those assumed by Wang and Spelke illustrates this well. Therefore we need not only to raise hypotheses about 'the mechanisms by which humans overcome the limits of our primitive navigational systems' [1], but also to extend these same hypotheses to a vast spectrum of animals, in the study of which the lack of conceptual and technical tools might so far have prevented us from appreciating the true nature of their spatial representations.

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How seriously should we take Minimalist syntax?

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Lasnik's review of the Minimalist program in syntax [1] offers cognitive scientists help in navigating some of the arcana of the current theoretical thinking in transformational generative grammar. One might observe, however, that this journey is more like a taxi ride gone bad than a free tour: it is the driver who decides on the itinerary, and questioning his choice may get you kicked out. Meanwhile, the meter in the cab of the generative theory of grammar is running, and has been since the publication of Chomsky's *Syntactic Structures* in 1957. The fare that it ran up is none the less daunting for the detours made in his *Aspects of Theory of Syntax* (1965), *Government and Binding* (1981), and now *The Minimalist Program* (1995). Paraphrasing Winston Churchill, it seems that never in the field of cognitive science was so much owed by so many of us to so few (the generative linguists).

For most of us in the cognitive sciences this situation will appear quite benign (that is, if we don't hold a grudge for having been taken for a longer than necessary ride), if we realize that it is the generative linguists who should by rights be paying this bill. The reason for that is simple and is well-known in the philosophy of science: putting forward a theory is like taking out a loan, which must be repayed by gleaning an empirical basis for it; theories that fail to do so (or their successors that might have bought their debts) are declared bankrupt. In the sciences of the mind, this maxim translates into the need to demonstrate the psychological (behavioral), and, eventually, the neurobiological, reality of the theoretical constructs. Many examples of this process can be found in the study of human vision, where, as in language, direct observation of the underlying mechanisms is difficult; for instance, the concept of multiple parallel spatial-frequency channels, introduced in the late 1960s, was completely vindicated by purely behavioral means over the following decade (see, for example, [2]).

In linguistics, the nature of the requisite evidence is well described by Townsend and Bever: 'What do we test today if we want to explore the behavioral implications of syntax? …the psychological basis for the two primary and ever-present operations, merge and move.' (Ref. [3], p.82). Unfortunately, to our knowledge, no experimental evidence has been offered to date that suggests that *merge* and *move* are real (in the same sense that the spatial-frequency channels in human vision are). Generative linguists typically respond to calls for evidence for the reality of their theoretical constructs by claiming that no evidence is needed over and above the theory's ability to account for patterns of grammaticality judgments elicited from native speakers. This response is unsatisfactory, on two accounts. First, such judgments are inherently unreliable because of their unavoidable meta-cognitive overtones, because grammaticality is better described as a graded quantity, and for a host of other reasons [4]. Second, the outcome of a judgment (or the analysis of an elicited utterance) is invariably brought to bear on some distinction between variants of the current generative theory, never on its foundational assumptions. Of the latter, the reality of *merge* and *move* is but one example; the full list includes assumptions about language being a 'computationally perfect' system, the copy theory of traces, the existence of Logical Form (LF) structures, and 'innate general principles of economy'. Unfortunately, these foundational issues have not been subjected to psychological investigations, in part because it is not clear how to turn the assumptions into testable hypotheses.

Lasnik is optimistic that Minimalism, which is "as yet still just an 'approach', a conjecture about how human language works ('perfectly')" (Ref. [1], p. 436), can be developed into an 'articulated theory of human linguistic ability.' Such optimism would seem to require that the foundational issues be thoroughly addressed, but to our

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surprise they are not on Lasnik's list of 'Questions for future research'. This might explain why Minimalism is not even mentioned in recent reviews of, and opinions on, various aspects of language research in this journal, ranging from sentence processing and production [5–7] and syntactic acquisition [8,9] to the brain mechanisms of syntactic comprehension [10–12]. We believe it would be in the best interests of linguistics and of cognitive science in general if the linguists were to help psychologists like ourselves to formulate and sharpen the really important foundational questions, and to address them experimentally. This, we think, would help cognitive scientists take Minimalist syntax more seriously.

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Linguistics and empirical evidence

Reply to Edelman and Christiansen

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The main claims of Edelman and Christiansen's (E&C's) comment [1] on Lasnik's article [2] are that generative grammar is built upon empirically weak (perhaps non-existent) foundations, and that generative grammarians aggressively resist experimental testing of their assumptions. Neither of these claims survives even brief scrutiny.

