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## DISCUSSION

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# Clarifying the logical problem of language acquisition*

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MacWhinney is to be commended for reopening questions about the logical problem of language acquisition in the light of new data and models. Unfortunately his discussion is marred by technical errors, false dichotomies, and inadequate attention to detail.

The 'logical problem of language acquisition' (Baker & McCarthy, 1981; Pinker, 1979, 1989) is not the belief that 'the input to the learner is too inconsistent to determine the acquisition of grammar.' The input CANNOT be 'too inconsistent', because children do acquire grammar. It is the question of how acquisition could work in principle – how a learner can correctly generalize from a finite sample of sentences in context to the infinite set of sentences that define the language from which the sample was drawn. As such, the logical problem is not a belief or theory or claim (either nativist or empiricist) but a research topic.

The problem, as with all problems of induction, is that an infinite number of generalizations are consistent with any finite sample of data. Many curves can be drawn through a set of points, many laws are consistent with a set of observations, and many grammars are consistent with a set of sentences. Therefore any learner who correctly induces a function, theory, or grammar must respect prior ('innate') constraints on its hypothesis space; the data alone are insufficient. This is a logical point which cannot be denied by any theory, nativist, empiricist, behaviourist, connectionist, constructivist, or emergentist (Quine, 1969). For the behaviourists, the innate constraints reside in the generalization gradients and response classes. For the connectionists, they reside in the features defining the units and the topology of the networks. For Chomskyans, they reside in categories, operations, and principles. For MacWhinney, they reside in the cues, items, alternatives pitted in competition, and categories whose absence constitutes 'indirect negative evidence.' Thus 'conservatism, item-based learning, indirect negative evidence, competition, cue construction, and

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monitoring' are not 'alternatives' to innate constraints on a learner's hypotheses, but claims about what those constraints are.

This nature–nurture dichotomy is also behind MacWhinney's mistaken claim that the absence of negative evidence in language acquisition can be tied to Chomsky, nativism, or poverty-of-the-stimulus arguments. Chomsky (1965, p. 32) assumed that the child's input 'consist[s] of signals classified as sentences and nonsentences …' – in other words, negative evidence. He also invokes indirect negative evidence (Chomsky, 1981). And he has never appealed to Gold's theorems to support his claims about the innateness of language. In fact it was a staunch ANTI-nativist, Martin Braine (1971), who first noticed the lack of negative evidence in language acquisition, and another empiricist, Melissa Bowerman (1983, 1988), who repeatedly emphasized it. The implications of the lack of negative evidence for children's overgeneralization are central to any discussion of learning, nativist or empiricist.

MacWhinney's confuses a finite language (one consisting of a finite number of sentences) with a finite-state language (one that requires a finite number of memory states). Neither Gold nor Hopcroft & Ullman (1979) proved that 'finite-state grammars can be acquired from positive evidence'; Gold proved that the class of all FINITE languages can be identified in the limit (one kind of acquisition) from positive evidence, but the class of finite-STATE grammars (which includes all the finite languages plus an infinite number of infinite languages) CANNOT be so identified. MacWhinney then confuses infinite languages with language that can be described with a phrase-structure grammar: though (3) and (4) in the target article are phrase-structure grammars, the languages they generate are not infinite. Finally, the fragment in (43) is not a finite-state grammar but a recursive transition network (equivalent to a phrase-structure grammar): it uses symbols such as 'gift' and 'recipient' each of which much be unpacked as further sequences of phrases and words (*the pony*, *the pretty pony*, *the pretty pony with a black tail*, *the same kind of pony that I gave her*, etc.).

More false nature–nurture dichotomies ensue. 'I would be willing to accept the claim that four-year-old children are beginning to behave in accord with the Structural [sic] Dependency Condition … But does this mean that they reach this point without learning?' The Condition is not a claim that children don't learn; it's a claim about HOW they learn, namely that they learn rules that manipulate phrase structures rather than word strings. Similarly, the fact MacWhinney's formulation 'does not rely on barriers, ECP, HCP, INFL, COMP, or movement' is irrelevant. Neither does the hypothesis about structure-dependence.

