ments?[30] In these experiments, orc two word strings is 'worse'. Since a certain hypothesized syntactic pragmatic factors. So we can be judgments, in contrast to simple j responding only to grammatical fa feature: they are as close to theory laden with a theory, even if c the experiment (see Reply to Obje of which string is worse *grammati* right.

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Although the intuitions discussed linguistics should mostly rely on are the linguists are the most expert. T the simple cases to theoretically in the barn fell' and 'Who do you wan at identifying items with and with biologist's skill at identifying items is likely to be better than the folk' linguists have firm, and surely corr many sentences, and about some r not.[31] Linguistic theory is, as ling

[30] See (Crain and Thornton [1998]) for a he

[31] Subjects in an experiment (Spencer [197 gorized as clearly acceptable or unaccept

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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.

Finally, what about intuitions elicited in ingenious 'minimal-pair' experiments?[30] In these experiments, ordinary speakers are asked to say which of two word strings is 'worse'. Since the two strings differ only in that one fails a certain hypothesized syntactic constraint, the experiments control for pragmatic factors. So we can be fairly confident that these comparative judgments, in contrast to simple judgments of acceptability (goodness), are responding only to grammatical facts. And the judgments have another nice feature: they are as close to theory-free as one could get. Still they are lightly laden with a theory, even if only with a theory constructed during the experiment (see Reply to Objection 2 in Section 6); they are judgments of which string is worse *grammatically*. And the judgments are likely to be right.

In sum, it is obvious that a speaker's own linguistic competence plays some role in the intuitive judgments she makes about the grammatical properties of expressions in her language. On the received linguistic view, the competence supplies *information* about these properties. On the modest view I am urging, it supplies *behavioral data for a central-processor judgment* about those properties. In particular, the grammatical (sometimes partly grammatical) notions that feature in these judgments are not supplied by the competence but by the central processor as a result of thought about language. Similarly, the notion of *following from* that may feature in intuitive judgments about thinking is not supplied by the competence to think but by the central processor as a result of thought about thinking. In neither case does competence have a voice.

Although the intuitions discussed are likely to be right, the intuitions that linguistics should mostly rely on are those of the linguists themselves, because the linguists are the most expert. This is particularly so 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?' The linguists' skill at identifying items with and without a syntactic property like, say, the biologist's skill at identifying items with and without a biological property, is likely to be better than the folk's because their theories are better. Thus linguists have firm, and surely correct, intuitions about the acceptability of many sentences, and about some matters of co-reference, that the folk do not.[31] Linguistic theory is, as linguists are fond of pointing out, in good

[30] See (Crain and Thornton [1998]) for a helpful discussion of experiments of this sort.

[31] Subjects in an experiment (Spencer [1973]) considered 150 sentences that linguists had categorized as clearly acceptable or unacceptable. The subjects disagreed with the linguists over

shape, far better shape than semantic theory. As a result of their incessant observation of language, guided by a good theory, linguists are reliable indicators of syntactic reality; analogously, biologists are reliable indicators of biological reality. So it is appropriate that linguists do tend to rely on the intuitions of other linguists, as we have already noted (Section 2).

To say that intuitions, whether those of the linguists or the folk, are good evidence is not to say that they are the only, or even the primary, evidence. Indeed, we can look for more direct, less theory-laden, evidence by studying what the intuitions are *about*, the linguistic reality itself. In fact, there are many other sources of evidence, as I have pointed out in Section 2. If this is right, theory construction in linguistics *could proceed without any appeal to intuitions at all*. This is not to say, of course, that it *should* so proceed. I have accepted that these intuitions, particularly those of the linguists, are often good evidence. So, they should be used. Intuitions are often a very convenient shortcut in theorizing.

It is time to compare my modest explanation of the evidential role of linguistic intuitions with the standard linguistic one.

6 Comparison of the modest explanation with the standard Cartesian explanation

I shall start by replying to two objections to the modest explanation. I shall then give some considerations against the standard one. I shall conclude by claiming that the modest one is better.

