ON RESTRICTING THE EVIDENCE BASE FOR LINGUISTICS

C. Iten, R. Stainton, and C. Wearing

1 INTRODUCTION

This chapter will investigate two questions about the appropriate evidence base for linguistics. The first question is: *should the evidence base for linguistics be restricted a priori and in principle in any fashion?* This isn't the question of whether one should make reasonable guesses about where to look for evidence. Of course one should. The first question, rather, is whether we can determine in advance of inquiry, from the 'armchair' as it were, what evidence could not possibly play a part. The second question is: *should evidence from the cognitive sciences be excluded?* In particular, we will be interested in the status of evidence from mentalistic psychology and neuroscience. This second question is in part a special case of our first question: should the evidence base for linguistics be restricted *a priori* and in principle to exclude evidence from mentalistic psychology and neuroscience? At the same time, it includes the more practical question of whether, even if admitted in principle, there is good reason to think that such evidence will not actually play a part in linguistics. (An analogy may clarify the difference between the 'in principle' and the 'in practice' questions. In the novel *Dirk Gently's Holistic Detective Agency*, the eponymous detective insists that everything is connected, so that evidence about, say, where a missing cat has gone could come from anywhere. Thus Dirk would reject any in principle restriction on the evidence base for his detections. He adds, however, that because of the fundamental interconnectedness of all things, he begins each investigation with a client-financed fortnight on the beach in Bermuda — even if the investigation is about a lost cat in Surrey. Here he (implausibly) suggests that *in practice* all evidence is on equal footing.)

We will pursue these two questions by considering three *pro-restriction* answers to them — answers that favour restricting the evidence base for linguistic enquiry, specifically setting aside neuropsychological evidence. In particular, we focus on positions inspired by Jerrold Katz, W.V.O. Quine and Ludwig Wittgenstein. Our goal in discussing these negative answers will be to clarify two things: first, the views of language (and language study) underlying each answer, and second, how each of these views leads to the restriction of the evidence base. Our aim is not exegetical. Instead, we want to make clear to linguists who *are* prone to use

Handbook of the Philosophy of Science. Volume 12: Philosophy of Psychology
Volume editor: Paul Thagard. Handbook editors: Dov M. Gabbay, Paul Thagard and John Woods.

evidence from the cognitive sciences why anyone would favour restrictions. As will emerge, restricting the evidence base may ultimately be a mistake; but we want to make clear why it isn't an obvious mistake. That's the burden of §§2-4. In §5, we will respond to the pro-restriction arguments, showing why they are less compelling than they initially appear. Given the problems with restricting the evidence base, we will conclude that such restrictions are not ultimately justified. We end with one quite unexpected way in which evidence from mentalistic psychology and neuroscience has recently illuminated English morphology, syntax and compositional semantics. The overall conclusion is that, though the alternative can be tempting, linguistics should proceed in the same way as other scientific endeavours, namely, by allowing that evidence from any quarter, including specifically from the domains of psychology and cognitive science, might in principle prove to be illuminating. What's more, though it may initially seem that neuropsychological evidence won't turn out in practice to be of great use, this impression is ultimately mistaken as well.

2 JERROLD KATZ: AN ONTOLOGICAL ARGUMENT FOR RESTRICTION

As a preliminary to our discussion of Katz, consider that there are at least three kinds of objects: concrete physical objects, mental objects, and abstract objects. Some examples of concrete objects include snow, cats, buses, the Empire State Building, and the black marks on this page. Mental objects include such things as pains, beliefs, memories, hopes, and dreams. By contrast, each of Beethoven's 5^{th} *Symphony*, Tolstoy's *War and Peace*, the American Constitution, and the number 8 is an abstract object — none is identical with any particular physical instantiation of it (a specific performance of certain sounds, a specific copy of the book, or piece of parchment, or squiggle on a piece of paper).[1]

One set of reasons for restricting the evidence base when studying a thing X arises from certain conceptions of what sort of object X is. In particular, if X is *not* the kind of object that certain types of evidence could (in principle) tell us anything about, then there is no need to bring those types of evidence to bear on the study of X. More than that, to do so just generates confusion. A specific example may help. Simplifying for expository purposes, Gottlob Frege [1884] insisted, *contra* a dominant attitude of the day, that numbers, numerical facts, and mathematical systems were not mental entities. That the square root of 698 is larger than 25 is a fact about numbers, not about human psychology. And no psychological experiments could refute a mathematical proof of this fact. Indeed, Frege's arguments have convinced most contemporary philosophers that numbers are acausal, atemporal entities. He further urged that, in light of these

[1] This does not exclude the possibility that something may belong to more than one of these categories. In particular, mental objects such as pains may also be physical objects (e.g., brain states). Nor do we wish to exclude concrete properties, mental properties, etc. Given present purposes, however, such metaphysical niceties need not detain us.

ontological considerations, only confusion was generated by reflecting on mental things when doing mathematics. Thus he insisted that mathematics be insulated, both in principle and in practice, from work in psychology: "...psychology should not suppose that it can contribute anything at all to the foundation of arithmetic. To the mathematician as such, these mental images, their origin and change are irrelevant" (87).

For Frege's line of thought to be plausible, it's crucial to make a distinction. We must distinguish between studying something and studying our psychological access to that thing. For example, studying the weather involves gathering information about such physical phenomena as winds, temperatures, and tides. But studying our psychological grasp of weather-facts involves looking at us — specifically, at how we acquire information about the weather, as well as how we store that information and how we put it to use. The latter is an investigation into our psychology, but the former is not. The same distinction holds for abstract objects: studying abstract objects such as numbers is a different enterprise from studying our psychological ability to grasp those objects. Studying numbers, which is what mathematicians do, involves discovering and proving mathematical theorems. Investigating how humans develop and use their mathematical knowledge (e.g., how children learn numbers), by contrast, is a job for psychologists. So, studying numbers and studying our knowledge of numbers are different kinds of enquiry. Moreover, if numbers are in fact abstract objects, then it looks as though the sort of enquiry that psychologists undertake into our knowledge of mathematics is not the right sort of enquiry to yield mathematical truths about numbers. In other words, psychological evidence about our knowledge of mathematics, though of terrific interest in itself, is not relevant to studying mathematics proper. (Knowing, for example, how children learn to add does not help us prove that 4 plus 3 equals 7.)

Now consider languages like English and Swahili. Suppose that such languages are also abstract objects, just as the number 8 is. One might infer, following Frege's lead, that psychology should have no place in the study of language either. This is precisely Katz's take on things. On this sort of view, the restriction of the evidence base follows from one's conception of what language is — the restriction derives from *ontological* considerations about the nature of language. That's the first sort of argument we will consider for restricting the evidence base in linguistics.[2]

The first step in the argument to the effect that languages are the wrong kind of thing to be illuminated by psychology is to show that languages are not concrete items. That is, they are not simple physical objects, consisting of non-mentally-specified physical marks and sounds, paired with the objective concrete objects which trigger the production of those sounds. Such a wholly concrete view of

[2]David Lewis [1975] and Scott Soames [1984; 1985] defend views that are similar to Katz's both in their treatment of languages as abstract formal objects and in drawing consequences from this for the evidence base appropriate for linguistics. As will emerge below, however, not everyone who takes the point that languages are abstract infers thereupon that psychological evidence is irrelevant to linguistics. For intermediate positions and discussions of them, see Barber [2003], Devitt [2003], George [1989] and Higginbotham [1983].

language was purportedly held by structuralist linguists such as Bloomfield before the 1950s; however, it was shown by Chomsky to be radically inadequate. It would take us too far afield to rehearse the key points; and we are especially uninterested in exegesis here. But notice, for example, that things which count as the same 'linguistic sound' can be physically very different. Thus, in (one dialect of) Spanish, "bee" and "vee" are different ways of pronouncing the very same linguistic sound. To take another example, think of the different acoustics of the word "vitamin" when said by a small child, a woman, an elderly man, someone with a bad cold, someone yelling, a tracheotomy patient, and so forth. Here again, what counts linguistically as 'the same sound' is acoustically diverse. These examples show that even the most 'concrete' aspect of language isn't entirely concrete or physical: what holds them all together as 'the same sound' is something more abstract. The same holds for marks. Consider all the different ways of writing a word: printed by hand (in countless different handwritings), drawn in the sand, written in cursive, typed in bold, italics, all-caps, etc. In fact, what all these ways of writing have in common is something about how language users process them. Similarly for the soundings of "bee" and "vee", or "vitamin". So treating language as a concrete physical object precludes the possibility of making the right generalizations by preventing abstraction away from the physical variation exhibited across different instances. Equally problematic for taking a language to be a simple concrete, non-mental thing, is that the same acoustic item can realize different 'linguistic sounds'. Thus we English speakers 'hear different sounds' in the middle of "writer" and "rider". But in fact, acoustically, the middle sound is the same: it's just that the surrounding material causes us to process this acoustic item differently in the two cases.