Kimball (1973) called attention not just to three-auxiliary constructions like *may have been* but to four-auxiliary constructions like *John must have been being tickled when the photo was taken*. It seems implausible that every



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speaker who accepts this construction heard it beforehand, or, more generally, that speakers will not accept any sequence of nouns and verbs that they have not previously heard. (The preceding sentence is as good an example as any.) With MacWhinney, I accept Pullum & Sholz's (2002) challenge to linguists to DOCUMENT the putative rarity of sentence constructions, but the combinatorial explosion of interlocking constructions in any language would surely yield many examples of sentence types that speakers accept without prior exposure.

MacWhinney claims that Horning's results on probabilistic language acquisition has not received due attention, but I discussed it 25 years ago (Pinker, 1979). Rather than 'undercutting the core logic of the logical problem' of language acquisition,' it reinforces it: Horning proposed a Bayesian model of language learning, in which the learner is innately equipped with *a priori* probabilities for all grammars.

MacWhinney's claims about conservatism and item-based learning are too glib. If children were truly conservative, they would parrot back only the sentences they hear, never making errors such as 'We holded the baby rabbits' (Marcus, Pinker, Ullman, Hollander, Rosen & Xu, 1992) or 'Don't giggle me (Bowerman, 1983). Nor would they generalize to novel words, as they do in producing *wugged* in experimental elicitations; nor would adults inflect neologisms such as *moshed* and *spammed* (Marcus *et al.*, 1992; Pinker, 1999). For that matter, people would never produce or understand any sentence they did not witness.

The problem is that children generalize along some dimensions but not others. Presumably MacWhinney assumes that children are not conservative in substituting different nouns within a noun phrase or different noun phrases (with or without an adjective, with or without an adjective and a prepositional phrase, etc.) in a given predicate-argument structure, but that they ARE conservative in other possible substitutions (for example, a double-object dative for a prepositional dative). But such assumptions define the content of a theory of language acquisition, and are what so-called 'nativist' theorists try to spell out explicitly. Appeals to 'analogic force' are empty unless one specifies the dimension along which speakers analogize. The dimension cannot be sounds alone, since children and adults generalize regular *-ed* to words that don't sound like those in their input (Marcus *et al.*, 1992; Prasada & Pinker, 1993). If the dimension is 'being a verb,' we are back to the innate syntactic categories MacWhinney claims to eschew.

Appeals to 'indirect negative evidence' are no better: children fail to hear *goed*, but they also fail to hear *wugged*, yet they have no problem generalizing from *wug* to *wugged* (nor do adults when they first hear *spam* and generalize to *spammed*). MacWhinney claims that children 'need to compute regular 'ed' as a percentage of all verbs' in order to recover from overregularization, but the same phenomena occur in constructions with wide



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variation in the percentage of regular forms and sound patterns (Marcus, 1995; Marcus, Brinkmann, Clahsen, Wiese & Pinker, 1995; Berent, Pinker & Shimron, 1999).

The discussion of the dative is also unsatisfactory. Children and adults are not itemwise conservative, as they produce errors such as *Button me the rest* and acceptable neologisms such as *fax him the instructions* (Gropen, Pinker, Hollander, Goldberg & Wilson, 1989). They cannot recover from errors such as *delivered him a package* because of competition with *deliver the package*; that would predict that *fax the instructions* would pre-empt *fax him the instructions*, which it doesn't. The problem is that some dyadic verbs allow generalizations to triadic forms (*tell a story/tell Bill a story*) while other, similar verbs do not (*whispered a story*/whispered him a story*). There are indeed criteria that differentiate them (Pinker, 1989), but they have nothing to do with 'syntactic frame competition': different verbs happily accommodate one, two, three, or more syntactic frames, with no signs of competition among them (Pinker, 1989, Chapter 1). Nor does 'semantic transitivity' work, even as a first approximation.

The general problem is that MacWhinney leaves the key assumptions unstated. He invokes 'competition' without specifying which entities compete with which other ones. (What about synonymous words, or synonymous constructions, especially in free-word-order languages?) He invokes 'item-based learning' without specifying what the 'items' are (rote word strings? constructions? what defines a construction?) And he invokes 'indirect negative evidence' without defining the categories that the child monitors for non-occurrence. MacWhinney just helps himself to whichever assumptions get him through an example, which creates an illusion of doing without innate mechanisms.

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