Objection 1.[32] Intuitions about touch typing, thinking, and the like are not the right analogies for linguistics intuitions. Rather, the right analogy is with intuitions about perceptual experiences, for example, the intuitions aired in illusory situations: 'It looks like there is water on the road' when experiencing a mirage; 'The moon looks larger when it is close to the horizon' when experiencing the moon illusion; and so on. These intuitions are immediately based on the outputs of a module (Fodor [1983]) and throw an interesting light on the nature of that module. They are not covered by the modest theory. No more are linguistic intuitions.

Reply. (i) Perceptual judgments are not good analogs of linguistic intuitions. Consider the visual module. Its task is to deliver information to the

73 of these, either finding them unclear or giving them an opposite categorization. In another experiment (Gordon and Hendrick [1997]), naïve subjects found coreference between a name and a pronoun that preceded it unacceptable even where the pronoun did not c-command the name. This is one of several experiments where folk intuitions were discovered to be at odds with the linguists' and with Binding Theory.

32 An objection along these lines has been pressed on me vigorously by Georges Rey.

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central processor of *what is seen*,[33] information that is indeed the immediate and main basis for judging what is seen; our earlier 'That grass is brown', 'That person is angry', 'This is an echidna but that isn't' and 'a pig's jawbone' are examples of such judgments. Sometimes what is delivered is misinformation; for example, 'There is water on the road' when experiencing a mirage. Locutions like 'looks like' enable us to allow for the possibility of misinformation in reporting these deliverances, as Objection 1 illustrates. The language module has the tasks of language production and comprehension. The task of production is clearly not analogous to the task of the visual module, but the task of comprehension is: it is to deliver information to the central processor of *what is said*, information that is the immediate and main basis for judging what is said, for judging 'the message'.[34] So, intuitions about what the message is are analogous to intuitions about what is seen. But the former intuitions are not the ones that concern us: for, they are not intuitions *about the syntactic and semantic properties of expressions*. (The contrast is between the intuition that the message is that Tom thinks that John loves himself and the intuition that in 'Tom thinks Dick loves himself' 'himself' must refer to the same person as 'Dick'.) If the objection is to be effective, it would have to be the case that the language module *also* delivers information of those syntactic and semantic properties, information that would be the immediate basis for the intuitions that concern us. But the view that the module does deliver this information is, of course, precisely what is at issue, and so it needs an independent argument. Indeed if the language module did deliver this information it would be *dis*analogous to a perceptual module, as we shall soon see (Further Consideration 1). (ii) The outputs of the language module do indeed throw an interesting light on the nature of that module; consider, for example, the significance of garden-path phenomena in comprehension and slips of the tongue in production. But these phenomena are examples of language usage; they are not intuitions about the linguistic properties of the expressions that result from that usage. (iii) Although perceptual judgments are not good analogs of linguistic intuitions they are covered by the modest theory. I see them as paradigms of fairly immediate and unreflective empirical responses to phenomena, as my discussion showed (Section 4).

Objection 2.[35] The claim that the intuitions of the ordinary competent speaker are empirical observations deploying theory-laden linguistic vocabu-

[33] As Fodor says, 'information about the 'layout'... of distal stimuli.' ([1983], p. 45). He later speculates that it 'delivers basic categorizations' (p. 97), categories like dog rather than poodle or animal.

[34] Some 'pragmatic' abilities supply bases too, of course, determining the reference of indexicals, removing ambiguities, making Gricean derivations, and so on.