As noted, the first step in the argument is to show that languages are not concreta. Just about everyone who works in linguistics has granted this by now. More plausibly, languages might be something in the mind — e.g., whatever it is in virtue of which someone is able to produce the marks and sounds that we recognize as linguistic. This would capture the fact that 'linguistic sounds' aren't really acoustically individuated. It would also capture the absolutely essential fact that any given human language contains many, many more sentences than ever have been, or ever will be, produced. But, to come to the second step in Katz's argument, he has urged that mental items are still too concrete to be the sort of thing that a language is. Says Katz, words and sentences, like numbers, are atemporal and acausal entities. They are types, not tokens. As such, he adds, they are ontologically independent both of concrete matter and of human minds. The implications for our questions should be clear: if language is a mental object, then evidence from mentalistic psychology and neuroscience will be important to its study, for such evidence aims to reveal how the mind works; but if language is an abstract object, then it does not follow in the same immediate way that information about the minds or brains of language speakers tells us anything about that abstract object. Psychology might tell us how we come to know and use languages, but it doesn't seem able to tell us about language itself.

Why should one regard language in this way? What arguments support this view of languages as abstract objects not constrained by psychological considerations? Katz views Platonism as a largely neglected alternative to the mentalistic approach, an alternative which is not touched by Chomsky's arguments against the structuralist linguistics of Bloomfield and others. Those arguments clearly demonstrated the explanatory advantages of a mentalistic view of language over the view that language consists merely in a set of physical marks and sounds, but they were not simultaneously aimed at defending such a mentalistic view of language from a Platonist alternative, for no one really considered such a possibility. As a result, Katz argues, it remains an option until proven otherwise.

Taking off from the perspective of Platonism as a neglected alternative, he pursues two lines of argument. First, Katz argues that the empirical commitments of the mentalistic view of language get in the way of an adequate description and explanation of grammar: "there is a possibility of conflict between a demand that grammars satisfy an extrinsic, ideologically inspired constraint and the traditional demand that grammars meet intrinsic constraints concerning the successful description and explanation of the grammatical structure" [Katz, 1985, 196]. One such conflict, although offered by Katz as a mere possibility, arises if the 'psychologically preferable theory' (grammar) is inferior to others on methodological grounds. For example, if the way of representing grammar that English speakers seem to use proves to be less elegant than other possibilities, then the mentalistic approach must compromise such standards as simplicity. Faced with such a possibility, Katz suggests that "abandoning [this approach] is preferable to committing ourselves to such methodologically perverse choices" [1985, 198].

Second, Katz offers a Frege-inspired argument for the abstractness of language, and hence (allegedly) for the irrelevance of psychological evidence. We typically take truths about numbers to be necessary — they could not be otherwise — and psychology doesn't look capable of yielding truths of this sort. Psychology can only tell us about the limitations on our abilities to conceive of numbers as having certain properties, but it can't tell us about how numbers behave independently of how we are able to think about them. So, an explanation of human mathematical capability is incapable of explaining the necessity of mathematical truths. Empirical investigation just can't uncover such deep and abiding necessities. Katz extends this problem to the case of language by focusing attention on the grammatical (semantic) property of *analyticity*, which is exhibited by such sentences as:

- Nightmares are dreams, and
- Flawed gems are imperfect.

(These examples are from Katz [1985, 199]. For more extended discussion, see Katz [1981, Chapter 5].) Katz takes these sentences to be necessary — they are true come what may. Moreover, he takes their truth to depend on the combination of their meanings and their syntax. As a result of this dependency, their analyticity must be explained by the grammar of English, for it depends solely on grammatical properties. But, as a result of their necessity, the analyticity of these sentences cannot follow from a psychological explanation of the grammar of English — or so Katz insists — for psychology can only deliver "what human beings are psychologically or biologically forced to conceive to be true no matter what. But this is a far cry from what *is* true no matter what" [Katz, 1985, 200]. So, Katz effectively presents a dilemma: either one seeks a grammar that explains the necessity of the truth of these sentences, or a grammar that is psychological. One cannot accomplish both tasks at once, for the analyticity of these sentences is only intelligible if the languages are regarded as abstract entities, rather than mental entities.

Let's sum up so far. Katz argues on two different grounds that languages are abstract objects, on a par with numbers or algebras: first, if we don't treat languages as abstract, we risk not meeting certain intrinsic demands on a theory of language; second, if we don't treat languages as abstract, we won't account for certain necessities. He also urges that if languages are abstract, then neuropsychological evidence cannot bear on their nature. Thus his negative answer to both of our questions: the evidence base for linguistics should be restricted *a priori* and in principle; in particular, evidence from mentalistic psychology and neuroscience should be excluded not only in practice, but in principle. The key rationale for this answer is that the nature of an abstract system is one thing, the nature of our grasp of it is quite another. Psychology can of course tell us about the latter. But it simply cannot be relevant to the former. (See [George, 1989] and [Soames, 1984; 1985] for related points. Devitt [2003], in contrast, urges that the move from ontological status to evidence-restriction is unwarranted.) We turn now to a second, quite different, way of arriving at similar conclusions.

3 QUINE: A RADICAL EMPIRICIST ARGUMENT FOR RESTRICTION

Quine's reasons for restricting the evidence base are, unlike Katz's, more learning-theoretic than ontological. It is not because he views languages as abstract objects that he restricts the evidence base to "what is to be gleaned from overt behaviour in observable circumstances" [Quine, 1987, 5]. Instead, an anti-nativist commitment underwrites Quine's view of the evidence relevant to linguistic enquiry. Or so it seems to us. (Again, we are not interested in exegesis. The aim, to repeat, is to review initially attractive arguments for restricting the evidence base for linguistics.) To foreshadow the second line of argument as a whole: we read Quine as supporting anti-nativism on the basis of the success of the natural sciences; anti-nativism then has implications for what fixes the nature of the language spoken around one; and this, finally, entails that certain kinds of evidence should be avoided.

The starting point for understanding Quine's anti-nativism is his strong commitment to empiricism, i.e. to the view that our knowledge comes from experience. Insofar as our knowledge of language is simply one branch of our total knowledge, it too must derive from our experiences. One might wonder why this empiricist commitment constitutes a good starting point — why is it plausible that our knowledge comes from experience? One consideration, which constitutes a point in its favour if not an answer, is the fantastic success of the sciences that take empiricism as their foundation. The domains of physics, chemistry, and so forth, which take themselves to be based on and responsible to what we can observe (even indirectly) about the world, achieve ever greater predictive and explanatory power. Clearly, this sort of instrumentalist reasoning in favour of empiricism ('it works, so it's right') doesn't immediately establish its correctness, but it does suggest that the empiricism is a fruitful methodology to adopt.

Now for Quine, what goes for the scientific community goes for the individual as well. (See, for example, [Quine, 1969b].) Both start in the same place, and both use the same tools of investigation and confirmation. That's one key idea. The other is that empiricism doesn't, for Quine, just say that our knowledge requires experience. It says that the *only* evidential source for our knowledge is our observations of the world; we do not have any innately endowed ideas. This is not to insist that we don't make any contribution at all. Like earlier radical empiricists such as Hume, Quine allows for innate general-purpose mechanisms that operate on experience: innate quality spaces, innate mechanisms for forming associations, etc. What he and other radical empiricists reject is the claim that humans have innate information, evidence, beliefs, contents, reasons, ideas, etc. The only information from which knowledge derives, whether for the scientific community or for the infant, is information that comes from experience. In short, we see a move from views about the nature of scientific inquiry, and its success, to anti-nativism about individuals.

Given this general Quinean view about learning, let us consider the situation of the child who sets out to acquire a language. (This will support Quine's behaviorism.) The child, Quine holds, is in the same position as the empirical scientist: she must figure out how language works solely on the basis of the evidence presented to her. As he puts it, "each of us learns his language by observing other people's verbal behaviour and having his own faltering verbal behaviour observed and reinforced or corrected by others" [Quine, 1987, 5]. In this way, Quine takes the child to learn her language strictly and solely on the basis of the evidence presented to her (the sentences she hears others speak) and the corrections of her own attempts to utter appropriate sentences in one situation or another:

> Language is a social art which we all acquire on the evidence solely of other people's overt behavior under publicly recognizable circumstances. Meanings, therefore, those very models of mental entities, end up as grist for the behaviorist's mill [1969a, 26].

> ...even in the complex and obscure parts of language learning, the learner has no data to work with but the overt behavior or other speakers [1969a, 28].

The learner could not, then, be required to use information about what is going on in the minds or brains of other language users, because she has no access to this information. More importantly, she has no *need* of this sort of information, for all she needs to learn is which utterances it is appropriate to produce in response to which stimuli — and how exactly other speakers do this is not something she needs to know. No other evidence, such as the evidence of mentalistic psychology or the other of cognitive sciences, could be relevant here, for there are no additional facts for this evidence to uncover. More than that, looking at purported evidence of this kind can only lead to confusion and error, since it may (wrongly) suggest to the investigator that there are more facts than there genuinely are.