There is a long history of laboratory-based work that investigates foundational constructs of generative grammar. The operation *merge*, which produces recursive hierarchical structures, was already a focus of interest among psycholinguists 30 years ago [3]. Under the heading of the 'binding problem', the same issue is a leading problem in neuroscience research today [4]. The nature of long-distance dependencies, which some versions of generative grammar capture via *movement* operations, has been widely investigated using reaction-time methodologies since the 1960s [5]. The inconclusiveness of earlier results has led to progressive sharpening of the issues [6–8], particularly concerning traces. Developmental considerations have been decisive in areas such as binding theory [9], and argument structure [10]. More recently, techniques from cognitive neuroscience have been added to the inventory of tools [11,12]. This listing could be continued for pages.

Moreover, the list grows exponentially once we move beyond the methodological imperialism of E&C's letter. Gathering of native-speaker judgments is a trivially simple kind of experiment, one that makes it possible to obtain large numbers of highly robust empirical results in a short period of time, from a vast array of languages. Any good linguistics study involves carefully constructed materials, appropriate control items, and robust and replicable results. It is only because the technique is so easy and requires no more than a notebook that it is not usually described as an 'experiment'. Note that when 4-year-olds are involved, the same task calls for a quiet room, toys, and various clever ruses [13,14], and then everybody agrees that it is an experiment. Outsiders would surely be puzzled by the attitude that seeks to deny the psychological relevance of easy, robust results, while insisting on other, far more subtle measures, such as 20-ms differences in reaction times, or 1-s changes in how quickly babies get bored, or 2% changes in regional cerebral blood flow. The variability that one observes in native-speaker judgments is real, but very small relative both to the agreement among speakers and relative to the variability in other measures (we have observed this repeatedly in our own studies). Furthermore, it is a truism in linguistics, widely acknowledged and taken into account, that acceptability ratings can vary for many reasons independent of grammaticality [15].

In language, as in any other area of inquiry, decisive evidence can – and does – come from a variety of sources, and it is hard to know in advance where the key evidence will come from. It is at best misleading for E&C to

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maintain that it is not clear how to turn the foundational assumptions of generative linguistics into testable hypotheses, as many researchers, whether in labs or with notebooks, have been doing so for decades [16].

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Capturing underlying differentiation in the human language system

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The extended treatment of the 'past-tense debate' in *TICS* [1,2] is a useful reminder that this debate consists of two kinds of issue – a broad, almost philosophical dispute about the role of symbolic computation in human cognition, and a more specific and empirical debate about the underlying functional and neural structure of the human language system. Whether the philosophical issues are decidable is a matter of opinion, although there is little to suggest from the *TICS* contributions that we are any closer to resolution than we were 15 years ago. Where the actual structure of the language system is concerned, the accumulating evidence points to significant underlying differentiation in function. In this respect, we believe the McClelland and Patterson (M&P) position [2] to be ill-founded. On the other hand, we doubt that evidence for underlying differentiation is particularly strong evidence, *per se*, for the cognitive reality of symbolic computation.

Neuropsychological evidence clearly indicates some differentiation in language function between posterior, temporal brain regions and anterior, frontal regions. The English regular and irregular past tenses seem to differ in their dependence on these two regions. We argue that this is because regular inflected forms in English are morpho-phonologically complex, and this engages specialised frontal parsing mechanisms [3–5]. To cope with the growing evidence for neural differentiation, perceived as being incompatible with the connectionist 'single-mechanism' approach, M&P argue for a model where performance on irregulars is more dependent on semantics, and performance on regulars is more sensitive to phonological factors. This account not only fails to reflect the neurological structure of the language system in the brain, but also seems empirically incorrect.

The first problem is that the model makes the wrong predictions about the role of semantics in the relationship between an irregular past tense form and its stem. We were the first to report a correlation between semantic deficits and impaired performance on the English irregular past tense [6,3]. In subsequent studies with normal adults, designed to probe this implied causal link, we showed that the underlying relationship between irregular forms and their stems was morphological rather than semantic. Pairs like *gave/give* and *jumped/jump* are related because they share a common morpheme, in contrast to semantically related pairs (*cello/violin*) that do not have a common morpheme and are lexically separate. In a delayed repetition priming experiment designed to separate semantic from morphological effects, priming of regular and irregular pairs was equally well preserved over time, whereas semantic priming dissipated [3]. In an ERP study, the patterns of brain activity associated with regular and irregular crossmodal repetition priming patterned together, with both showing left anterior negativities standardly associated with linguistic processing, whereas semantic primes showed only a centrally

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