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Commentary: Mechanisms of Language Acquisition

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Investigators who study language acquisition from widely different theoretical perspectives rarely talk to one another. This is unfortunate but understandable. In part, they are separated by their subject matters. Learnability theorists, along with other modern-day nativists who stress the role of children's innate linguistic endowment, have typically focused on abstract syntactic constraints that have no obvious connection to semantics or pragmatics. In contrast, their empiricist counterparts—researchers who emphasize the role of general learning heuristics—more often concentrate on aspects of language with clear functional correlates, such as morphology, word meaning, and the ordering of major sentence constituents. If they think about constructs like 'bounding node' or 'c-command' at all, they tend to view them with suspicion.

Investigators have also been divided by their attitudes toward child language data. Empiricists pay close attention in their theory construction to what children actually say and how their utterances change over time. Nativists are typically guided more by theoretical considerations of how the ultimate goal of learning must be described and what kind of learning system could arrive at this goal. Of course, it is not incidental to these postures that data bearing on the acquisition of morphology, word meaning, and word order is abundant in almost every transcript of spontaneous speech, whereas evidence relevant to children's grasp of subadjacency, for example, is much harder to come by. But despite good reasons for differences in attitude toward child language data, each camp is impatient with the other—the one is criticized for engaging in abstract, intellectual exercises that have little relationship with what children actually do and the other is accused of seriously underestimating the discrepancy between the input available

to the child and the complex and strongly constrained system that must be acquired.

This is at least how it has been in recent years. But the present volume, and the conference on which it is based, suggest the welcomed onset of a new phase of investigation in which researchers of diverse orientations begin to pay more attention to each others' work. Beneficial exchange has been encouraged by at least two developments. One is the increasing reliance of investigators of different theoretical persuasions on computer modelling of language acquisition. The demands of successful computer simulation force an explicitness about what is learned and how it is learned that brings underlying assumptions into the open and facilitates comparison and evaluation. The second development is the emergence of a small set of basic questions about language acquisition that investigators from very different research traditions can agree are important. To the extent that we are focused on the same problems, we can assess the adequacy of each others' attempts to solve them.

This discussion is organized around two such shared problems to which participants in the conference returned again and again.¹ First is the question of how children avoid ending up with an overly general grammar if they receive little feedback about what is not a possible sentence. Second is the puzzle of what drives *change* in children's grammars—what causes the learner to move from one level of knowledge or representation to the next. Given the range of strongly-held views on these and other topics represented at the conference, it is a tribute to the manners of the participants that they did not come to blows. There may be another, less complimentary reason for the general mood of good will: as Mike Maratsos rightly pointed out in discussion, the data are often compatible with everyone's position. One problem for the future will be to become more precise about what would or would not constitute counterevidence to particular proposed solutions. In the interests of promoting the necessary evaluations and revisions, I raise certain issues that I think present problems for just about everyone's theory.

WHY DON'T CHILDREN END UP WITH AN OVERLY GENERAL GRAMMAR?

The problem of how children avoid an overly general grammar was first raised in 1971 by Martin Braine, who used it to argue against the nativist position espoused by Chomsky and in favor of the idea that language is learned largely from scratch. It was later revived by Baker (1979) who, in an interesting turn-around,

¹Some of the papers in this volume are naturally more concerned with these problems than others. Because the problems are so fundamental I have decided to go into them in some depth, which means, unfortunately, that I cannot discuss the many other interesting hypotheses and issues covered in these proceedings.

made it the cornerstone of the argument that children must be guided by innate constraints in their acquisition of language. That nativist theorists have been able to adapt Braine's puzzle to their own use suggests that the problem transcends party lines. Indeed, I believe it constitutes one of the most intriguing and difficult challenges for all students of language acquisition.

The Problem

According to Chomsky (1965), children construct an internalized grammar by using incoming language data, together with innate linguistic knowledge, to formulate hypotheses about possible grammatical rules. Braine pointed out that in order for a hypothesis-testing procedure to work, learners must get feedback about the correctness of their predictions. In particular, they need to know when their predictions are incorrect so that they can revise hypotheses that are *over-general*—that generate not only all acceptable sentences but ill-formed sentences as well. However, language learners get little negative feedback, argued Braine. He concluded that the hypothesis-testing approach cannot be right and that children must acquire language with procedures for which positive evidence (i.e., exposure to possible sentences) is sufficient.

Baker agreed with Braine that children receive little information about what is not a possible sentence and must be prepared to learn from positive evidence only. However, he argued that this situation is not damning to the innatist program in general, but only to grammatical frameworks that allow types of rules that children apparently could not acquire on the basis of the evidence available to them. Baker's proposal was to constrain linguistic theory so that it excludes such rules. Put differently, Baker suggested that children are constituted in such a way that they will not entertain any rule that, if incorrect, could not subsequently be corrected on the basis of positive evidence alone.

Since Baker's article, hypotheses about how to solve the "no negative evidence" problem have proliferated. I do not attempt a detailed critique of every proposal, but I raise some general problems that affect many of the approaches that are pursued in the chapters of this volume.

The Appeal to Subsets

Initial restriction to a subset. Several investigators have suggested solutions that exploit the fact that candidate grammatical rules, constraints, or parameters may stand in a subset-superset relation to each other. For example, Berwick (1985; Berwick & Weinberg, 1984) proposes that since learning must proceed from positive instances only, children must first hypothesize the *narrowest possible rule* compatible with the evidence observed so far. If the rule is too narrow, positive evidence (i.e., sentences in the input that the rule will not

cover) will eventually reveal this to the learner, who then moves on to the next larger rule compatible with the data as he now perceives it. What the learner must not do is start with or ever hypothesize a rule that is too general, since in the absence of negative evidence there seems to be no way he could then cut back to the correct, more narrow rule. (See also Dell, 1981, for a general statement of this principle; Wexler and Manzini, in press, for an application to parameter-setting; and Smith, 1981, for an application to rules with semantic constraints.)

Fodor and Crain (this volume) rightly point out an unattractive corollary to this approach to the "no negative evidence" problem. When the output of one rule is a proper subset of the output of another rule, then the more narrow rule is typically more complex than the broader one since it is annotated for one or more constraints on application that the broader rule does not respect. This means that if children always start out by hypothesizing the narrowest rule possible, their first rules are routinely more complex than their later ones. But it clashes with our intuition to imagine that children move consistently from more complex to less complex rules—that language acquisition proceeds by the successive removal of constraints on rule application.