In sum, reflecting on the learning situation of the infant, Quine infers that the situation is (and must be) the same for the practicing linguist who aims to discover what we know when we know a language: "in psychology, one may or may not be a behaviourist, but in linguistics one has no choice" [Quine, 1987, 5]. Just as behavioural evidence is sufficient for the child, it must be sufficient for the linguist, because all that is required of our linguistic knowledge is that it "fits all external checkpoints", i.e. conforms to the regularities of stimulus-response pairings in the language community. As Davidson puts it, "The semantic features of language are public features. What no one can, in the nature of the case, figure out from the totality of the relevant evidence cannot be part of meaning" [1979, 235]. Thus we again have negative answers to our two questions: if Quine is right then we know already, prior to any empirical investigation, that the evidence base for linguistics should be restricted in principle; in particular, we know that neuropsychological evidence should be excluded not just in practice but in principle as well. (Note: It is well known that Quine is a behaviorist. But, it's worth stressing, his conclusion here goes far beyond the restriction of evidence to observable behavior: the only kind of behavior the linguist can make use of, if Quine's argument works, is behavior that an ordinary infant has access to. This excludes behavioral evidence from clinical cases, comparison with other languages, etc., as well as other physical evidence from autopsies, brain scans, and so on.)

4 WITTGENSTEIN: A 'NORMATIVE COMMUNITY PRACTICE' ARGUMENT FOR RESTRICTION

We have presented two views on language each of which leads to restrictions in the evidence base for linguistics. We have not tried to show that these views are ultimately correct, nor have we been much concerned with the question of whether the views discussed fit perfectly with the names we have associated with them. Our aim, rather, is to explain why imposing restrictions is neither perverse nor merely contrarian. We turn now to a third example. It is widely agreed that Ludwig Wittgenstein [1953] suggests that language is a kind of social practice. The view has been defended more recently by Michael Dummett. In this view of language we can discern three central features: first, language is community-based; second, it is a practice; and third, it has an irreducibly normative dimension. Weaving together these central features, the Wittgensteinian argues that linguistic meaning is constituted by *use*: there is nothing to a language beyond the way it is used in a given culture. In defending this view of language, the Wittgensteinian rejects the idea that words have meanings *beyond* how they are collectively used — human-independent meanings that are capable of autonomously adjudicating whether a particular use of those words is correct or incorrect. All that we have are the norm-governed *uses* to which words are put *by us*. (To be clear, by "use" Wittgenstein *et al.* do not mean the kind of thing that a scientistic behaviorist might: e.g., bodily motions of various types. They have in mind culturally constructed actions, norm-bound moves in a collective game. This is already one sense in which Wittgensteinians differs markedly from Quine.) Thus Michael Dummett writes:

> The meaning of [any][3] statement determines and is exhaustively determined by its *use*. The meaning of such a statement cannot be, or contain as an ingredient, anything which is not manifest in the use made of it, lying solely in the mind of the individual who apprehends that meaning: if two individuals agree completely about the use to be made of the statement, then they agree about its meaning. The reason is that the meaning of a statement consists solely in its rôle as an instrument of communication between individuals, just as the powers of a chess-piece consist solely in its rôle in the game according to the rules [1973, 216].

As we shall see, this view of language as *nothing more than* a normative community practice leads Wittgenstein *et al.* to abjure psychological evidence in studying language. (Or better, one can construct on the basis of certain of Wittgenstein's aphoristic remarks some *prima facie* good reasons for doing this. An old joke has it that, if you put three learned rabbis in a room, you'll get four interpretations of the Talmud. Say we, if you put one philosopher in a room, you'll get four interpretations of Wittgenstein!)

Let's examine how language might be an irreducibly normative, community-based practice by drawing an extended analogy with the waltz. First, the waltz is not something that you can do alone. It requires two dancers, and perhaps an orchestra to play appropriate accompanying music. In this way, it is an activity that is only possible with the participation and coordination of multiple people. At the same time, doing the waltz consists in moving in particular conventional ways in coordination with one's partner. It is distinguished by doing certain specific steps in time with specific kinds of music. Which steps constitute the waltz is a matter of convention, which reflects a second, even deeper, sense in which 'others' are required: a convention is the property of a group. Similarly, speaking a language, as Wittgenstein famously argues, is not something one can

[3] In the paper in question, Dummett is discussing mathematical statements. Thus instead of "any statement", he actually says "a mathematical statement". But what he endorses in this paper, he takes to apply to linguistic expressions generally.

do alone – language only makes sense as something that people share in a culture. As he says, "it is not possible to obey a rule 'privately': otherwise thinking one was obeying a rule would be the same thing as obeying it" [1953, §202]. So language, like the waltz, involves, and indeed requires, a community.

Third, we can see the essential normativity of the practice of waltzing in the fact that only certain performances of the waltz count as successful. There is a 'right way' to move your feet which will have you waltzing, while if you move your feet a different way, you may find yourself doing the fox-trot or simply shuffling around the dance floor looking slightly ridiculous. So there is a standard of correctness associated with waltzing: you have to do *the right steps*. Language works the same way: if you don't use the words of a language in the 'right way', you fail to count as a participant in the language-game. Imagine a person who consistently uses English nouns in ways that differ from other English speakers, say, they call dogs "horses", and snowballs "cars", and trees "turnips", and so forth. We would judge that this person is doing something *wrong* — she is failing to do what she is supposed to in order to count as speaking English. So, like the practice of waltzing, the practice of speaking a language comes with a normative standard — only some performances count as speakings of that language.

Finally, coming to the idea that the action is exhausted by these three features, notice that nothing more is involved in doing the waltz than following the norm-governed publicly observable steps. If we want to decide whether someone is doing the waltz, we need to look at their feet to see which steps they are performing. Suppose that they are indeed performing the steps of the waltz (i.e. the same steps that everyone else performs, the steps that are taken to be the steps of the waltz). How exactly their mind and body get their feet to follow those steps, rather than moving in some other way, is *irrelevant* to whether they are doing the waltz. Each dancer could be using a different part of the brain; indeed, something without a human brain could presumably dance the waltz. (Recall the Beatles' song *Being for the Benefit of Mr. Kite*: "And of course Henry the Horse dances the waltz".) All that matters is that the dancer's feet perform the right steps. (Says Wittgenstein: "a wheel that can be turned though nothing else moves with it, is not part of the mechanism" [1953, §271]. See also the invisible beetle in the box analogy, at §293.)

In a similar way, goes the idea, speaking a language involves nothing more than using the words of that language in just those ways that other speakers of the language do. For Wittgenstein, Dummett, *et al.*, what's going on in someone's head in virtue of which they manage to participate successfully in their community's language-game is irrelevant to their participation. All that matters is that their actions conform to the community's linguistic norms. Put otherwise, which words are to be used when is a matter of convention, and (so Wittgensteinians argue) there is nothing more to speaking the language than coordinating one's behaviour with that convention. What goes on 'inside' is just beside the point. (Other examples of cultural practices where what's going on inside seems irrelevant include voting for the Democratic candidate in your riding, depositing $10.00 in your checking account, and driving over the speed limit.)

If this view of language as an irreducibly normative, community-based practice is correct, then the implications for the evidence base for linguistics seem clear: evidence from such fields as psychology and neuroscience are largely irrelevant, for there is nothing in the heads of individual speakers that is particularly relevant to understanding something whose nature is *exhausted by* being a norm-governed community practice. More precisely, there may be facts about what people are capable of learning or remembering, and facts about how their psychology affects their social coordination, that are relevant to *how* one manages to speak. But there is an important sense in which what is going on in speakers' heads is quite irrelevant to explaining *whether* they are participating in the language-game correctly. Their correct participation amounts to nothing more than their conforming successfully to the norms of the game, and we can assess their success best simply by observing their 'outside'.

So it seems to follow from the Wittgensteinian's view of language that the evidence base appropriate to linguistics is restricted to observations of "moves in the language game". Information about the psychology of speakers, while it might seem capable of illuminating how that behaviour is produced, is at the very least always trumped by, if not plainly irrelevant to, what we learn from observation of speech.

To conclude our discussion of these pro-restriction answers, let's briefly contrast the ways that Katz's, Quine's, and Wittgenstein's views lead to restrictions of the evidence base for linguistics, and in particular, how evidence from the cognitive sciences is excluded both in practice and in principle. For Katz, such evidence is irrelevant because the nature of language is such that language doesn't 'come from us' — languages are abstract objects independent of mind and matter. As a result, evidence about our psychology cannot illuminate the nature of language. For Quine and Wittgenstein, by contrast, language does 'come from us', but the evidence of psychology and neuroscience is nonetheless irrelevant to its study because of *the way* in which each of them thinks language comes from us. For Quine, the radically restrictive learning conditions of the infant lead to constraints on what evidence could be relevant to studying language. For Wittgenstein and his followers, such constraints emerge from the view of language as nothing more than a norm-governed cultural practice — from meaning being exhausted by use.

5 AGAINST RESTRICTING THE EVIDENCE BASE

Our central questions are (i) whether we should impose *a priori* and in principle restrictions on the evidence-base for linguistics, (ii) whether in particular such restrictions should be imposed on evidence from the cognitive sciences, and (iii) whether, even if allowed in as a matter of principle, there is reason to think that neuropsychological evidence won't in fact turn out to be relevant. (Recall Dirk Gently: he may be right that all evidence is *in principle* relevant to finding a lost cat; but he is surely wrong that a two-week trip to Bermuda is as good a way as any to begin the search.) The views of language and linguistics discussed in §§2-4 of this chapter all give a positive answer to our questions, and on all three counts.