[35] Based on an objection Stephen Stich made in correspondence on (Devitt and Sterelny [1989]).

lary is at odds with the following phenomena. (i) Take an English speaker with near enough no education in linguistics and give her two lists, one of 20 clearly grammatical sentences of English and the other of 20 clearly ungrammatical word strings. The first group is labeled '*A*' and the second '*B*'. Now give her a set of strings, some grammatical some not, and ask her to classify each one as an *A* or a *B*. She is likely to classify them near enough perfectly. (ii) Next we give her a list of 20 pairs of sentences that are related as active to passive and a list of 20 pairs of sentences that are not so related. We tell her that pairs in the first group are related as *X* to *Y*, those in the second are not. We give her many more pairs, some active-passive related some not, and ask her whether they are related as *X* to *Y*. One again she is likely to classify them near enough perfectly. These intuitive judgments are not laden with any theory and deploy no linguistic vocabulary.[36]

Reply. What happens in these experiments is that the speaker either *learns* to use the terms '*A*' and '*B*' for her concepts <grammatical-in-English> and <ungrammatical-in-English>, and the terms '*X*' and '*Y*' for her concepts <active> and <passive>; or, more likely, she acquires these linguistic concepts while she is learning to use the terms for them.[37] These experiments are analogous to the classic concept learning experiments in psychology and to solving many puzzles in IQ tests. It is not part of my modest view of intuitive linguistic judgments that an ignorant person cannot easily learn to make them. Indeed, most of us *do* easily learn to make many of them in primary school. These judgments are typically about basic linguistic facts that are very epistemically accessible. The judgments are theory-laden, but probably not much more than many 'observation' judgments; for example, 'Grass is green'; 'Rocks are hard'; 'Elephants are bigger than mice'. Once one has acquired the necessary concepts, these judgments are easy to make; and the concepts are easy to acquire. On the Duhem-Quine Thesis, all judgments are theory-laden, but they are not all laden to the same degree.

So the modest explanation is still in good shape. Turn now to the standard one.

According to the standard Cartesian explanation, the intuitions of someone competent in a language are good evidence for a theory of that language,

[36] I assume that Fodor has something like experiments (i) and (ii) in mind in claiming:

> Normal human children are, as far as we know, quite extraordinarily good at answering questions of the form: 'What grammar underlies the language of which the following corpus is a sample (insert PLD here)?'

Yet, he claims, the children 'exhibit no corresponding capacity for answering questions about bagels' ([2001], p. 129). I would have thought they would do just as well in experimental analogs of (i) and (ii) for bagels. (The questions about both language and bagels would surely have to be much less sophisticated than Fodor's.)

[37] Similarly, I would say, subjects in 'artificial grammar (AG)' tasks in psychology (Reber [1967]) acquire the concept <grammatical-in-AL> where AL is the artificial language.

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because they are derived from her representation of the rules of the language in her language faculty. The explanation is appealing, because the idea of one sort of representation leading to another is familiar. Still, attention to the language in which competence allegedly speaks, and to how little it says, raised concern about the details of the causal-rational route from an unconscious representation in the language faculty to a conscious judgment in the central processor (Section 3). I shall now give three further considerations against the standard explanation.

Further Consideration 1.[38] According to the standard explanation, the language module delivers syntactic and semantic information about expressions to the central processor. If it did this it would be disanalogous to perceptual modules (as noted in Reply to Objection 1). For, if it did, the central processor would have direct access to information that the language module allegedly uses to fulfill its task of processing language. But nobody supposes that the central processor has direct access to analogous information used by perceptual modules to fulfill their processing tasks.[39] Thus, the visual module simply tells the central processor what is seen: something along the lines of brown grass, angry person, an echidna, a pig's jawbone, water on the road, and so on. It does not deliver whatever information the module may use to arrive at such conclusions; it does not deliver 'Marr's 'primal', '2.5 D', and '3 D' sketch' (Fodor [1983], p. 94).

Further Consideration 2. I have noted in the last section that the intuitions of linguists often differ from those of the folk. This should be an embarrassment for the official line. First, why would the intuitions be different if they were the voices of competencies in the one language? An easy answer would be that, to the extent of the differences in intuitions, the linguists do not speak the same language as the folk. But this is very implausible. It is also belied by evidence, both anecdotal and experimental, that linguistic intuitions change with a linguistic education.[40] It is odd that this education should interfere with the causal-rational process by which intuitions are allegedly derived from the underlying representation of linguistic rules. Second, and more serious, from the Cartesian perspective it seems that we should see this interference as the *contamination* of the pure voice of competence with theoretical bias. So, rather than relying on this contaminated

38 I am indebted to my student, David Pereplyotchik, for this point.

39 'central processors should have free access only to the outputs of perceptual processes, interlevels of perceptual processing being correspondingly opaque to higher cognitive systems.' (Fodor [1983], p. 60).