We do not have to rely on theoretical considerations alone to question the subset principle as a general solution to the "no negative evidence" problem. There are also empirical grounds for rejecting it: evidence, in the form of children's errors, that learners do *not* always start out with the most restrictive rule compatible with the evidence. Some sample errors are shown in Table 1; see also Mazurkewich and White, 1984, for comparable overgeneralizations in children's judgments of the grammaticality of sentences with "shifted" datives.²

An alternative: Cutting back to a subset. Aware of such errors, some investigators (e.g., Mazurkewich & White, 1984; Pinker, 1984, Chapter 8) have proposed another way of exploiting subset relationships among certain types of rules to solve the "no negative evidence" problem. According to this approach it is not essential to block overgeneralizations before they occur. Retreat from overgeneralization can be triggered later, where necessary, when the child discovers that all the lexical items observed to undergo a given rule in adult speech *share certain semantic, morphological, or phonological properties*. Once these properties are recognized (i.e., once the appropriate subset is identified) the child will limit productive use of the rule to lexical items of the right class and errors will cease.³

²Such errors also go counter to Baker's (1979) proposal for solving the "no negative evidence" problem: that children do not form general rules for regularities with lexical exceptions but wait to see, for every item, which syntactic frames it can appear in (see Bowerman, 1983).

³This approach might also be extended to more complicated situations, if they exist, where a rule applies not to a single subset of lexical items but to two or more disjunctively defined subsets. Of course, a critical question for this general approach is *how* children identify the properties shared by a subset of lexical items subject to a rule; for relevant discussion see Maratsos and Chalkley (1982), Pinker (1984) chapter 8, and my later section on "Does change take place on-line or off-line?"

TABLE 1
Some Overgeneralizations of Rules with Lexical Exceptions^a

A. Dative Alternation

1. C 3;1 *I said her* no.
2. C 2;6 Don't *say me* that, or you'll make me cry.
3. C 2;6 I want Daddy *choose me* what to have. (Re: what kind of juice to have at breakfast.)
4. M 5+ *Choose me* the ones that I can have.
5. C 3;4 *Button me* the rest. (Request to have remaining snaps on her pyjamas fastened.)
6. — 6;0 Mommy, *open Hadwen* the door. (Mazurkewich and White, 1984.)
7. — 2;3 I'll *brush him* his hair. (Mazurkewich and White, 1984.)

B. Lexical Causatives. (The regularity to which the verbs below are exceptions is exemplified by intransitive/transitive pairs like *The stick broke/I broke the stick*.)

8. J 6+ Do you want to see us *disappear* our heads? (Then, with a friend, she ducks down behind couch.)
9. — 2;8+ I don't want any more grapes; they just *cough* me. (Braine, 1971.)
10. R 5;9 I want to *comfortable* you. (R lying on sofa cuddling her mother.)
11. E 3;0 Don't *giggle* me. (As father tickles her.)
12. E 3;2 Will you *climb* me up there and hold me? (Wants mother to help her climb a pole.)
13. C 3;6 Did she *bleed* it? (After her sister falls and hits head on edge of table.)
14. C 4;3 It always *sweats* me. (Refusing sweater.)
15. M 5;8 M: These are nice beds.
Mother: Yes, they are.
M: Enough to *wish* me that I had one of those beds.

C. Reversative *un-* prefixation.

16. E 3;11 How do you *unsqueeze* it? (Coming to mother with clip earring dangling from ear; wants it off.)
17. E 3;10 Mother: I have to capture you. (Grabbing E in a game.)
E: *Uncapture* me!
18. C 4;7 C: I hate you! And I'm never going to *unhate* you or nothing!
(Angry after request is denied.)
Mother: You're never going to *unhate* me?
C: I'll never like you.
19. C 4;5 (C has asked mother why pliers are on table):
Mother: I've been using them for straightening the wire.
C: And *unstraighting* it?
20. C 5;1 He tipped to the graveyard and *unburied* her. (Telling ghost story.)
21. C 7;11 I'm gonna *unhang* it. (Taking stocking down from fireplace.)

^aChild's age is given in years; months. Sources as indicated, plus Bowerman, 1983, for dative alternation; 1982a,b,c, 1983 for lexical causatives; 1982b for reversative *un-* prefixation.

For dative alternation, for example, the child must learn that "shiftable" indirect objects must be "prospective possessors" of the entity named by the direct object and that the verb itself must be of "native stock," not Latinate (Mazurkewich & White, 1984, drawing on Green, 1974; Stowell, 1981; among others). For causativization of an intransitive verb or adjective, the causation must be direct (e.g., physical), and the agent, manner, and goal of causation must be stereotypic or conventional for the act in question (Pinker, 1984, drawing on Shibatani, 1976; Gergeley & Bever, in press; among others). And for reversative *un-* prefixation, a "covert semantic class" identified by Whorf (1956) is relevant: verbs that can be *un-*ed share "a covering, enclosing, and surface-attaching meaning . . . hence we say 'uncover, uncoil, undress, unfasten, unlock, unroll, untangle, untie, unwind,' but not 'unbreak, undry, unhang, unheat, unlift, unmelt, unopen, unpress, unspill'" (p. 72).

Although this alternative approach to exploiting subsets has the advantage of allowing for overgeneralizations that children actually make, and although there is evidence that children are indeed capable of restricting a rule that is initially overly general to verbs of the "right" semantic class (see Bowerman, 1982b, on *un-* prefixation), I do not think it is the right answer to the "no negative evidence" problem, at least for rules with lexical exceptions. (I leave open whether it is the solution for rules or constraints of other kinds.) The problem is that many of the subsets associated with rules with lexical exceptions are themselves dotted with gaps: items that fully conform to the semantic/morphological restrictions on candidates for the rule, as best we can identify them, but that still do not undergo the rule.

For dative alternation, such an item is *choose*. Many speakers find something distinctly odd about "shifted" indirect objects with *choose*, as in "I chose you a book at the library sale."⁴ Yet *choose* satisfies the putative semantic/morphological restrictions on verbs that allow dative alternation, since (a) it is of native stock and (b) the beneficiary of an act of choosing is the "prospective possessor" of the object named by the direct object. (Many speakers who regard "shifted" indirect objects with *choose* as unacceptable find them perfectly normal with *pick out*, which is semantically almost identical to *choose*.)