In this section, we argue for the alternative view that no *a priori* restrictions should be imposed, and hence that evidence from the cognitive sciences — because evidence from any area of inquiry — is potentially relevant to linguistic inquiry. More than that, we will argue that such evidence has been and will continue to be relevant in practice — in a way that, say, the number of planets orbiting Alpha Centauri is unlikely to be. The game plan for the rest of the chapter is as follows. In this section, we respond to the pro-restriction arguments introduced above, and provide some novel anti-restriction arguments. In the next section, we offer a detailed example of how neuropsychological evidence has recently provided very surprising evidence about morphology, syntax and compositional semantics. (Many of the ideas below owe their origin to lectures by, and conversations with, Noam Chomsky. They do not, however, represent his views in this area. For his seminal writings on these topics, see especially [Chomsky, 1969, 1986, 2000, 2002].)

5.1 Replies to Wittgenstein

Recall the analogy, on the Wittgensteinian view, between a language and the waltz. Both are nothing more than social practices with conventions that govern their correct execution. From this viewpoint, psychological evidence about the internal states of participants in the practice looks irrelevant to studying the practice itself because what occurs internally has no bearing on one's successful participation in the practice — all that matters is what is outwardly displayed. So the only evidence that will be illuminating must come from observation of the practice.

There are two points to make about this view. First, a note about what practicing linguists do. Just as a matter of fact, linguists do not study human linguistic activity for its own sake, cataloguing patterns; instead, they are interested in explaining the *causes* of linguistic behaviour. An important cluster of these causes is taken to be internal to the humans producing them, and specifically, to be part of their cognitive apparatus, so that the linguist is seeking to explain something quite different from (and causally responsible for) the Wittgensteinian language-games. Hence, there is already a worry that these Wittgensteinian considerations don't speak to linguists.

Just because linguists do this, of course, does not show that what they are doing is right. Is this search for causes a mistake? It's not, if linguistics is any kind of science at all. That's because it cannot just be assumed that the theorist knows which phenomena *are genuinely linguistic*. (See [Fodor, 1981] on this.) To take a simple example, is the fact that people are unlikely to ever make a move in the language game with "The cat the dog chased died" a linguistic fact, or not? One school, inspired by Chomsky, would say that such avoidance is not a properly linguistic fact, because the cause is a curious and specific limitation of human short-term memory. Or again, other discoveries about human short-term memory constraints may well help to explain why our linguistic practices have the general form they do — say, why the sentences we utter are typically not hundreds of words long. Thus the use of certain specific constructions, and our use of language more broadly, can be informed by studying human memory — so as to tell us which features of the 'game' are really due *to language*. This alone suggests that the restriction of evidence to what we can observe about linguistic interaction is unnecessarily strict.

Here is a second argument in reply to the Wittgensteinian stance. It again assumes that linguistics is some kind of science. It is bad scientific practice in general to assume *a priori* that certain kinds of evidence are not relevant to one's enquiry. For example, assuming that only terrestrial data will be relevant to understanding the ocean's tides might seem plausible. After all, the tides are on the earth, so it seems *prima facie* reasonable that they should be affected only by terrestrial interactions. As it turns out, of course, the tides are caused by the gravitational attraction exerted by the moon, so evidence about the moon turns out to be vital for explaining the tides. But this was not obvious at the outset of the enquiry. In a similar way, it is bad practice to prejudge the question of what evidence is relevant to helping us understand language — even if we suppose that Wittgenstein is correct to characterize language as a social practice.

The fundamental insight behind both of the foregoing replies to Wittgenstein, Dummett, *et al.* (as we are reading them) is this: the job of the scientist is to find out how things are connected. Since we don't know this in advance — not least because we don't ourselves fix how they are connected — it's not a good idea to rule out *a priori* that certain connections hold, sealing us off forever from looking in such-and-such locales. More than that, what the evidence base for linguistics itself turns out to be depends on how things end up being connected. (Put into a slogan: What the evidence base is turns out to be an empirical question.)

True enough, as investigation advances we may make enough progress in finding the causes of linguistic performances that we reasonably hypothesize that certain *a priori* possible evidential linkages do not obtain — and this can lead us to prioritize our search in practice. This is what makes it possible to answer "No" to "Should there be any *a priori* and in principle restrictions on linguistic evidence ?" while answering "Yes" to "Is there good reason to think that evidence of such-and-such kind will not actually play a part?" Given this, the Wittgensteinian characterization of language may, for all that has been argued above, privilege the data gleaned from the observation of linguistic moves in the sense that such data is sought first, and arguably trumps information from other sources. But the view is not thereby exempt from the more general maxim against excluding neuropsychological data *a priori.*

We have been drawing lessons from the practice of ordinary science to respond to Wittgensteinian arguments: scientists look for causes, they don't presume to know at the outset which phenomena properly fall within the purview of their domain of inquiry and, related to this, they do not stipulate beforehand that certain connections simply cannot hold. But what if one denies that the study of language should be scientific? Some precisely read Wittgenstein as suggesting that the scientific approach is inappropriate here: it would be like a 'science of poetry' or a 'science of fashion journalism'. The grounds for restricting the evidence base could then be couched in terms of a slightly different analogy: it would be foolhardy indeed, when trying to arrive at a proper literary interpretation of Kurt Vonnegut's novel *Slaughterhouse-Five*, to do CT scans on monkeys looking at its notoriously peculiar title page. More than that, it's hard to see how literary criticism, or fashion journalism for that matter, could gain anything from neuropsychological findings about brains. And, taking this stance, one won't be at all moved by comparisons with other sciences — since the whole idea is that the study of cultural linguistic practices shouldn't be scientific at all. Truth be told, we are not sure how one can reply to a theorist who simply refuses to look at language scientifically. One can't even say: "Look how successful the science of linguistics has been so far", since to their eyes the whole discipline will seems confused. On a happier note, such an anti-theory Wittgensteinian also cannot make claims about how one ought ideally to restrict the evidence base for linguistics, since she would have us scrap the whole enterprise, in favour of another topic entirely. Put another way, as we suggested above, our questions are about linguistics in the sense in which it exists as a discipline — i.e., they are questions about what evidence those who publish in linguistics journals, teach in linguistics departments, get degrees in the area, etc., should appeal to. Specifically, the issue is whether practitioners of that discipline are misguided if they try to employ evidence from the cognitive sciences. It isn't really an answer to *this* question to say that one should simply abandon the science of linguistics altogether.

5.2 Replies to Quine

Like the Wittgensteinians, Quine is motivated by a commitment to emergentism, in the sense of taking languages to be human products — things that come from us, rather than being independent of us. At the same time, Quine endorses empiricism: he is very strongly committed to respecting both the results and the methods of the empirical sciences. As we read him, the former commitment underwrites his view that there cannot be more to a given language than what the child acquires, while the latter commitment underwrites his anti-nativism about language acquisition. From these two commitments, we suggested, Quine derives his view of how language should be studied: there can be nothing more to what she has to learn than what can be gleaned from external stimuli (there are no further linguistic facts); consequently, the linguist must confine herself to what can be learned from the evidence available to the child. Worse, appealing to other data is apt to mislead the linguist into thinking that such data decides questions about language that simply cannot be decided.

Before introducing the first difficulty for the argument, we need to make two distinctions about what "empiricism" means. First, there is a necessary versus necessary-and-sufficient issue. Some read "empiricism" as saying that sensory experience is necessary for knowledge. Others, whom we have called "radical empiricists", think that — aside from innate physiological structure and general-purpose mechanisms such as forming associations — sensory experience is both necessary and sufficient for knowledge. Second, some read "empiricism" as a label for a doctrine in philosophical epistemology — a doctrine about which kinds of data support which kinds of theories. Others read it as a label for a view about how human infants develop psychologically. Since these distinctions cross-cut, we have four possibilities:

SENSES OF "EMPRICISM"	Experience is Necessary	Experience is Necessary and Sufficient
An Epistemological Doctrine about Justification of Theories	A	B
A Psychological Doctrine about Infant Development	C	D

With these distinctions in place, note that Quine adopts empiricism as a thesis about both epistemic justification for theories *and* infanto;e psychological development. At the same time, he takes both the developing infant and the theorizing scientist to be subject to the 'necessary and sufficient' demand – their knowledge does not merely *require* experience, but, general learning-theoretic machinery aside, comes *entirely* from experience. In sum, Quine endorses both B and D from the table above. More than that, if we read him correctly, then Quine understands B and D to be part and parcel of one doctrine, empiricism.[4]

With this terminological clarification in hand, let's revisit the argument from §3. The fact that there is a good reason to be an empiricist about epistemic justification, namely, the success of the empirical sciences, does not give us a reason to be an empiricist about human psychological development. We cannot assume that empiricism is the correct approach to one just because it looks like the correct approach to the other. Thus we cannot infer from the truth of A or B to the truth of C or D. The second problem is that the success of science supports only the 'necessary' side of the first distinction — the view that knowledge requires

[4]Again, our aim in this chapter is to explain what might be tempting about restricting the evidence base for linguistics. It's not exegesis. Nevertheless, since readers may wonder why we read Quine this way, we should at least say this: we can find in Quine no genuine effort to defend, on the basis of empirical evidence, anti-nativism about human development. This suggests that he takes empiricism, which is non-negotiable for him, to immediately license, as a sub-doctrine, anti-nativism in psychology: "Two cardinal tenets of empiricism remain unassailable, however... One is that whatever evidence there *is* for science *is* sensory evidence. The other... is that all inculcation of meanings of words must rest ultimately on sensory evidence" [Quine, 1969, 75].

observation or experience, rather than the view that nothing beyond observation or experience is required. That is, what observations of successful sciences support is A, not B. As a result, Quine cannot draw any reasonable inference from the success of the sciences to the extreme view that the child is an empiricist-learner whose only source of information is experience.