40 In one experiment cited in note 31, subjects with at least one course in generative grammar agreed more with the linguists than did 'naïve' subjects (Spencer [1973]). In another, subjects who were encouraged to reflect on a sentence rather than give an immediate reaction agreed more with the linguists (Gordon and Hendrick [1997]). Consider also Matthews' report in note 26.

evidence, linguists should be seeking the intuitions of the most uneducated folk. So, the actual practice of linguists is mistaken. In contrast, if my proposal about the place of intuitions is correct, the change of linguistic intuitions with education is just what we should expect. And the actual practice of linguists is fine. The educated intuitions are contaminated only in the way that all evidence is and must be: it is all theory-laden. Linguistic education should make a person a better indicator of linguistic reality just as biological education makes a person a better indicator of biological reality. Of course, a person educated into a false theory may end up with distorted intuitions.[41] But that is an unavoidable risk of epistemic life, in linguistics as everywhere else. We have no unsullied access to any reality.

Further Consideration 3. If a speaker represents the rules of her language then that representation must surely control her language use, whether or not her linguistic intuitions are derived from the representation. And, of course, the received linguistic view is that the representation does both control use and yield intuitions. Yet, there is persuasive evidence that it does not play both these roles. If it does not, then the intuitions clearly cannot be derived from the representation (because if they were the representation would have to play both roles). The evidence is to be found in the study of 'implicit learning', learning that takes place 'largely without awareness of either the process or the products of learning' (Reber [2003], p. 486). Contrary to a common view in linguistics, language seems to be a paradigm of such learning. Suppose that it is. Now, if implicit learning were largely a matter of acquiring representations of 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. Consider this summary by Broadbent et al. ([1986])

> Broadbent ([1977]) showed that people controlling a model of a city transportation system gave more correct decisions when they had practiced the task than when they had not. However, they did not improve in their ability to answer questions about the relationships within the system. Broadbent and Aston ([1978]) found that teams of managers taking decisions on a model of the British economy showed a similar improvement in performance after practice. Yet the individuals making up the team did not improve on multiple choice questions about the principles governing the economic model.' (p. 34).

[41] This is not to say that there is no limit to the distortions that education can bring. Here, as everywhere else, reality constrains theories and hence distortions.

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ons is correct, the change of linguistic
hat we should expect. And the actual
ıcated intuitions are contaminated only
nust be: it is all theory-laden. Linguistic
a better indicator of linguistic reality
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ıt is an unavoidable risk of epistemic
e. We have no unsullied access to any

ker represents the rules of her language
control her language use, whether or not
rom the representation. And, of course,
ıe representation does both control use
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the intuitions clearly cannot be derived
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to be found in the study of 'implicit
largely without awareness of either the
(Reber [2003], p. 486). Contrary to a
e seems to be a paradigm of such learn-
plicit learning were largely a matter of
ıat both govern the performance of a
ask, we would expect improvement in
rovement in intuitions. Yet that is not
ı task performance is dissociated from
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ple controlling a model of a city trans-
ct decisions when they had practiced
owever, they did not improve in their
the relationships within the system.
ınd that teams of managers taking
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. Yet the individuals making up the
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he distortions that education can bring. Here, as
and hence distortions.

The paper reports further experiments that confirm these results (and go beyond them). Thus, one experiment found 'no increase in verbalizable knowledge associated with [a] sudden increase in performance' (Stanley et al. [1989], p. 569).[42] So there is good evidence for thinking that what we implicitly learn in acquiring a language is not a representation of the rules from which we derive our intuitions. So we have good evidence against the standard explanation.