For *un-* prefixation, the verb *squeeze* is an inexplicable exception. You can squeeze somebody's hand but you can't **unsqueeze* it, even though *squeeze* falls into Whorf's covert class of "centripetal" verbs. (Note, for example, that you can both clench *and* unclench your teeth or fists; *squeeze* is similar to *clench* in specifying a continuous pressure toward a center point.)

⁴I base this claim on an informal survey of about 20 native speakers of English, most of whom rejected examples like these. Failure to find 100 percent agreement is not surprising: for every rule with lexical exceptions there are items about which speakers disagree or are uncertain. This is not important for my argument. As long as there are mature speakers who find such examples odd, we must explain how in their grammars the lexical item involved came to be excepted from the rule under examination.

For lexical causatives there are gaps in English like 'to **cough*/**laugh* /**comfortable*/**vomit* someone.' There seems to be no principled reason why a too-big bite can *choke* or *gag* us but not *cough* us, why we can *cheer* someone up but not *laugh* or *giggle* her (e.g., with tickling or a joke), and why we can *quiet* or *burp* a baby but neither *comfortable* (or *comfy*) her nor *vomit* her when she is nauseated (e.g., with a finger). The acts of causation specified by the latter verbs do not seem any less 'direct' or 'conventional' than those specified by the former.

Gaps like these—let's call them "negative exceptions"—have gone largely unnoticed in discussions of the "no negative evidence" problem. In contrast, *positive* exceptions—items to which a rule *does* apply even though they do *not* belong to the right class—have been widely discussed. Positive exceptions are generally regarded as tractable, since in principle they could be learned on the basis of positive evidence. For example, children could learn that *assign* allows dative alternation—even though it is Latinate—through hearing sentences like "The teacher assigned John a desk in the back row." However, there is no comparable evidence to mark *squeeze*, *choose*, *cough*, etc., as exceptions that *cannot* undergo rules whose conditions they otherwise satisfy. They are invisible holes within their subsets.

How do speakers identify such holes? How do children learn to stop constructing sentences with *unsqueeze*, '*choose* + indirect object', and causative *cough* (cf. Table 1)? It would be handy, of course, if we could invoke the principle of preemption. If every negative exception just happened to have an exact irregular counterpart, the way *foots* has a counterpart in *feet*, children would eventually give up their rule-governed form in favor of the form they consistently heard. Unfortunately, many negative exceptions lack such counterparts—e.g., there are no forms that preempt *unsqueeze*, '*choose* + indirect object', and causative *cough*, nor do phrasal alternatives always do the trick. (I come back to this in more detail in the next subsection.)

Perhaps, however, there really are no "negative exceptions." Steve Pinker (personal communication) suggests that where there appear to be such exceptions, we might simply have failed to adequately pin down the semantic/morphological/phonological constraints on the class of items that can undergo a rule. This raises an important question about the nature of the lexicon. The regularities reflected in the errors shown in Table 1 should, according to Pinker and many other investigators currently interested in the "no negative evidence" problem, be characterized as lexical rules, not as syntactic transformations. (And *un-* prefixation is a lexical rule under almost everyone's treatment.) Do there have to be systematic principles governing which lexical items do and do not undergo a lexical rule? No, according to Wasow (1977):

I assume that if a tree satisfying the structural conditions of a transformation is prohibited from undergoing the structural change, then some explanation is called

for, but a lexical item that *does not undergo a lexical rule whose conditions it satisfies is perfectly normal*. Transformations are crucial to the generation of all and only the sentences of the language (and hence have infinite domains); in contrast, lexical rules express subregularities within a finite lexicon . . . Hence, I assume (following Jackendoff, 1975) that lexical rules . . . *will typically have un-systematic exceptions*. (p. 331, emphasis added)

Unsystematicity in lexical rules is of course recognized by most theorists; in fact this has been one important basis for arguing that certain regularities should be handled in the lexicon rather than in the syntactic component of grammar. However, as I have noted, investigators concerned with the "no negative evidence" problem have worried about only one kind of unsystematicity: items that *do not* satisfy the conditions on a rule and yet *do* undergo the rule. There seems to be no reason to rule out the opposite kind of unsystematicity: items that *do* satisfy the conditions and yet *do not* undergo the rule. I believe that Wasow's view of the lexicon is correct and that no matter how hard we try to pin down the conditions that correlate with the candidacy of lexical items to undergo a given rule we are always going to be able to find unexplained negative exceptions as well as positive ones.

If so, children must have techniques for identifying them. And if they do have such techniques, they could presumably apply them directly to *all* lexical items to which a rule has been overgeneralized, rather than first eliminating some items through discovery of the appropriate subset before bringing in stronger methods to detect any remaining stragglers. In sum, it is not clear that children's discovery of semantic/morphological/phonological subsets plays any necessary role in their retreat from an overly general grammar—although, of course, their identification of such subsets is an interesting phenomenon in its own right (see Bowerman, 1982b), and it raises interesting issues for the problem of what drives change in children's grammars, as discussed later.

Preemption

Almost every investigator who worries about the "no negative evidence" problem has assumed that at least part of the answer lies in preemption: when children formulate overly general rules, they eventually give up overgeneralized forms if they are consistently faced with positive evidence for other forms expressing the same meanings. For example, a child with general rules for forming plural and past tense forms will for a time say *foots* and *broke*d. However, in the contexts where she uses these forms she hears only *feet* and *broke* from others. Eventually, therefore, she abandons her own forms in favor of those that are conventional. Baker (1979) called rule exceptions that could be corrected in this way "benign" exceptions; in contrast, exceptions for which there is no exact irregular counterpart he termed "embarrassing."

How does preemption work? Investigators differ in their assumptions about why children give up their rule-governed forms in the face of evidence for conventional alternatives. On one side are theorists who propose that there is some property "in the child" that rejects the idea that two forms should have exactly the same meaning. For example, Pinker (1984) postulates a "Unique Entry" principle, according to which children resist having more than one entry in an inflectional or derivational paradigm (e.g., both *breaked* and *broke*; both 'to die (someone)' and 'to kill (someone)'). Clark (this volume) proposes a more general "Principle of Contrast," according to which children assume that every two forms differ in meaning, and resist acquiring or retaining two forms that seem synonymous unless they are faced with strong positive evidence that both forms exist.