The first point, then, is that Quine hasn't supported one of his key premises. A further problem with this aspect of Quine's account is that his (empirical) claim that the child learns her language *solely* on the basis of her observations of others and their corrections of her behaviour is probably false. Poverty of stimulus arguments have been offered by Chomsky and many others to show that such data cannot possibly suffice to explain how the child comes to speak a language. What the child *acquires* (e.g. how to form yes-no questions properly, or how to group a wide range of sounds into instances of a single word) is not something that she could plausibly infer from the very limited range of data that she typically has. Instead, it seems that she herself contributes some information which combines with the data that she receives so that she ends up knowing how to speak a language. (See Laurence and Margolis [2001] and Stainton [2006] for recent discussions of the Poverty of Stimulus arguments, including philosophical challengers.)

However, even if anti-nativism were true, and the child contributed nothing of substance to her own language-learning, it still would not follow that the practicing linguist is in the same situation as the child with respect to how to learn about language. This constitutes a final problem for Quine's restriction of the evidence base to observable behaviour in the language group: it simply is not the case that how the studied object develops (in this case, how the child comes to learn her language) limits how a group should study that object. Consider carrots. Suppose, counterfactually, that carrots developed solely on the basis of nutrients that they receive from outside themselves. This still would not mean that the scientist who wishes to understand how carrots develop should study only their nutrients. He should study the carrots themselves, too, their effects on various things, how they rot, and perhaps even other root vegetables. Applying this to the case of language, even if the child were an empiricist-learner, this is not a reason why the scientist studying language should be restricted to the data and methods that are available to the child.

To sum up, Quine's constraints on the evidence base available to the linguist are rendered problematic for several reasons. First, radical empiricism about human development is not supported by reflections upon the methods of the successful sciences. Second, it is unlikely that children are empiricist-learners of language. Third, even if they were, this would not require that the scientist should study language using only the resources available to the child. As a result, Quine's view does not ultimately give us a principled reason to restrict the evidence base available for linguistics.

5.3 Replies to Katz

We end this section of rebuttals by recalling Katz's reasons for thinking that the evidence base for linguistics should be restricted. As discussed in §2, Katz takes human languages to be abstract objects, knowable by us and yet independent of mind and matter. As such, he claims, their nature will not be revealed by psychological evidence, for their nature is not dependent on our psychology.

Notice first off that Katz systematically takes mathematical objects as his point of comparison. However, his argument for regarding languages as abstract objects don't establish that they are of the mathematical kind. As discussed in §2, the range of abstract objects also includes such entities as Tolstoy's *War and Peace.* Unlike numbers, this abstract object seems to depend crucially on the activities of a particular human — there is only as much to *War and Peace* as Tolstoy put into it. In systematically comparing languages with numbers and algebras rather than abstract objects like *War and Peace* or the American Constitution, Katz exaggerates the extent to which abstract objects are divorced from the activities of people. This is especially important because of how abstract *creations*, unlike abstract mathematical objects, can be illuminated by psychology: accepting that novels such as *Slaughterhouse-Five* and *Moby Dick* are types does not turn American Literature into a branch of mathematics; and certainly some psychological evidence will bear on the nature of these novels, e.g., the mental states of Vonnegut and Melville when they wrote them. More broadly, insofar as these abstract things emerge from us, we can psychologically investigate *the thing known* and thereby find out about it, the abstract thing. (See [Higginbotham, 1983] for more on this.)

Katz will object that there is a good reason to assimilate natural languages to mathematical systems, viz. that both give rise to abiding necessary truths. The argument, recall, rests on the claim that only by seeing languages as mathematical can one account for properties such as analyticity, which Katz claims are exhibited by some linguistic items. Such properties, he argues, simply cannot be explained by regarding language as a mental object along the lines defended by the cognitivist. Our reply takes the form of a dilemma. Either there are language-based necessities or there are not. If, as Quine [1953a; 1953b] has suggested, there really aren't any, then there is nothing for Katz-style Platonism about language to account for. If, in contrast, there are language-based necessities, then it's plain enough how the emergentist can account for them: they are language-based! The reason why it's necessary that flawed gems are imperfect, for example, is because of something we English speakers did: we made "flawed" semantically related in the right way to "imperfect". (Of course if such necessities exist *and* humans cannot bring them into existence with their conventions, then a problem remains. However, Katz hasn't given us reasons to think that. See [Boghossian, 1997] for contemporary discussion of the larger issues surrounding analyticity.)

The first argument against treating languages as mathematical entities is that this misses their dependence upon us. A second argument against Katz's Platonist view is this: while conceptually possible, it is not interesting. Fodor [1981, 158–9], for example, describes Platonism as, on the one hand, "unassailable", but on the other hand, as something that "nobody is remotely interested in". Fodor notes that Platonism about language does not *preclude* the pursuit of psycholinguistics, but simply remains indifferent to its findings. Insofar as Platonism dedicates itself to finding out which grammar(s) is consistent with the intuitions of speaker/hearers (which everyone grants to be relevant), it may run in parallel, up to a point, with psycholinguistics. At the point where they diverge, where the psycholinguist rejects certain candidate grammars on the basis of data *other than* intuitions accepted by the Platonist, Fodor complains that the Platonist's project ceases to be worthy of pursuit. The reason is that the Platonist view of language rests on *stipulation* — what it chooses to study is not determined by how the world is, but by the investigator. Obviously, the investigator is at liberty to choose which *language* she investigates; but more importantly, on Katz's view she seemingly also has considerable liberty in determining which data, and even which intuitions, are properly linguistic, and hence will count as the facts to which her grammar must be responsible. Unlike the psycholinguist, whose restrictions (if any) must be (eventually) empirically grounded, the Platonist linguist is not so constrained because what she aims to explain is not assumed to have an empirical foundation. Thus, she may study whichever language-intuitions pair she chooses.

Katz [1985, 177] has complained that the objection begs the question against the Platonist, to the extent that it forgets that the Platonist is not interested in 'speaker/hearer capacities' at all. If, as the Platonist insists, languages are abstract, but nonetheless real, objects, then ignoring language-intuition pairs that are empirically grounded in experimentation in favour of others that are not is no failing: "it can no more be to the discredit of Platonism that it doesn't pay attention to psychological capacities than it can be to the discredit of Fodor's psychologism that it doesn't pay attention to abstract objects" [1985, 177]. However, Katz underestimates the extent of stipulation that is involved in delineating the scope of the Platonist's enquiry into any particular case. Specifically, recalling a point made when discussing the Wittgensteinian view, Katz overlooks how much stipulation will in fact be required to isolate the 'purely linguistic facts' that he takes the Platonist linguist to be concerned with. Consider, for instance, the following list of facts:

- English speakers produce "Ouch!" instead of "Ay!" when injured
- We seldom encounter sentences like "The cat the dog chased died"
- One can request the salt by saying "Can you reach the salt?"
- When one says "Stella and Wayne just got married" one usually means that they got married to one another
- Certain linguistic greetings are more polite than others
- In writing, a sentence starts with a capital and ends with a period

- Every utterance of "I cannot speak a word of English" is false
- The sentence "Juan's mother and father like themselves" cannot mean that Juan's mother, his father and Juan all like themselves
- "The if two Uruguayan" is less grammatical than "The man seems sleeping", though both are ungrammatical to some degree
- "I lifted up him" sounds odd but "I lifted up John's dog" does not

The question is: which of these facts are properly linguistic? Katz's Platonist must account for all the *linguistic* facts about a given language... but first he must identify them. It is not clear how the Platonist is able to do this — there don't seem to be any constraints. One simply stipulates.

This point also speaks to Katz' complaint that mentalistic approaches may impose extrinsic demands on grammars. First off, it is not clear that Katz can draw a sharp line around the 'purely linguistic' properties, so that the 'conflicts' Katz points to may be quite difficult to cash out in a principled way.[5] More specifically, he takes it to be a point against such views that, for instance, what they term the correct grammar (the one speakers actually know/use) might be messier than other descriptively adequate competitors — say, for reasons to do with the evolutionary development of linguistic abilities through the co-opting of pre-existing non-linguistic mechanisms. But if we do not assume that the 'purely linguistic' facts can be found except by looking at psychological facts, then grammatical messiness may simply be a reality.

