

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.

#### References

- 1 Pinker, S. and Ullman, M. (2002) The past and future of the past tense. *Trends Cogn. Sci.* 6, 456–463
- 2 Pinker, S. and Ullman, M. (2002) Combination and structure, not gradedness, is the issue. Reply to McClelland and Patterson. *Trends Cogn. Sci.* 6, 472–474
- 3 Pinker, S. (1999) *Words and Rules*, HarperCollins
- 4 Pinker, S. and Prince, A. (1988) On language and connectionism: analysis of a parallel distributed processing model of language acquisition. *Cognition* 28, 73–193
- 5 Kim, J.J. *et al.* (1991) Why no mere mortal has ever flown out to center field. *Cogn. Sci.* 15, 173–218
- 6 Ramskar, M.J.A. (2002) The role of meaning in inflection: why the past tense doesn’t require a rule. *Cogn. Psychol.* 45, 45–94
- 7 Ramskar, M.J.A. (2002) When the fly flied and when the fly flew: the effects of semantics on the comprehension of past tense inflections. *Proc. 24th Annu. Conf. Cogn. Sci. Soc.*, pp. 768–773, Erlbaum
- 8 Kim, J.J. *et al.* (1994) Sensitivity of children’s inflection to grammatical structure. *J. Child Lang.* 21, 179–209

1364-6613/03/\$ - see front matter © 2003 Elsevier Science Ltd. All rights reserved.  
doi:10.1016/S1364-6613(03)00019-6

#### Letters Response

## Beyond one model per phenomenon

Steven Pinker<sup>1</sup> and Michael T. Ullman<sup>2</sup>

<sup>1</sup>Department of Brain and Cognitive Sciences, NE20-413, Massachusetts Institute of Technology, Cambridge, MA 02139, USA

<sup>2</sup>Department of Neuroscience, Research Building EP-04, Georgetown University, 3900 Reservoir Rd, NW, Washington DC 20007, USA

#### 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

Corresponding authors: Steven Pinker (steve@psyche.mit.edu),  
Michael T. Ullman (michael@georgetown.edu).

denominal status circularly. Our first set of denominal items were existing verbs that had been independently categorized as denominal and judged as regular by linguists; because some skeptics had dismissed the judgments, we tested them to verify that naïve subjects preferred the regular forms. The second set presented nouns to participants, followed by a novel denominal verb usage. As this was an experimental manipulation that defined denominal status a priori, rather than a correlational study, the absence of ratings of denominal status does not create a logical circle.

Ramscar's findings differ from ours because his instructions stacked the deck against finding an effect of morphology. Sensitivity to morphology was measured by asking participants whether a target verb was similar to a single example: a novel usage of *fly* meaning 'to greet customers while wearing a fly costume'. This complex metalinguistic judgment – whether the target verb is crucially similar to the one in this odd scenario – is an insensitive measure of people's perception of whether a verb is based on a noun, as can be seen in anomalous data such as participants indicating little perceived relationship between 'to brake' and *brakes*.

Conversely, the measure of semantic similarity was confounded with headedness: subjects were asked whether the activity described by the target word 'reminded' them of the base word and to 'consider all the possible things [they] associated with [the] use of the word.' If two words share a root, one will certainly remind people of the other, and trigger associations with the other. This is distinct from whether the two words share semantic features, the mechanism invoked in connectionist accounts.

The pair *shod*–*shoed* does not contrast headless and headed verbs. Both are headless: dictionaries define *to shoo* as 'to say *shoo*.' In any case, *shod* is a dubious example of people irregularizing headless verbs. It is an archaic form (the fossil of a defunct phonological rule) that today is used most often as

a participial adjective. Many people are unaware that it is the preterite of *to shoe*, as can be seen in errors such as *to shod* [13]. That Ramscar had to reach for this as his counter-example shows that the overwhelming tendency is for headless verbs to get regular past-tense forms.