In contrast to those who rely on some version of Uniqueness or Contrast, "competition" or "connectionist" theorists (MacWhinney & Sokolov, Bates & MacWhinney, Rumelhart & McClelland, all in this volume) explain preemption as the outcome of *competition* among alternative forms for expressing the same meaning. According to this approach, children do not come with any built-in assumptions about whether or not forms should contrast in meaning, or how many entries a cell in a paradigm should have. They simply use the forms that they associate with the meanings they want to express. At some point in the acquisition of an inflectional or derivational paradigm children may have several forms for the same meaning (e.g., *breaked*, *broke*, *broked*), and use them all. Over time, however, the activation strength of some forms increases and that of others decreases, both as a function of what the learner says and what he hears from others. Eventually the forms heard consistently from fluent speakers become so strong that they overwhelm the child's overregularized forms, and these weakened competitors fade out.

How shall we evaluate these two assumptions about how preemption works? This seems to be one of those situations alluded to earlier, where much of the available evidence is compatible with either view; where it is not, there are persuasive arguments either way (compare, for instance, Clark, this volume with Gathercole, to appear).

For example, we might assume that if children start out with the expectation that two forms should not have the same meaning—and especially that there should not be two entries for a single cell in a paradigm—they should not use forms like *breaked* and *broke* or *bringed*, *brang*, and *brought* all interchangeably in the same speech context. They ought to decide on one and reject the others. But such alternation is in fact common (e.g., Kuczaj, 1977; Maratsos, 1979). This looks on the face of it like grist for the competition mill. However, Clark (this volume, note 8) argues that "instances of over-regularization may well linger on after children have begun to produce the appropriate, irregular past tense forms just because children have become used to saying the past-tense form of a verb that way. After all, they have been doing so for three or four years."

Thus, alternation between regular and irregular forms is interpretable on either account.

A point against Clark's interpretation and in favor of the competition account is that the simultaneous embrace of more than one form for the same meaning does not seem to be limited to spontaneous speech. Kuczaj (1978) found that when young children are asked to judge the acceptability of (for example) *eated*, *ate*, and *ated* as past tense versions of *eat*, they often find two or more forms equally acceptable. This outcome cannot be explained by appeal to automated routines for speaking. If the Uniqueness/Contrast approach were correct, we would expect that children should accept only *one* form for any given meaning.

However, the balance between the two approaches is again righted when we consider the following problem: where do intuitions of ungrammaticality come from? According to the competition account, the decline of *breaked*, *foots*, and the like is a matter of gradually decreasing activation strength. Presumably, activation strength never hits zero, since even adults occasionally produce forms like these and they certainly hear them from children. Where in the downhill slide of *breaked* and *foots* do these forms pass over the boundary from being possible but simply less robust instantiations of the notions of 'break + PAST' and 'foot + PLURAL' to being actively rejected as ungrammatical and unacceptable? (Note, for example, that adults and even relatively young children often correct such "slips" in their own speech when they detect them; see Clark, 1978.) Why should there be any such thing as a sense of ill-formedness, as opposed to simply a feeling of "low likelihood"? I do not know what the competition theorist's answer to this problem is. But clearly advocates of Uniqueness/Contrast have no trouble with it.

How much can preemption account for? Although it is not clear *how* preemption by existing forms works, I do not dispute that it does work. But how much of the "no negative evidence" problem can it handle? In general, I have the impression that investigators are often oversanguine about its potential.

A precondition for preemption is that for a child's overgeneralized word or construction there must exist a conventional adult counterpart that means what the child's form means and that occurs consistently in the contexts in which the child would use her own form. This precondition is met in most cases of inflectional overgeneralization. However, it is only spottily met in the case of overgeneralizations of derivational morphology and other lexical rules (e.g., *un-*prefixation, dative alternation, causativization; see Bowerman, 1983), and as Fodor and Crain (this volume) point out, it is not met at all for certain important syntactic phenomena, including extraction ("There is no well-formed competitor, for instance, for the ungrammatical sentence **Who did John overhear the statement that Mary kicked?*")

The notion of "preemption" is somewhat flexible, and several researchers have suggested what amounts to stretching it to cover certain otherwise prob-

lematic overgeneralizations. For example, Clark (this volume) suggests that children will give up their overgeneralized causative form 'to *disappear* (something)' (which lacks an exact suppletive counterpart) in favor of 'to *make* (something *disappear*', since in every context where they would say the former, they hear adults say the latter.

A stretch is involved here because *make disappear* is not a perfect semantic match to causative *disappear* (as *kill*, for example, is to causative *die*). In general, lexical causatives and their periphrastic counterparts differ with respect to the directness and conventionality of the act of causation specified (compare, for example, *John stood the baby up* [direct physical causation] with *John made the baby stand up* [indirect causation, e.g., through giving an order]). The weight of this meaning distinction, pervasive throughout English, ought to work somewhat against children's willingness to let periphrastic causatives like *make disappear* preempt nonexistent lexical causatives like *disappear*. Still, we might be willing to accept this solution, if only for lack of a better idea. (See also Pinker, 1981; Maratsos & Chalkley, 1982; and Bowerman, 1983 for the related proposal that children may identify items that are exceptions to their overly general rules through continually failing to encounter those items in discourse contexts where they "expect" them; this is sometimes called "indirect negative evidence," following Chomsky, 1981:9.)

But this extended view of preemption buys us only a little more help with the "no negative evidence" problem, not a cure. For the approach to work, there must at least be a *consistent relationship* between the child's overgeneralized form and an adult counterpart, even if that counterpart is not identical in meaning to the overgeneralization it will eventually come to replace. This condition is indeed met with causative *disappear* and its periphrastic counterpart. But what about, for example, errors with reversative *un-*? Here the child meets with no consistent alternatives in the adult input. For instance, in contexts where *un-squeeze* would be appropriate, if it existed, adults might say *loosen*, *ease up*, *release*, *let go*, *remove*, and so on. None of these is in direct semantic competition with *un-squeeze*, since none of them specifies or requires that the event referred to is the reversal of an act of "squeezing." Nor should the child take the existence of such forms as having any bearing on the possibility of *un-squeeze*: reversative *un-* forms coexist harmoniously with various related constructions, e.g., *unwrap* and *take the wrapper off*, *unzip* and *pull the zipper down*, *unload* and *empty*.