None of these considerations against the standard Cartesian explanation is decisive, of course. Still, taken together, they do seem to me to undermine its plausibility considerably. Furthermore, I think that these considerations, together with the case for the modest explanation, are sufficient to establish that the modest explanation is better than the standard one.

But there is one more important reason for preferring the modest explanation. It arises from the extreme *im*modesty of the standard explanation. The standard explanation rests on RT, a very powerful assumption about the mind. And the important reason is this: we do not need this powerful assumption to explain the reliability of linguistic intuitions. We can explain that reliability without positing representations of linguistic rules in the minds of competent speakers. Consider the analogous phenomena for typing and thinking. We can explain the reliability of intuitions about those processes without positing representations of the rules that govern the processes. Our explanations of these intuitions make do with cognitive states and processes that we are *already* committed to. These modest explanations seemed perfectly adequate for the job and, indeed, much more plausible than their representational rivals. Similarly, I am urging, the modest explanation in the linguistics case. Language is a very striking and important part of the human environment. It is not surprising that empirical reflection on linguistic data, aided by some education, should make people fairly reliable detectors of the most obvious facts about language. We are surely similarly reliable about other striking and important parts of the environment, for example, the physical, biological, and psychological parts. If we can explain the reliability without positing representations of the rules, we should do so.

In conclusion, the modest explanation is better than the standard linguistic one. So the standard one cannot be the basis for a successful abduction to RT. The evidential role of linguistic intuitions does not support RT.

[42] The evidence does not, of course, show a total dissociation of verbalizable knowledge and performance, just a far greater one than would be expected if the knowledge and the performance stemmed from the one representation of rules. Less dissociation was found in some other experiments (Mathews et al. [1988]) in which the verbalizations were largely descriptions of prior sequences of events in performing the task rather than expressions of knowledge of the rules governing the task. See also (Mathews et al. [1989]).

7 A nonstandard Cartesian explanation of the role of intuitions?

The standard explanation that I have rejected rests on RT. For that reason, I allowed that some linguists may not endorse this explanation (Section 1). Some may believe that the syntactic rules of a language are embodied somehow in its competent speakers without being represented (in the sense illustrated). Yet they are still committed to the Cartesian view that linguistic intuitions are the voice of competence, the view that competence alone provides information about the linguistic facts. How could this be so? Can we find what I called a 'nonstandard Cartesian explanation' of the evidential role of intuitions (and hence the basis for an abduction to the thesis that the rules are embodied without being represented)? I know of no such explanation and I do not think that any one will be forthcoming.

It helps to note first that, even if the syntactic rules were embodied without being represented, this would make no contribution to the *modest* explanation. According to this explanation linguistic intuitions arise from mostly reliable central-processor reflection on linguistic data. If the rules of the language were embodied and governed language processing they would have a role in producing the data that are thus reflected upon (abstracting from performance error). But this would not be a contribution to the explanation. There must, after all, be *some* embodied processing rules that produce the data but, so far as the modest explanation is concerned, it does not matter what rules do so. All that matters to the explanation is that the data are the product of competent speakers, whatever their competence consists of and however these data are produced. The explanation does not require that the psychological processing rules involve the syntactic rules of the language.

Any nonstandard Cartesian explanation must of course be different from the modest one. To be different, it must give the embodied but unrepresented rules a role in linguistic intuitions other than simply producing data for central-processor reflection. And it must do this in a way that explains the Cartesian view that speakers have privileged access to linguistic facts. It is hard to see what shape such an explanation could take.[43] 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? The earlier Further Consideration 1 (Section 6) comes into play again. Perceptual modules may well be governed by embodied but unrepresented rules. And the operation of these rules may yield information that guides the module in arriving at its message to the central processor about what is

[43] Graves et al. dismiss the possibility of such an explanation ([1973], pp. 326–7).

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perceived. Yet the central processor has direct access only to the message, not to any intermediate information involved in arriving at it. Why suppose that the language module is any different? 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 an answer to these questions and it seems unlikely that the future will bring answers. Even if we could answer the questions and come up with the required explanation, we would still need a persuasive reason to prefer that explanation to my modest one.

