

account extends to other phenomena, including some types of developmental dyslexia [5] and developmental language impairments. In two articles [6,7] we suggest how a phonological deficit could give rise to deficits in both inflectional morphology and aspects of grammar seen in cases of specific language impairment (SLI). Pinker and Ullman dismiss this account of SLI, citing studies that failed to find deficits in processing auditory (non-speech) signals in such individuals [8,9]. At best, however, such evidence indicates only that the language problem is not caused by a peripheral auditory-perceptual deficit. It has no bearing on the existence of processing deficits related to speech or phonology (as observed in [9,10] and many other studies). The principles that were implemented in the J&S model also produce the graded phenomena discussed by McClelland and Patterson, and explain facts about plurals and their occurrence in compounds [11]. Finally, one more prediction: German inflection will also turn out to be explained by the conjunction of phonological and semantic constraints [12].

Pinker and Ullman assert that connectionist models will ultimately need to incorporate structured symbolic representations. However, the opposite seems more likely. Pinker *et al.* have never implemented a computational version of their theory. Our models engage issues about the feasibility of proposed mechanisms at a level that they have not approached. Pinker's method, by contrast, involves describing the lexical system in words and ascribing to it whatever computational properties are desired. When Pinker and his colleagues begin to address how the complex system they propose could be learned and used, they will converge on the kind of mechanisms described in our models. Such models exhibit behaviors not deducible from symbolic theories and thus are not 'mere

implementations' of them. This step would probably also increase their sensitivity to distinguishing between what is central to a model and what is an implementational detail of no theoretical consequence.

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The past-tense debate: exocentric form versus the evidence

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Homophone verbs that take different inflected forms (*shoe–shod*, *shoo–shoed*) are central to the past-tense debate [1–4]. Pinker and Ullman [1,2] claim that exocentric words (which lack a root in their head position, that is, verbs derived from nouns) are automatically regularized. Accordingly, inflection cannot be explained according to the distributional properties of language; it is also affected by grammatical concepts such as noun and verb.

What evidence is there that exocentric forms affect past-tense processing? To examine whether semantics or exocentricity governs homophone processing, Kim, Pinker, Prince and Prasada [5] elicited ratings of inflected forms of

homophone verbs, and independent ratings of the semantic similarity of the verbs-in-context to standard irregular senses. Unfortunately, Kim *et al.* did not take any measure of whether verbs were exocentric (i.e. derived from a noun) or not. The data for the second of the two predictors supposedly being compared were plucked from thin air. In Kim *et al.*'s study, verbs that were already known to have a regular past tense were defined as 'denominal' and verbs that were known to have an irregular past tense were defined as 'deverbal'. Kim *et al.* then tested to see whether this 'classification' would better predict acceptability than their semantic measures, and finding that it did, concluded that exocentric forms, not semantics, determined homophone inflection patterns.

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Noting this circularity, Ramskar [6] repeated the Kim *et al.* study, taking truly independent ratings to see which verbs participants considered to be denominal or deverbal (i.e. exocentric or not). The results of this new experiment were clear. The semantic similarity (or otherwise) of verbs-in-context to ordinary irregular usage predicted the acceptability of past-tense forms. Whether or not verbs were perceived to be exocentric was irrelevant to this. It is worth noting that these results were obtained using the verbs put forward by Kim *et al.* [5] to try to demonstrate their theory. The theory that homophone verb processing can be explained by exocentricity fails completely in the face of examples like *shoe–shoo*, where it is the denominal verb that is irregular (*shoe–shod*) and the deverbal verb regular (*shoo–shooed*). (Further support for these findings comes from on-line studies that have shown that semantics – and not exocentric form – predict comprehension time for homophone verbs [7].)

What is the relevance of these data to the past-tense debate? First, they demonstrate that the ‘in-principle’ objection to single-route models supposedly posed by homophone verbs is wrong. Inflections of homophone verbs are determined by complex semantic and phonological patterns, not grammatical status. Second, they show that children’s ability to process homophone verbs is not indicative of their innate sensitivity to nouns and verbs [8], but rather is indicative of children’s ability to learn

languages. Finally, because the exocentricity thesis is false, the dual-route theory itself cannot account for the processing of homophone verbs. Providing an account of homophone-verb inflection is vital to explaining past-tense processing [1–4]; the evidence suggests that it is two routes that this phenomenon rules out, not one.

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Letters Response

Beyond one model per phenomenon

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Reply to Seidenberg and Joanisse

We would be the last to claim that the Joanisse and Seidenberg model (JSM) is the same as the Words and Rules theory. ‘Crude implementation’ refers to an earlier model [1] with units encoding the relatedness of a verb to its noun root (a surrogate for morphological structure), not to JSM. Our noting that JSM contains a lexicon is not a failure to distinguish theoretical commitments from implementational details: Joanisse and Seidenberg themselves justified their decision to represent words, not semantic features, by pointing out that semantic similarity, ‘although... crucial for other phenomena,... is not important for the past tense.’ Our point exactly.

As for the differences, current evidence indicates that deficits on irregulars in anomia cannot, as J&S predicted, be reduced to semantic deficits [2]; nor can deficits with regulars be reduced to phonological deficits either in agrammatic aphasia [3] or in SLI [4]. We disagree that regular–irregular differences in agrammatic aphasia are as rare as in occasional random simulation runs selected post hoc [5]: the difference has been reported in eight studies

(see [6,7]), and has survived several attempts to eliminate it by controlling for factors such as phonology ([3,7,8]; this includes Bird *et al.*’s reading data [9]).

Although J&S protest our observation that recent connectionist models are tailored to a single phenomenon, Seidenberg’s past-tense models are an example. One of them addressed frequency effects in reaction time, and had no lexical or semantic nodes, which might have created an unwanted frequency effect [10]. Another addressed systematic regularization, and built in verb-noun-relatedness units [1]. A third (JSM) addressed double dissociations, and had distinct sub-networks of lexical and phonological units (but no relatedness units). Yes, the parallels between morphology and spelling are noteworthy. But they were first addressed by dual-route models [11], and the successes of connectionist models in both domains derive from a feature we readily acknowledge: the sensitivity of superpositional memory to frequency and similarity.

Reply to Ramskar

In noting the unimportance of semantic similarity for the past tense, J&S are on our side, against Ramskar. Ramskar is incorrect in claiming that Kim *et al.* [12] defined the

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