For overgeneralizations of the type shown in Table 2, the problem is even more complicated. This construction pattern is highly productive in English to express combinations of a causing event and a resulting change of state or location, but it is subject to constraints that are still poorly understood (Green, 1972; McCawley, 1971; Randall, 1983). How do children come to appreciate that there are any restrictions at all, much less what these restrictions are?

The difficulty is that novel utterances of this type, whether acceptable or

TABLE 2
Overgeneralizations of the 'Effect Complement' Sentence Pattern^a

(The models for overgeneralizations of this type include *shoot dead, pat dry, wipe clean, eat (oneself) into a stupor, pull up/down/in/out, etc., cut off/down, etc.*)

1. C 3;8 I *pulled it unstapled*. (After pulling stapled booklet apart.)
2. C 3;10 *Untie it off*. (Wants mother to untie piece of yarn and take it off tricycle handle.)
3. C 4;0 I'm *patting her wet*. (Patting sister's arm after dipping her own hand into a glass of water.)
4. E 6;3 His doggie *bited him untied*. (Telling how tied-up man in a T.V. show was freed.)
5. M 5;6 Are you *washing me blind?* (As mother wipes corners of her eyes.)
6. M 5;10 Feels like you're *combing me baldheaded*. (As mother combs her hair.)
7. A 4;3 When you get to her, you *catch her off*. (A is on park merry-go-round with doll next to her. Wants a friend, standing nearby, to remove doll when it comes around to her.)
8. R 4;9 I'll *jump that down*. (About to jump on bath mat M has just put on top of water in tub.)

Sources: Bowerman, 1982b,c.

peculiar to adult ears, are usually "one time only" constructions—designed to fit a certain passing configuration of cause and effect such as pulling on a book and the book's becoming unstapled, or combing the hair and becoming bald. This means that learners do not have the opportunity to observe "the way other people express this particular meaning." Even if a particular configuration of cause and effect should arise quite frequently (say, "untying a rope" so that it "comes off" of something, as in example 2), so that a child has a chance to hear other ways of expressing it (e.g., "untie the rope and take it off," "take the rope off by untying it," or just plain "take the rope off"), these alternatives have no bearing on the grammaticality of the child's version. As Fodor and Crain (this volume) point out, a learner cannot take every sentence he hears as precluding all sentences that express somewhat related messages; natural languages are too rich for this.

To summarize, children make a number of overgeneralizations for which preemption, even if interpreted liberally, fails to provide a correction. This is a problem for everyone, but I think especially for the competition approach to language acquisition. This is because this model explicitly rejects the need for innate linguistic constraints on the child, and instead tries to solve the "no negative evidence" problem by reference to the successful resolution of competition between alternative ways to express the same meaning.⁵ To the extent that

⁵This resolution can come about either by automatic adjustment of the relative activation strengths of overgeneralized and conventionally correct forms or by children's detection of discrepancies between the way they express a given meaning and the way others express it; see MacWhinney et al., this volume.

“alternative ways” do not battle it out—either because all of them normally coexist happily or because the particular message to be expressed is so rare that alternatives are never modeled—the child seems to be left with a rampant capacity for overgeneralization and very little to keep it in check.

Innate Constraints

Fodor and Crain (this volume) and Roeper (this volume) propose handling the “no negative evidence” problem by appeal to innate knowledge, although they do this in different ways.

Formal knowledge. Fodor and Crain’s approach is to equip the child with a “metalanguage” for constructing grammatical rules. This metalanguage constrains learners in such a way that they hypothesize only very narrow phrase structure rules that are justified by the data already observed. If two narrow rules are formally related, they can be collapsed to create a single, broader rule. However, the broader rule is based on fully instantiated, narrower rules, so children will not make syntactic overgeneralizations.

Fodor and Crain recognize that children produce lexical overgeneralizations of the types shown in Table 1. They suggest that these might be dealt with through preemption. In essence, then, they assume that syntactic and lexical overgeneralizations are inherently different, with the former requiring prevention and the latter being amenable to correction.

I have argued earlier that preemption, along with “retreat to a subset,” is insufficient to correct many errors involving lexical exceptions and that children must have other techniques for retreating from overly general rules of these types. Whatever these techniques are, it is possible that they can also be applied to overly general syntactic rules, which would make it unnecessary to constrain the child at the outset. However, children seem to make fewer syntactic than lexical overgeneralizations, and Fodor and Crain present some reasons for doubting even those syntactic ones that have been documented. So it may be desirable after all to somehow constrain children from the beginning in their construction of syntactic rules.

The success of Fodor and Crain’s approach will hinge critically on how they spell out their notion of “rule collapsing” beyond its schematic introduction in this volume. Their problem will be to define the circumstances under which rule collapsing takes place in such a way that the new, broader rules can never accidentally be too broad. What kinds of sentence representations will children rely on to determine “formal relatedness” between rules? Description at the level of the actual sentences instantiated in the input is clearly too limited, but the moment that more abstract representations are brought in (e.g., $VP \rightarrow V$; $VP \rightarrow V + NP$, as in a hypothetical example by Fodor and Crain), there is the risk that rule collapsing will result in overly powerful rules. I look forward to more concrete proposals from Fodor and Crain about the level of representation at

which all the desired generalizations can be captured without concomitant overgeneration.

Substantive knowledge. In this volume and elsewhere, Roeper proposes that children's grammars are held in check by knowledge of substantive principles of grammar. His specific hypotheses are rather complex, and each one requires separate evaluation (for example, see Smith, 1981 for a careful consideration of proposals in Roeper, 1981). Without going into too much detail, I would like to suggest that at least one of the hypotheses he puts forward in this volume may be vulnerable to a criticism I raised earlier for the "retreat to subset" approach.

According to Roeper, children know, without having to learn it, that although they can say things like (1a) *the city's destruction* they should not say things like (1b) **the play's enjoyment*. (1a) is allowed while (1b) is not because children have innate knowledge that only "affected objects" can be proposed in nominalizations. (This follows from more complex considerations about the establishment of case; see Roeper's discussion, this volume.)