#### References

- 1 Daugherty, K.G. *et al.* (1993) Why no mere mortal has ever flown out to center field but people often say they do. *15th Annu. Conf. Cogn. Sci. Soc.*, pp. 383–388, Erlbaum
- 2 Miozzo, M. (2003) On the processing of regular and irregular forms of verbs and nouns: evidence from neuropsychology. *Cognition* (in press)
- 3 Tyler, L.K. *et al.* (2002) Phonology and neuropsychology of the English past tense. *Neuropsychologia* 40, 1154–1166
- 4 van der Lely, H.K. and Christian, V. (2000) Lexical word formation in Grammatical SLI children: a grammar-specific or input-processing deficit? *Cognition* 75, 33–63
- 5 Joanisse, M.F. and Seidenberg, M.S. (1999) Impairments in verb morphology after brain injury: a connectionist model. *Proc. Natl. Acad. Sci. U. S. A.* 96, 7592–7597
- 6 Pinker, S. and Ullman, M.T. (2002) Combination and structure, not gradedness, is the issue. *Trends Cogn. Sci.* 6, 472–474
- 7 Ullman, M.T. *et al.* (2003) Neural correlates of lexicon and grammar: evidence from the production, reading, and judgment of inflection in aphasia. *Brain Lang.* (in press)
- 8 Ullman, M. *et al.* (1997) A neural dissociation within language: evidence that the mental dictionary is part of declarative memory, and that grammatical rules are processed by the procedural system. *J. Cogn. Neurosci.* 9, 289–299
- 9 Bird, H. (2003) Deficits in phonology and past tense morphology: what is the connection? *J. Mem. Lang.* (in press)
- 10 Daugherty, K. and Seidenberg, M. (1992) Rules or connections? The past tense revisited. *14th Annu. Conf. Cogn. Sci. Soc.*, pp. 259–264, Erlbaum
- 11 Coltheart, M. (1985) Cognitive neuropsychology and the study of reading. In *Functions of the Right Cerebral Hemisphere* (Young, A., ed.), pp. 3–37, Academic Press
- 12 Kim, J.J. *et al.* (1991) Why no mere mortal has ever flown out to center field. *Cogn. Sci.* 15, 173–218
- 13 Pinker, S. (1999) *Words and Rules: The Ingredients of Language*, HarperCollins

1364-6613/03/\$ - see front matter © 2003 Elsevier Science Ltd. All rights reserved.  
doi:10.1016/S1364-6613(03)00021-4

#### Letters

## Mental imagery: against the nihilistic hypothesis

Stephen M. Kosslyn<sup>1,2</sup>, Giorgio Ganis<sup>1,3</sup> and William L. Thompson<sup>1</sup>

<sup>1</sup>Harvard University, Department of Psychology, 830 William James Hall, 33 Kirkland Street, Cambridge, MA 02138, USA

<sup>2</sup>Massachusetts General Hospital, Department of Neurology, 55 Fruit Street, Boston, MA 02114, USA

<sup>3</sup>Massachusetts General Hospital, Department of Radiology, 55 Fruit Street, Boston, MA 02114, USA

We begin with a summary of central elements of our depictive theory, which will allow the reader to decide whether it is 'grotesque' (to use Pylyshyn's term [1]). The theory hinges on the facts that: (i) the occipital lobe contains numerous topographically mapped areas that support depictive representations; and (ii) most cortical areas used in visual perception are also used in imagery [2], including early visual cortex [3–5]. In perception, the occipital topographic areas provide input to two major

visual pathways. One, running down the inferior temporal lobe, is involved in object recognition. Visual memories are stored in this pathway, but in a non-topographic form [6]. The other, running up to the posterior parietal lobe, is involved in specifying locations and orientations in space. According to our theory, a mental image of a shape is created when a visual memory is activated top-down, inducing a pattern of activation in the topographically mapped areas; backward connections from higher-level to lower-level visual areas are well-documented [7]. Similarly, images of spatial relations are created when a spatial

Corresponding author: Stephen M. Kosslyn (smk@wjh.harvard.edu).