The final objection to Katz takes the form of a differential certainty argument. This is a kind of argument where one must choose between two propositions, typically one supported by abstruse philosophical reasoning, and one that seems immediately obvious. The strategy is to say, "Though we aren't sure what it is, there must be *something* wrong with the abstruse argument, because what it seemingly supports is far less plausible than other things we know". (An example: following Zeno, one might argue that because any distance can be divided in half, one cannot really walk across a room. Long before knowing what was wrong with this line of thought, a differential certainty argument can show that there has to be *something* wrong — for people cross rooms all the time.) Here is how the argument form applies here. Katz suggests that if languages are ontologically abstract, then psychological evidence is not relevant; he then affirms the antecedent of this conditional. His conclusion is that psychological evidence should be avoided. But this conclusion conflicts with the patent relevance of certain psychological facts to the nature of language. Take two short examples. (A much more detailed

[5] Louise Antony [2003] pursues this line of argument against Soames [1984], who, like Katz, holds that one can distinguish the 'purely linguistic' properties *a priori*. However, Soames differs from Katz in taking linguistics to be an empirical discipline, such that his arguments (Antony suggests) are more vulnerable to her criticisms. In any case, Katz would need to defend a principled distinction between linguistic and psychological properties, without *assuming* Platonism, in order for the 'conflicts' to constitute problems for the mentalistic approach.

one appears in the next section.) We'll begin with a very obvious real-world example. Even those who say that they aren't interested in psychology don't genuinely ignore it in practice. For instance, *everyone* who undertakes to describe a language takes on board that the language must be finitely specifiable, because otherwise it would be unlearnable. Everyone pays heed to learnability precisely because it's obvious that a grammar's being learnable is relevant to whether it's the grammar we use/know. But, of course, being learnable is a psychological requirement. Here is a more exotic case. There is a rather rare anomia that attacks the ability to use proper names, but leaves the ability to use quantifier phrases (e.g., "every dog", "several computers", "all hamburgers") more or less intact. Given this, consider a thought-experiment. Suppose we found out that this anomia leaves definite descriptions such as "The king of France" intact as well: this phrase is spared in just the way "Some king of France" is. This data-point clearly would not prove that definite descriptions are more like quantifier phrases and less like names. But surely a theorist who supposed, with Frege [1892], that definite descriptions are name-like would have to at least explain away the data. As a linguist, she could not just say, "That is irrelevant to my topic". Given such examples, differential certainty now comes in. Which is more secure, our sense that the anomia and learnability are relevant to linguistics, or our confidence in Katz' abstruse reasoning? The answer is clear. (Of course, this argument doesn't tell us exactly where Katz went wrong in his reasoning. On that issue, however, see above.)

Before moving on, it's worth making a point about the relationship between what one takes the ontology of languages to be and one's view on methodology. Katz, for example, sometimes writes as if the very fact that languages are abstract entities means that psychological evidence will not be apposite. Certain Wittgensteinians equally seem to suppose that just because a language is, ontologically speaking, a normative cultural practice, it follows that psychological evidence should be avoided. Ontological reflections can also *support* an ecumenical methodology, of course: if languages just are mental items, then of course mentalistic evidence will be relevant both in principle and in practice.[6] This recognition can then encourage the idea that it's *only if* languages are mental items that mentalist evidence will be so relevant. But, say we, it's not necessary, for evidence from all sources to be permitted, that one follow Chomsky and take languages to be internal psychological entities. To the contrary, the relationship

[6]By the way, taking languages to be mental items is not, we think, ultimately in conflict with taking languages to also be abstract or social in certain regards. Suppose, for example, that what determines which linguistic facts obtain is something directly about the minds of its native speakers. Still, those 'internal' facts will fix a cluster of 'external' facts — about which sentences mean what, which sentences are well-formed and to what degree, etc. (Put crudely, the intensionally correct mental grammar settles which other grammars are extensionally correct. See [Laurence, 2003] for more.) What's more, even supposing this is how the facts are settled, there is no bar whatever to writing grammars that don't track the psychology of native speakers, to capture the resulting external facts, for whatever purpose — and to doing it in a way that distinguishes correct versus incorrect grammars. (For instance, language instructors may want a correct grammar for English that is easy to teach to Malagasy speakers.)

between ontology and methodology is far less tight than this. What matters is not what kind of thing a language is — concrete, abstract, mental, etc. — as much as what languages are connected with and (especially) what they are constituted by. Specifically, if the nature of our languages emerges from us — if as the metaphysicians say it 'supervenes' on us — then even if languages themselves are abstract things, or social things, rather than mental things, mental evidence can easily be relevant. (Just think of the waltz, or *Slaughterhouse-Five*. Why exactly shouldn't evidence about the psychology of dancers, readers and authors bear on the nature of these?)

To summarize so far, we began by introducing three families of arguments in favour of restricting the evidence base for linguistics. The aim, recall, was to make clear why anyone would feel tempted to do this; it was not to provide a detailed exegesis of the theorists discussed. Having provided arguments in favour of restriction, we then responded to them. We hope to have shown that, though imposing restrictions is not absurd or fool-hardy, it is ultimately incorrect. (Further arguments along the same lines may be found in [Antony, 2003], [Laurence, 2003] and [Stainton, 2001].) Most of our rebuttals have been directed to the issue of restrictions *a priori* and in principle. It remains to consider in detail whether results from the cognitive sciences are likely, in practice, to afford important evidence. We address that issue next, by means of a detailed example.

6 A DETAILED EXAMPLE

The previous section gives reasons why evidence of a psychological kind ought not be excluded in principle. Here we provide a specific example of how data from the cognitive sciences in fact play a useful role.

A standard view about the morphology of regular verbs like "start" and "link" is that their compound forms are built from the root plus a suffix. For instance, "started" is built from two elements: "start" [the root] and "-ed" [the suffix]. Put in terms familiar to philosophers and logicians, this amounts to something like: English contains syntactic axioms for "start" and for "-ed", and a formation rule that says how to put them together. There is then a semantic axiom for "start" and one for "-ed", and a compositional semantic axiom that captures the meaning-effect of combining these two items. The standard view says something different about irregular verbs like "swim", "hit" and "fall". Morphologically speaking, they apparently don't have a suffix; and in particular, their past tense forms ("swam", "hit", "fell") pretty clearly don't contain the suffix "-ed". What the standard view says about these is that they are atomic. Again, putting the point in terms of axioms and rules, the idea is that there is a special *axiom* that says what the past form of "hit" is, and a corresponding semantic axiom with its meaning.

This standard view isn't the only option. An alternative is that this supposed difference between regular and irregular verbs is an illusion. Not because "swam", "hit" and "fell" are compounds built from the "-ed" suffix; rather, because "started" *et al.* are equally atomic, both in terms of how they are formally built up and in terms of how their meaning gets fixed in the language. (Note: the issue isn't whether the present and past tense forms of a regular verb are *semantically* related. Of course they are. The issue is what kind of rules account for their semantic relatedness: Are they semantically related because they share a morphosyntactic element — e.g., because the very same item "start" is brought to bear in deriving the meaning of both "start" and "started"? Or are they semantically related for some other reason?)

In short, we have two hypotheses about English. One, corresponding to the standard view, says that there is one set of rules for regular forms, and a different set of rules for irregular forms. The other says that there is one set of rules that applies to all verbs — so that the supposed regular/irregular distinction isn't linguistically important. Having introduced this disagreement about English, we will now explain some recent experiments in (neuro)psychology. The point, given our questions, will be that the results of these neuropsychological experiments pertain evidentially to the properly linguistic question.

Numerous psychologists have investigated how we process and represent past tense forms. They have looked in particular at whether native speakers of English, to arrive at the meaning of a sentence like (2), process their language in terms of (1):

1. *past tense = verb stem + -ed*
2. John walked past the Eiffel Tower.

There were two psychological hypotheses to consider. Hypothesis 1 has it that regular past tense forms are processed in accordance with the rule in (1), while irregular forms are stored separately in their entirety. An alternative hypothesis, Hypothesis 2, is that all past tense forms, regular and irregular, are stored in the lexicon individually and retrieved whole, without a rule like (1) being represented anywhere. A good way of testing this is via priming experiments. The idea behind such experiments is that recognizing a word is easier and therefore quicker after one has just been exposed to the same word or a closely related one. For instance, you are quicker to recognize "cat" as a word if you've just read "cat" a moment before. If Hypothesis 1 is right and regular past tenses really are processed as *verb stem + -ed*, one would expect the past tense to prime the stem to the same extent as the stem itself, because processing the past form necessitates accessing the stem form. To give an example, if you process "walked" as "walk + -ed", reading "walk" immediately after "walked" should make you quicker to recognize "walk" as a word, much the same way as reading "walk" immediately after "walk" would. With irregular past tenses, on the other hand, one would not expect the same amount of priming if the first hypothesis is correct. By contrast, Hypothesis 2 would predict no difference in priming effect across 'regular' and 'irregular' verbs: according to that account, processing "walked" does not require you to access "walk" any more than processing "went" requires you to access the actual word "go" — and thus there should be no difference in how fast you recognize "walk" after "walked" and "go" after "went".