This constraint has the effect of defining a subset within which nominalization can comfortably operate: nominalizations of the form "the X's Y" are all right as long as X specifies an affected object. Any positive exceptions could be learned from positive evidence, e.g., possibly "*China's recognition* by the United States," where China is not in any clear sense an "affected object." But what about negative exceptions, as discussed earlier? Such "invisible holes" include **John's hitting* (=the hitting of John), **the wine's drinking* (=the drinking of the wine) and **the hair's brushing* (=the brushing of the hair). Here, the proposed nouns are clearly "affected objects," yet the constructions are felt to be strange.

To be sure, Roeper could point to some differences between negative exceptions like **John's hitting* and acceptable nominalizations with gerunds like *John's trouncing* (=the trouncing of John) and *John's mugging* (=the mugging of John): the more strongly established the verb-ing form is as an independent noun, the better the construction sounds. However, these distinctions are not specified in the innate constraint proposed by Roeper. Unless he can find a principled solution (one that does not, as an accidental byproduct, rule out perfectly acceptable constructions in languages other than English), I am inclined to think that whatever techniques children use to determine the ungrammaticality of negative exceptions like **John's hitting* they can also apply to items like **the play's enjoyment*. If so, Roeper's innate constraint on what can be proposed would be unnecessary.

Does the "No Negative Evidence Problem" Really Exist?

In view of the difficulties I have raised for various approaches to the "no negative evidence" problem, the reader might wonder by now whether children

must not receive negative feedback after all. Several investigators have argued that they do, probably not in the form of explicit corrections but as misunderstandings, requests for clarification, repetitions, and recastings (e.g., Hirsch-Pasek, Treiman & Schneiderman, 1984; Demetras & Post, 1985). But I do not think the answer lies in this direction for the following reasons.

First, researchers who argue that children do get negative evidence from their speaking partners do not, in the studies I have seen, distinguish in the necessary way among responses to different categories of ungrammaticality on the child's part. Whenever negative feedback is observed for child utterances that are imperfect by adult standards, it is indiscriminately taken to count against the seriousness of the "no negative evidence" problem. But most of this feedback is simply irrelevant.

Many utterances, especially among younger children, are ungrammatical not because the speaker's rules are overly general but because the speaker hasn't yet constructed the necessary rules at all. (Omissions of grammatical morphemes are a case in point.) Adult misunderstandings, recastings and the like might or might not hasten rule construction in these cases, but they do not bear on the problem of how children cut back on overly general rules. Even when such feedback does follow upon errors resulting from overly general rules, these errors often involve "benign" rule exceptions, which, as noted earlier, can in principle be corrected *without* negative feedback. I do not know how much negative evidence remains after we eliminate these two types of irrelevant feedback, but I suspect it is not very much, especially since overgeneralizations of the types discussed in this chapter are produced by relatively old children and rarely cause misunderstanding (as noted also by Mazurkewich and White, 1984).

Second, listener misunderstandings, requests for clarification, repetitions, and recasts are not reliably diagnostic of ungrammaticality on the speaker's part: they follow well-formed utterances as well as those that are ungrammatical (Hirsch-Pasek et al., 1984; Demetras & Nolan, 1985). If a child's first impulse on hearing such responses is to question the adequacy of her grammar, she would continually be trying to revise perfectly acceptable rules. It seems unlikely that children are so readily led astray. Even if a child does on occasion question her grammar, only recasts give any information about where the problem lies—misunderstandings, repetitions and "what" questions are silent about what is wrong.

In sum, I conclude that the "no negative evidence" problem is not a myth, but a real and serious challenge for the construction of an adequate theory of language acquisition. I do not think this challenge has yet been satisfactorily met, but investigators are getting more ingenious in their ideas about it all the time.

WHAT DRIVES CHANGE IN CHILDREN'S GRAMMARS?

For many years a typical goal of research on children's developing grammars has been to provide a series of descriptions of what a child knows about language at

successive stages of development. This goal was in part encouraged by the "grammar-writing" methodology that dominated the field in the mid-'60s and early '70s: a grammar is in essence a static portrait of a linguistic system at a single point in time. The goal of successive descriptions is now making way for a new set of concerns, as investigators, many of them inspired by the demands of computer simulation of language acquisition, struggle to identify the factors that lead to *change* in children's grammars.

Failure-Driven Determinants of Change

According to Berwick (this volume), change takes place when the child's current grammar cannot parse an incoming string. If the string can be successfully parsed, the grammar remains as it was. MacWhinney et al. (this volume) sketch a more comprehensive set of circumstances under which change can take place, but, like Berwick's (and like Wexler & Culicover's, 1980; see Berwick, this volume) their approach is essentially failure-driven. For example, change takes place when the child reaches an impasse in comprehending or producing a sentence, or when the child's monitoring system detects a discrepancy between the way the child would express a given message and the way it is expressed in an incoming string.

Failure-driven mechanisms are surely necessary to account for the child's progression to a full adult grammar, but it seems unlikely that they are enough. Karmiloff-Smith (1979a,b, 1986) and I (Bowerman, 1982b,c, 1985) have both documented the emergence of errors that seem to reflect changes taking place in children's grammatical systems long after the learners are fully capable of parsing incoming sentences of the sorts in question, and are producing sentences that are indistinguishable from those of adults. These errors often suggest that the speaker has discovered relationships among lexical items that were not previously seen as related, or deep regularities linking grammatical subsystems that were earlier represented independently of each other in the learner's developing grammar. These relationships and regularities are sufficiently abstract that children do not need to be sensitive to them in order to understand others or to construct their own perfectly adequate sentences. What drives them on to find structure in language when that structure has no direct consequences for their ability to use language fluently?

A further problem for which failure-driven mechanisms do not provide enough help is how children cut back on overly general rules. If a child has formulated such a rule, he is capable of parsing or generating every utterance of the relevant type he will hear from adults, so no further change would be expected (except in cases where there is a preempting adult form). This is why Berwick (1985; Berwick & Weinberg, 1984) relies so heavily on the subset principle: within a model that makes changes only upon parsing failure, it is critical that the child should never formulate an overly general rule, since he would never be able to correct it. But, as discussed earlier, children do formulate

overly general rules that cannot be corrected by preemption. These are also a problem for MacWhinney et al.'s competition model, since when there is no conventional adult counterpart for a child's overgeneralization, the child's monitor can detect no mismatch and hence sees no reason to make a change.