The standard Cartesian explanation of the evidential role of linguistic intuitions rests on RT. I have earlier produced reasons for rejecting it in favor of the modest explanation. A nonstandard Cartesian explanation will rest on the idea that the rules of the language are embodied without being represented. I know of no such explanation. I have just given some reasons for thinking that there is unlikely to be one. In sum, we have found no support for the idea that linguistic intuitions reflect information supplied by linguistic competence, no support for the idea that they are the voice of competence.

8 Must linguistics explain intuitions?

The Cartesian view of a speaker's intuitive judgments leads to the view that linguistics should *explain* speakers' intuitive judgments: 'If a theory of language failed to account for these judgments, it would plainly be a failure' (Chomsky [1986], p. 37).[44] In thinking about this we need to make a distinction.

The distinction is between *what the intuitions express* and *the fact that speakers have these intuitions* (Devitt and Sterelny [1989], pp. 520–1). Now, it is obvious that linguistic theory must explain linguistic facts. Insofar as linguistic intuitions are *right*, they express linguistic facts and so the theory must explain what they express. But insofar as intuitions are wrong, the theory has no concern with what they express and no need to explain

[44] See also (Lees [1957], p. 36; Chomsky [1969], pp. 81–2; Baker [1978], pp. 4–5; Dwyer and Pietroski [1996], p. 340). Consider also these analogous claims about semantics: 'accounting for our ordinary judgments about truth-conditions of various sentences is the central aim of semantics' (Stanley and Szabo [2000], p. 240). 'Our intuitive judgments about what A meant, said, and implied, and judgments about whether what A said was true or false in specified situations constitute the primary data for a theory of interpretation, the data it is the theory's business to explain' (Neale [2004], p. 79).

them.[45] Linguists assume that the intuitions are largely right. I have agreed. It follows that linguistic theory must indeed explain what these intuitions express.

What about the explanation of how speakers' come to have the intuitions? It is no more the concern of linguistics to explain this than it is the concern of biology to explain how folk come to have their biological intuitions or physics to explain how folk come to have their physical intuitions. These explanations may well be worthwhile and interesting but they would be part of *descriptive epistemology* (and hence part of psychology). And, if an intuition is right, there is no *special* epistemological interest in explaining a person's having it: we expect the folk to be fairly reliable detectors of facts about their environment, particularly where the facts are rather obvious. (The need for an explanation is greater when people have *false* intuitions; for example, religious ones.) In this paper, I have offered the beginnings of an epistemological explanation of linguistic intuitions along these lines.

Although the explanation of speakers having correct linguistic intuitions is part of epistemology not linguistics, it may be *epistemically relevant* to linguistics. For, the abduction that we have considered (but rejected) finds evidence for RT in an explanation of speakers' having those intuitions. But then it is no surprise to find that an epistemological view can be epistemically relevant to linguistics. *Anything* can be epistemically relevant to linguistics. That is a consequence of the Duhem-Quine thesis.

9 Conclusion

Linguists greatly exaggerate the evidential role of the intuitive judgments of ordinary speakers. Still these intuitions are good evidence for a grammar. Why are they? The Chomskian answer is that they are derived by a rational process from a representation of linguistic rules in the language faculty. I have argued for a different view that has the great advantage of being theoretically modest. Linguistic intuitions do not reflect information supplied by represented, or even unrepresented, rules in the language faculty. Linguistic competence supplies data for these intuitions, but the intuitions are not its voice. Rather, linguistic intuitions are like intuitions in general. They are immediate and fairly unreflective empirical central-processor responses to linguistic phenomena. This conclusion accommodates the evidential role that intuitions play in linguistics, without any appeal to embodied rules of the language. There may be good reasons for thinking that a speaker embod-

[45] This is not to say, of course, that the theory has no need to explain performance errors like the failure to parse centrally embedded relatives. False intuitions about a language are one thing, errors in using it another.

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