Stanners *et al.* [1979] conducted such a priming experiment using reaction times (i.e., how long it takes a subject to recognize a letter or sound sequence as a word) and the results they found support Hypothesis 1: regular past forms prime the stem to a similar extent as the stem itself, while there is less priming in the case of irregular past forms. Applied to an example, subjects recognize "walk" as a word more quickly after reading "walked" than they recognize "go" after reading "went". Subsequent studies replicated these results for regular forms, i.e. they all agree that regular past forms prime their stems. However, Münte *et al.* [1999] point out that the results for irregular forms are less clear. For instance, Kempley & Morton [1982] found no priming in the irregular cases, while Fowler *et al.* [1985] and Forster *et al.* [1987] report full priming. According to Münte *et al.*, this highlights that priming studies *that crudely test reaction times* are problematic and may not be getting at the right phenomenon. It could be, for instance, that the reason for priming in regular past tense forms is that the past forms (e.g. "walked") contain the stems (e.g. "walk") phonologically and orthographically, though the former is still genuinely atomic in terms of how it is stored. In contrast, the equally atomic irregular past forms (e.g. "went") don't resemble their stems (e.g. "go") phonologically and orthographically to the same extent.

Because of these problems, Münte *et al.* set out to do a priming study that doesn't measure reaction times. Instead, they made use of technology that measures electric brain activity. It is known that people's event-related brain potentials (ERPs) pattern in particular ways when they're exposed to a word, a picture or a face for the second time (e.g. [Rugg, 1985]). In other words, measuring the electric activity in people's brains gives an indication of whether they've processed a particular form (linguistic or otherwise) before. Applying this to the question of past tense morphology, Hypothesis 1 about processing would be supported if verb stems following regular past tense forms led to ERPs characteristic of repetition, while verb stems following irregular past forms didn't. Hypothesis 2 would be supported if neither caused the brain activity typical of repetition.

A main goal of Münte *et al.*'s study was to avoid the possibility that the priming effect in regular past forms came down to the formal resemblance between regular past forms and verb stems. They wanted to make sure that the differences in observed effect, if any, was due to a difference in how morphology, syntax and semantics are processed — not because of very superficial differences between regular and irregular forms. For this reason, they did not expose subjects to the prime (e.g. "walked") and the target word (e.g. "walk") in immediate succession. Instead, they presented them with lists that always had between five and nine words between the prime and the target word — because previous evidence suggested that formal priming was a short-term effect. They also made sure to present the prime and the target word in different case letters, e.g., if the prime was written "walked" the target word would be written "WALK". Finally, they introduced two control conditions. In a phonological control condition the target word was preceded by a word that did or didn't share its initial phonemes (e.g. "sincere"–"sin" vs. "board"–"sin"). The nonce (invented) word control included made up words that either followed a regular or an irregular pattern (e.g. "renited"––"renite" vs. "ploke"–"plike"). The rationale behind this was the following. If the priming effect in the regular past tense case rests on the superficial formal resemblance between the past form and the verb stem, the same effect should also appear in the case of phonological similarity (e.g. "sincere"–"sin") and regular nonce words (e.g. "renited"–"renite"). If the priming effect were due to purely semantic relatedness (e.g., if it were due to the fact that "start" and "started" both pertain to *beginning*, while "leave" and "left" both pertain to *departing*), it should be produced by both regular and irregular past forms. Should it turn out, however, that it's only regular past tenses that prime the verb stem, Hypothesis 1 would be supported: processing "walked", for example, would, indeed, involve accessing the actual morphosyntactic item "walk" and the suffix "-ed", and deriving the meaning of the compound from these items.

What Münte *et al.* found was the following. Regular past tense forms resulted in ERPs typical of repetition when the subjects were presented with the verb stems. Irregular past forms did not have this effect. Interestingly, the phonological control condition where a target word was preceded by a word sharing its first few phonemes also produced no priming effect. Similar results were obtained in the nonce word condition: the characteristic patterns of electric brain activity associated with repetition did not appear, irrespective of whether the invented words followed a regular or irregular pattern. In terms of our example, reading "walk" after "walked" produced the typical repetition ERP pattern, while reading "go" after "went", "sin" after "sincere", and "renite" after "renited" didn't. This suggests that the priming effect in the regular past tense cases cannot be due to a superficial resemblance between the past form and the verb stem (if that were the case at least one of the resemblances in the control conditions should have led to the same effect) and it cannot be due to semantic relatedness either (otherwise irregular forms should also have a priming effect). Instead, it seems clear that subjects do access the verb stem in processing the regular past form, but not in processing the irregular form. This supports Hypothesis 1, whereas Hypothesis 2 is not compatible with the evidence found by Münte *et al.*.

Those are the neuropsychological results. Now consider what these experiments suggest about English. If the standard view about the contrasting morphology, syntax and compositional semantics for regulars versus irregulars is correct, this psychological effect is immediately accounted for. If English contains a syntactic axiom for "start" and another for "-ed", and it contains a semantic axiom for "start" and another for "-ed", then someone who knows English knows all of these rules, and she would naturally employ them to understand "started". Since she uses the axioms for "start" to understand "started", the various psychological and neurological priming effects are independently predicted to occur. Making the point in a general way, claims about the inner workings of the thing-known have implications; in particular, such claims have implications for psychology and neuroscience. Now, the thing-known in the case at hand is a language. So, claims about the structure of a language will yield testable predictions in the cognitive sciences. To be clear, to affirm that there will be testable predications is not to suggest that, e.g., the results presented above out-and-out *prove* that English has this structure. There may be better explanations of the observed effects, or the effects may be shown to be illusory, and so on. The question, applied to our detailed example, is this: could a reasonable person inquiring into the morphology, syntax and semantics of English, who is not wholly blinded by ideology, react to these results by saying "I don't care. My view makes no predictions about processing"? We think not.

One last thing. It may be objected that the problem isn't so much using psychology to find out about the rules of English, but the very search for "the right linguistic rules" in the first place. Stich [1972] suggests, for instance, that from a mathematical point of view there will be more than one 'descriptively adequate' grammar for any given language. In other words, there will be more than one grammar that succeeds in generating all the well-formed expressions of a language from its primitive parts. To borrow a pair of examples from that paper, if A were a descriptively adequate grammar for English, then we could construct another descriptively adequate grammar, B, by adding a few superfluous rules to A, or by replacing one of A's rules with several distinct rules that collectively cover all the cases covered by the original rule.[7] Building on such reflections, one might insist that the only job a linguist should take on is describing the set of well-formed sentences: taking seriously the question of which axioms actually make up the language is already conceding too much. Continues the objection, given that the foregoing considerations about ERPs and the like don't speak to the nature of English *understood simply as such a set of sentences*, they aren't pertinent to the nature of the language after all. This is a familiar objection, but, following James Higginbotham [1985], we think it actually carries little weight. First, the notion of a set of well-formed formulae that we are trying to describe comes to us from the study of invented logical languages. In those languages, a formula is either fully well-formed or it is fully ill-formed, and ill-formed formulae lack meaning entirely. What's more, with invented logical languages there isn't an empirical issue about which formulae are well-formed: this is stipulated, not discovered. But human languages just aren't like that. Natural language expressions that are somewhat ill-formed can be meaningful, hence must be accounted for; natural language expressions come in degrees of well-formedness, and this too must be accounted for; and reasonable people can disagree, on empirical grounds, about which category an expression belongs to. So, the 'pre-established set of sentences' picture just does not fit well with natural, spoken, languages. Second, in this chapter we have been considering whether those working in the discipline of linguistics should pursue their task while eschewing certain sorts of evidence. Put otherwise, the word

[7] As Stich points out, less trivial variations are also possible, but they are vastly more difficult to construct. The background issue here, by the way, is 'indeterminacy': Stich [1972] exhibits a sort of Quinean [1960] pessimism about the possibility that there is a single descriptively adequate grammar internalized by speakers of a language, seeing room instead for an indeterminacy of grammars. For a reply to such worries, see [Chomsky and Katz, 1974].

"linguistics" in our title is to mean... *the actual discipline of linguistics.* Now, even linguists who are suspicious of taking linguistics to be a psychological enterprise would emphatically reject the idea that the only thing they are trying to do is find some way, any way, of characterizing the well-formed expressions of various languages. So, even beyond its other weaknesses, this objection simply doesn't speak to the issues actually at play.

We take this example about priming effects to afford several lessons. Before reviewing them, however, let us stress again what the lesson is *not*: it's not that the distinction between irregular and regular verb forms is linguistically important. It doesn't matter at all for our purposes who ends up being right about that dispute. Instead, what one should take away from the example is, first, the cogency in this particular case of the differential certainty point: it's far more clear that reaction times and ERP data are relevant to the nature of English verbs than it is that, say, English is an abstract object, or a cultural practice such that... In particular, second, it couldn't be more clear both that infants do not have access to the results of priming experiments, and that it would be well-nigh irrational to simply ignore such evidence. Third, the structure of the system known has a causal impact on our minds/brains. Because of this, we can make inferences to the best explanation from neuropsychological effects to the structure of the thing-known. In the present case, for instance, we can infer from contrasting processing effects for regular and irregular verbs to contrasting rule systems in the language known: such a difference in rule systems would account for the observed effects. Finally, this case illustrates nicely that evidence from domains never imagined, not even fifty years ago, can end up bearing on a linguistic issue. As hinted, this is possible precisely because we are trying to find out how things are connected — and so we can end up being quite surprised by what connections actually obtain. (Recall the tides and "terrestrial evidence" example. Another very nice illustration appears in [Antony, 2003]: drawings on Amerindian ceramic pots turned out to be relevant to establishing the timing of a supernova.)