In sum, change mechanisms that bring about only accurate parsing and adult-sounding utterances are not enough. Our theory of language acquisition is going to have to explain what causes grammars to change even when children receive no overt evidence that there is anything wrong with their current grammars.

Does Change Take Place On-line or Off-line?

Failure-driven mechanisms of language acquisition spring into action when the child is having trouble processing an incoming sentence or producing one himself, or when he detects a mismatch between his own utterance and an adult utterance. Let us term this the "on-line" theory of change: change takes place when the child is actually *using* language. This approach is particularly compatible with acquisition theories based on computer modeling, since computer programs require some clear-cut stimulus to jolt the grammar that is being constructed out of its current state and into a new one.

The on-line theory may be contrasted with another approach that for years has been assumed implicitly by the many researchers who view language acquisition as the construction of a grammar by a cognitively active, involved child. This is the idea that children (unconsciously) compare forms, extract regularities, and deepen their analyses "off-line," such that their grammars continue to develop even when they are not using them to process or produce speech. This approach is quite comfortable with evidence that children's grammars become more differentiated, better integrated, and more abstract even when children meet with no overt indications of trouble. (Of course, off-line theorists would not deny that change can take place on-line as well.)

Several authors in the present volume explicitly criticize the idea of off-line processing in language acquisition. For example, Fodor and Crain reject the hypothesis that children compute *nonoccurrences* of constructions their rules would predict on grounds that there is no plausible evolutionary reason why children should engage in this labor "on the side," when they are not actually exercising their rules to comprehend or to speak. And both Braine's and Fodor and Crain's chapters (this volume) criticize Pinker (1984) for assuming that children engage in complex, off-line surveying and analysis of inflectional and derivational paradigms. Braine argues that it is unlikely that children have the requisite long-term memory with a self-editing capability for carrying out such analyses, and Fodor and Crain don't see "why human beings should be designed to go to all this trouble, given that languages would be just as learnable without it as long as all generations of learners abjured it equally."

Although my own work on language acquisition clearly falls into the off-line camp, I must confess to growing discomfort with our collective difficulty in

casting off-line hypotheses in more precise terms that would explain exactly how, when, and why change takes place in the child's system. When Herb Clark, despite my best efforts at defense, suggested that we should call the off-line approach the "theory of immaculate conception," the joke hit too close to home for comfort.

Two solutions for the problem seem possible. One is that more rigorous theories will be developed that fill in the missing details about when and how changes take place off-line in a child's language system (see Karmiloff-Smith, 1986, for a start on such a theory). The other is that the off-line approach is in fact wrong, and that all children's language processing does take place in the context of using language.

If the off-line approach is wrong, any on-line approach that replaces it should be sensitive to those aspects of language development that failure-driven, on-line approaches have typically ignored. I think the parallel distributed processing model discussed by Rumelhart and McClelland (this volume) might have promise in this respect. The advantage of this model is that it provides a mechanism, spreading activation, that allows relatively remote corners of the child's grammar to be "contacted" and brought into communication with each other while the child is actually only engaged in comprehending or trying to produce a single utterance. This means that the processes described by off-liners in terms of the child's "comparing of related forms" can take place more or less automatically as the child uses language. Let me try to suggest how this mechanism might account for a kind of learning that failure-driven, on-line approaches have trouble with: the child's gradual restriction of productive prefixation with reversative *un-* to the class of verbs sharing a "centripetal" meaning (covering, enclosing, surface-attaching, etc.).⁶

According to a parallel distributed processing explanation, every time the child hears or uses a particular verb prefixed with *un-*, all the other *un-* verbs in her vocabulary are activated as well, although more weakly. Let us assume that the child's lexical entries for "legitimate" verbs like *uncover* and *untie* contain features like [+reversative] (for the *un-* segment) and [+centripetal] (for the base verb). In this case each time the child hears or uses an *un-* verb, all these entries are activated and the connection between the features [+reversative] and [+centripetal] has an opportunity to get strengthened. Assuming that *un-* verbs of the right semantic class predominate in the child's lexicon (even though she

⁶Failure-driven on-line mechanisms are insufficient here for two reasons: (1) the child who is not yet sensitive to the associated semantic class can still parse and understand any verb prefixed with *un-* that she meets in the input, and (2) she will not be able to detect her own errors, since, as noted earlier, *un-* errors do not compete with preempting forms in adult speech. Negative evidence is also not the answer. Any explicit corrections, misunderstandings, etc. from adults would presumably be general across all children's errors with *un-* prefixation. This feedback cannot explain why errors involving verbs of the "wrong" semantic class (e.g., *unhate*, *unstraight(en)*), die out while those with verbs of the "right" class continue unabated for a time (Bowerman, 1982b).

may have stored a few odd items of her own, like *unhate*; cf. Table 1), this connection will eventually get so strong that novel words constructed with *un-* will respect it, i.e., reversative *un-* will not be selected together with a verb that lacks the feature [+centripetal].

This account can in principle explain children's ability to home in on covert semantic, morphological, or phonological classes associated with particular rules. But it comes with a high cost. For the process to work, each lexical item would have to be entered into the child's mental lexicon tagged ahead of time with all the features that could possibly be relevant, in any language, to a semantically, morphologically, or phonologically constrained rule involving such a lexical item. Otherwise there would be no assurance that the activation process would contact and strengthen the features that are relevant to a particular rule in the particular language the child happens to be learning. But the number of such "possibly relevant" features, even if finite, is surely vast. It seems unlikely that children routinely mark their lexical items for the entire universal set (see also Pinker, 1984:168-171, who presents strong evidence that children do *not* engage in exhaustive *a priori* marking of this kind in the acquisition of inflections).

More plausible is that children enter at least some features into their lexical representations on the basis of evidence that these features are important in their language. But if this is so, parallel distributed processing loses much of its explanatory power and we are left with the same puzzle as before: how does the child identify these features as important if they are not present ahead of time to be strengthened through repeated activation?

