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REPLY
A Reply to Woodward and Markman

BRIAN MACWHINNEY

Carnegie Mellon University

Woodward and Markman (1991) argue that the Mutual Exclusivity constraint is an inborn bias, rather than a fixed limitation, and that its expression during early childhood may be weak and incomplete. By couching their position in these terms, Woodward and Markman have made falsification of the alleged inborn basis of the Mutual Exclusivity constraint extremely difficult. In particular, it will be hard to distinguish the late expression of a weak inborn bias from the acquisition of a cognitive strategy designed to deal with increased competition arising from lexical learning. © 1991 Academic Press, Inc.

When children learn new words, they also pick up new meanings. For example, the child may be shown a small cup and told that it is a "demitasse." The child then has to figure out what the new word "demitasse" means. Following Markman's Mutual Exclusivity (ME) constraint, the child can reason that the word "demitasse" means something different from "cup" and that the same object cannot be both a cup and a demitasse. As Merriman and Bowman (M&B) (1989) have shown, the child can provide evidence for the functioning of the ME constraint by applying any one of four strategies for dealing with new words. The four strategies are disambiguation, rejection, correction, and restriction. Consider the case in which both a demitasse and a cup are placed in front of the child. If the child is asked to point to the demitasse and decides to point to the object that is not a cup, the strategy being used is "disambiguation." If the child is told that an object is a demitasse and thereafter refuses to call it a cup, he has "corrected" or "restricted" the meaning of the word "cup." If child totally refuses to accept the new name and sticks to the name "cup" as the correct label for a demitasse, the strategy being used is "rejection."

It is important to recognize that we have no direct window upon the ME constraint. The ME constraint is a construct, not an observable. Our understanding of ME is dependent upon the child's use of one or more of the four strategies. If we fail to find the child applying a particular strategy, it could be that the child has not yet learned to link the constraint to application of one of these strategies. Attempting to prove that there is no

Please address reprint requests to Brian MacWhinney, Department of Psychology, Carnegie Mellon University, Pittsburgh, PA 15213.

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ME constraint at a given age in a given child amounts to an attempt to prove the null hypothesis. What this means is that the empirical debate over the ME constraint really revolves around the development of the strategies that enforce the constraint. To address this issue, M&B have presented studies indicating that, beginning around the age of 3, children make increasing use of these strategies. In the years between 3 and 6, their use of disambiguation and correction grows sharper and evidence for the use of the ME constraint grows clearer. On the face of it, this research seems to provide evidence that the strategies which enforce the ME constraint are learned during the preschool years.

M&B's analysis leaves open the question of the origin of the constraint itself. One possible account is the strong nativist account offered by Woodward and Markham (W&M) (1991). However, a learning account of the type developed in MacWhinney (1989) is more compatible with the gradual growth in the strategies actually observed by M&B. This account holds that what is innate is the principle of competition, rather than the ME constraint. The underlying force driving the acquisition of word meanings is the competition of word forms for semantic space. At first, there are only a few words in a vast space. With time, words become more densely packed and the competition between neighbors sharpens. The various strategies observed by M&B are developed in response to these pressures. There is a fundamental underlying pressure throughout development, but the chief developmental "action" is in the acquisition of the strategies.

The competition account fits well with M&B's finding that none of the four strategies are used consistently around age 2. However, W&M raise a number of serious concerns regarding the way in which M&B tested their youngest subjects. These various flaws are real enough, but the fact that 2-year-olds failed to show the effect in at least this one set of published investigations now shifts the burden of proof onto the shoulders of those who believe that the ME constraint is present from birth. A top priority of research in this area must now be the construction of experiments that provide clearer tests for the presence of the four strategies around age 2, while still avoiding the problems of novelty and lexical gap filling recognized by both M&B and W&M. In this regard, W&M's reference to an unpublished study by Hutchinson is interesting. However, we will have to wait until that study is published before we can determine how well it resolves this crucial issue.

Unfortunately, the W&M reply to the work of M&B drives large segments of this interesting debate outside of the arena of empirical investigation. In particular, W&M claim that there is no necessary linkage between innate constraints and their realization at particular points during development. This is certainly true for the genes controlling late-emerging

abilities arising during puberty and disabilities such as Huntington's chorea. For each of these characteristics, late emergence makes biological sense. It is a fact of human development and ecology that the sexual abilities emerging during puberty need not be available during childhood. Similarly, because the gene controlling Huntington's chorea is dominant, its effect must be delayed until the carrier has had a chance to further transmit the gene through sexual reproduction. But by what similar logic could we argue that there should be a delay in the expression of the gene controlling the ME disposition? Perhaps W&M could argue that there is no need for the ME constraint to express itself before the emergence of language. However, there is a great deal of symbolic expression and communication during the second year. Is there some reason that the ME constraint should rest unexpressed during this period?

It is important for developmentalists to be extremely wary of theories that decouple underlying constructs from their empirical realizations. Rather than focusing on ways in which we could maintain the ME constraint in the face of contradictory empirical evidence, it seems to me that we should focus on ways of using the ME constraint to generate detailed empirical predictions. In my review of M&B, I concluded that, "it is now time for the study of the ME constraint to give way to a more detailed process-based account of early word learning." This is not to say that the ME constraint is not real or that it may not have some deeper biological basis. It may well turn out in the end that the ME constraint is indeed present from birth and only expressed at some later point in development, just as W&M believe. The point is that we cannot make further progress in this discussion until we move to constrain the invocation of constraints. It is time for a detailed model of early word learning that can generate new empirical predictions.

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