Our goal in this chapter has been to introduce the debate about whether there should be restrictions on the evidence base for linguistics. Specifically, we took up the question whether the evidence base should be restricted *a priori* and in principle, and whether it should exclude, in principle or in practice, evidence from mentalistic psychology and neuroscience. We described three views — whose origins lie in some sense with Katz, Quine, and Wittgenstein — that lead to placing such restrictions on the evidence base for language study. Our aim was to make clear why, on such views, there are *prima facie* plausible grounds for restricting the evidence base for linguistics. In the final two sections, however, we responded to the three pro-restriction views, arguing that the restrictions they seek to impose ultimately should be avoided.

ACKNOWLEDGEMENTS

We are grateful to Steven Davis, Ray Elugardo and Dave Matheson for comments on an earlier draft. Portions of this paper were presented at the Workshop on Semantics and Psychological Evidence at Washington University in St. Louis, April 15^{th}, 2005. We thank the organizer Sam Scott and the audience members for helpful feedback. Especially useful were comments from participants José Bermúdez, Philip Robbins and Ken Taylor.

BIBLIOGRAPHY

[Antony, 2003] L. Antony. Rabbit-pots and supernovas: On the relevance of psychological data to linguistic theory. In A. Barber (ed.), *Epistemology of Language*. Oxford: Oxford University Press, 47–68, 2003.

[Barber, 2003] A. Barber. Introduction. In A. Barber (ed.), *Epistemology of Language*. Oxford: Oxford University Press, 1–43, 2003.

[Boghossian, 1997] P. Boghossian. Analyticity. In B. Hale and C. Wright (eds.), *A Companion to the Philosophy of Language*. Oxford: Blackwell, 331–368, 1997.

[Chomsky, 1969] N. Chomsky. Quine's empirical assumptions. In D. Davidson and J. Hintikka (eds.), *Words and Objections*. Dordrecht: D. Reidel, 53–68, 1969.

[Chomsky, 1986] N. Chomsky. *Knowledge of Language: Its Nature, Origin, and Use*. New York: Praeger, 1986.

[Chomsky, 2000] N. Chomsky. *New Horizons in the Study of Language and Mind*. Cambridge, UK: Cambridge University Press, 2000.

[Chomsky, 2002] N. Chomsky. *On Nature and Language*. Cambridge: Cambridge University Press, 2002.

[Chomsky and Katz, 1974] N. Chomsky and J. Katz. What the linguist is talking about. *Journal of Philosophy*, 71, 347–367, 1974. Reprinted in N. Block (ed.), *Readings in the Philosophy of Psychology*, Vol. 2. Cambridge, MA: Harvard University Press, 223–237, 1981.

[Davidson, 1979] D. Davidson. The inscrutability of reference. *The Southwestern Journal of Philosophy*, 10: 7–19, 1979. Reprinted in his *Inquiries into Truth and Interpretation*. Oxford: Oxford University Press, 227–241, 1984.

[Devitt, 2003] M. Devitt. Linguistics is not psychology. In A. Barber (ed.), *Epistemology of Language*. Oxford: Oxford University Press, 107–139, 2003.

[Dummett, 1973] M. Dummett. The philosophical basis of intuitionistic logic. In his *Truth and Other Enigmas*. Cambridge: Harvard University Press, 215–247, 1973.

[Fodor, 1981] J. Fodor. Some notes on what linguistics is about. In N. Block (ed.), *Readings in the Philosophy of Psychology*, Vol. 2. Cambridge, MA: Harvard University Press, 197–201, 1981. Reprinted in J. Katz (ed.), *The Philosophy of Linguistics*. Oxford: Oxford University Press, 146–160, 1981.

[Fodor, 1983] J. Fodor. *The Modularity of Mind*. Cambridge, MA: MIT Press, 1983.

[Forster *et al.*, 1987] K. I. Forster, C. Davis, C. Schoknecht and R. Carter. Masked priming with graphemically related forms: repetition or partial activation? *Quarterly Journal of Experimental Psychology*, 39: 211–251, 1987.

[Fowler *et al.*, 1985] C. Fowler, S. Napps and L. B. Feldman. Relations among regular and irregular morphologically related words in the lexicon as revealed by repetition priming. *Memory and Cognition*, 13: 241–255, 1985.

[Frege, 1884] G. Frege. *Foundations of Arithmetic*, 1884. Reprinted in M. Beaney (ed.), *The Frege Reader*. Oxford: Blackwell, 84–129, 1997.

[Frege, 1892] G. Frege. On sense and reference. In M. Beaney (ed.), *The Frege Reader*. Oxford: Blackwell, 151–171, 197.

[George, 1989] A. George. How not to become confused about linguistics. In A. George (ed.), *Reflections on Chomsky*. Oxford: Blackwell, 90–110, 1989.

[Higginbotham, 1983] J. Higginbotham. Is grammar psychological? In L. Cauman *et al.* (eds.), *How Many Questions?* Indianapolis, IN: Hackett, 1983.

[Higginbotham, 1985] J. Higginbotham. On semantics. *Linguistic Inquiry*, 16(4): 547–593, 1985.

[Katz, 1981] J. Katz. *Language and other Abstract Objects.* Totowa, NJ: Rowman and Littlefield, 1981.

[Katz, 1985] J. Katz. An outline of Platonist grammar. In J. Katz (ed.), *The Philosophy of Linguistics.* Oxford: Oxford University Press, 172–203, 1985.

[Katz, 1990] J. Katz. *The Metaphysics of Meaning.* Cambridge, MA: MIT Press, 1990.

[Kempley and Morton, 1982] S. Kempley and J. Morton. The effects of priming with regularly and irregularly related words in auditory word recognition. *British Journal of Psychology*, 73: 441–454, 1982.

[Laurence, 2003] S. Laurence. Is linguistics a branch of psychology? In A. Barber (ed.), *Epistemology of Language.* Oxford: Oxford University Press, 69–106, 2003.

[Laurence and Margolis, 2001] S. Laurence and E. Margolis. The poverty of the stimulus argument. *British Journal for the Philosophy of Science*, 52, 2001.

[Lewis, 1975] D. Lewis. Languages and language. In K. Gunderstone (ed.), *Minnesota Studies in the Philosophy of Science*, Vol. VII, 1975. Reprinted in D. Lewis, *Philosophical Papers*, Vol. I. Oxford: Oxford University Press, 1983.

[Münte *et al.*, 1999] T. F. Münte, T. Say, H. Clahsen, K. Schiltz and M. Kutas. Decomposition of morphologically complex words in English: Evidence from event-related brain potentials. *Cognitive Brain Research*, 7: 241–253, 1999.

[Quine, 1953a] W. V. O. Quine. The problem of meaning in linguistics. In his *From A Logical Point of View.* Cambridge, MA: Harvard University Press, 47–64, 1953a.

[Quine, 1953b] W. V. O. Quine. Two dogmas of empiricism. In his *From A Logical Point of View.* Cambridge, MA: Harvard University Press, 20–46, 1953b.

[Quine, 1960] W. V. O. Quine. *Word and Object.* Cambridge, MA: MIT Press, 1960.

[Quine, 1969a] W. V. O. Quine. Ontological relativity. In his *Ontological Relativity and Other Essays.* New York: Columbia University Press, 26–68, 1969a.

[Quine, 1969b] W. V. O. Quine. Epistemology naturalized. In his *Ontological Relativity and Other Essays.* New York: Columbia University Press, 69–90, 1969b.

[Quine, 1987] W. V. O. Quine. Indeterminacy of translation again. *Journal of Philosophy*, 84, 1987.

[Rugg, 1985] M. D. Rugg. The effects of semantic priming and word repetition on event-related potentials. *Psychophysiology*, 22: 642–647, 1985.

[Soames, 1984] S. Soames. Linguistics and psychology. *Linguistics and Philosophy*, 7: 155–179, 1984.

[Soames, 1985] S. Soames. Semantics and psychology. In J. Katz (ed), *The Philosophy of Linguistics.* Oxford: Oxford University Press, 204–226, 1985.

[Stainton, 2001] R. Stainton. Communicative events as evidence in linguistics. In J. de Villiers and R. Stainton (eds.), *Communication in Linguistics*, Vol. 1: *Papers in Honour of Michael Gregory.* Toronto: Editions du GREF, 329–346, 2001.

[Stainton, 2006] R. Stainton (ed.). *Debates in Cognitive Science.* Oxford: Blackwell, 2006.

[Stanners *et al.*, 1979] R. Stanners, J. Neiser, W. Hernon and R. Hall. Memory representation for morphologically related words. *Journal of Verbal Learning and Verbal Behaviour*, 18: 399–412, 1979.

[Stich, 1972] S. Stich. Grammar, psychology, and indeterminacy. *Journal of Philosophy*, 79(22): 799–818, 1972. Reprinted in J. Katz (ed.), *The Philosophy of Linguistics.* Oxford: Oxford University Press, 126–145, 1981.

[Wittgenstein, 1953] L. Wittgenstein. *Philosophical Investigations.* Oxford: Blackwell, 1953.