The parallel distributed processing account would also, I think, have trouble with rules that are *negatively* constrained, i.e., that can apply *unless* such-and-such a condition obtains. For example, one of the constraints on "effect-complement" sentences of the type shown in Table 2 is that, as Green (1972) has pointed out, the "effect" must not be expressed with a past participle (compare, for example, *She combed her hair SMOOTH* and **She combed her hair UNTANGLED*; *She cooked the roast DRY* and **She cooked the roast BURNED/OVERDONE*).⁷ When a constraint involves something that existing instantiations of a rule do *not* have rather than something they *do* have, this property would not get activated during use of the rule. How could it then get strengthened and come to participate in a block against novel words or sentences that do have that property?

In summary, the opposition between on-line and off-line approaches has yet to be resolved. Both have their strengths and weaknesses, and both may ultimately play necessary roles in an adequate theory of language acquisition. But the work of comparing the approaches has barely begun. From my own perspective, I would especially like to encourage on-line theorists to inspect acquisitional

⁷*Closed* and *shut* seem to be the only exceptions to this constraint (cf. Green, 1972).

phenomena that seem resistant to treatment within failure-driven and possibly also activation frameworks.

The Child as Sleuth: In Pursuit of Elusive Meaning Distinctions

As a final problem for our theory of what brings about change in children's grammars, let us consider what happens when a child is confronted with two forms that seem to have the same meaning. According to Clark's (this volume) Principle of Contrast, discussed earlier, the child should immediately begin trying to figure out how these words differ. If she finds a difference, all is well. If she cannot find a difference but only one of the forms is actually attested in the input (e.g., the other form is an overregularization produced only by her), she throws out the nonattested form and keeps the one she hears. Finally, if she cannot find a difference but both forms are clearly attested, she concludes (reluctantly, one imagines) that in this exceptional case there simply are two forms with the same meaning. Pinker's (1984) Unique Entry principle works similarly but for the more limited case of multiple entries competing for the same cell in an inflectional or derivational paradigm.

Each outcome is appropriate for some situations. For example, a child who at first thinks that 'to *break* something' and 'to *make* something *break*' are synonymous eventually discovers that they differ with respect to the directness, conventionality, etc., of the act of causation they specify (Bowerman, 1982a,c; Pinker, 1984:335-338). A child faced with *goed* and *went* eventually gets rid of *goed* since he or she doesn't hear evidence for it in the input. And a child who continually hears both *dived* and *dove* comes to accept both of them as instantiations of *dive* + PAST.

Now, the problem is this: When faced with uncontrovertable evidence in the input for two apparently synonymous forms, how do children know whether they should carry on in their attempt to discover a meaning difference or whether positive evidence entitles them to conclude that in this case there are two forms with the same meaning? More concretely, how do they know whether this is a case like causative *break* and *make break* or a case like *dived* and *dove*? This is an important question because the child's "decision" determines whether her grammar is still pushing toward change or has in a sense closed down, declaring itself done with this corner of the grammar.⁸

⁸These questions are most critical for Clark and others who believe that children have a generalized resistance to synonymy. For Pinker the problem does not arise as often since, according to his model, the child only resists synonymy when he finds more than one entry for a single cell in a paradigm. Pinker's child will also try harder to find a distinction between causative *break* and *make break* than between *dived* and *dove*, as is appropriate, since in the former case there is a doubling up of entries in a single cell across a large number of verbs (which leads the child to try to split the paradigm), whereas in the latter case doubling is restricted to only a handful of items (see Pinker, 1984:198).

Some meaning differences are no doubt so salient that children will recognize them quickly. Others, however, are subtle and are known to give children trouble (e.g., the distinction in Turkish between two forms of the past tense, one for past events known through direct perception and the other for those known only through inference or hearsay; Aksu, 1978). How long do children go on trying to find a meaning difference between two attested forms before they give up and accept the forms as synonymous? What happens if a child should give up on a tough meaning distinction too early?

So far I have considered this problem from the perspective of the Uniqueness/Contrast principle. But it is even more problematic for those who do not suppose that children have any *a priori* expectations that two forms should contrast in meaning. If children lack such a bias, they should be perfectly happy to treat two observed forms as synonymous. Their problem will be that they routinely stop looking for nonobvious meaning distinctions too early (see also Pinker, 1984:201–202). What sets the search into motion again?

Notice that an appeal to “mismatch” between what the child says and what adults say will not work here, since the child will associate both forms with the same meaning, so for her they can be used interchangeably. A child’s own misunderstandings of what adults say might serve as clues that something is amiss, but true comprehension errors must be rare, since the meaning the child associates with the two forms will typically be underdifferentiated and so will comfortably subsume either of the adult’s intended meanings. (For example, the Turkish child for whom the two past tense forms are synonymous will associate them both simply with “past time”; if she misses the adult’s more refined intention this will normally pass unnoticed by both speaker and listener.) Children might soon shape up if they were corrected for using the wrong forms in their own speech (e.g., the “direct experience” past tense form for an event only heard about), but, for reasons discussed earlier, it seems unlikely that there is enough of this kind of feedback to push children down all the paths they must follow in search of difficult meaning distinctions.

These are difficult problems, and I am not certain what the right approach to them is. But solving them is clearly essential to arriving at a satisfactory understanding of what drives change in children’s developing grammars.

CONCLUSIONS

In this chapter I have concentrated on two challenging issues for students of language acquisition: the “no negative evidence” problem and the question of what motivates change in children’s grammatical systems. I have pointed out some difficulties with hypotheses that are currently being explored and certain phenomena that still need to be accounted for.

The perspectives reflected in the chapters of this volume suggest that the extreme polarization between nativist and empiricist approaches to language

acquisition is diminishing. Although there are certainly advocates of each camp represented, there seems to be a refreshing recognition on the part of those invoking innate knowledge that it is important to account for what children actually do and, on the part of those adopting data-driven approaches, that we must outfit the learner with special sensitivity to those features of language that lead to desirable generalizations. A new type of model is starting to appear: Braine, Macken, and Pinker (all in this volume) propose hybrid approaches, borrowing ideas from both ends of the theoretical continuum and weaving them into novel configurations designed to capitalize on the strengths of each approach while minimizing associated weaknesses.

In closing, I want to mention a remaining obstacle to an adequate theory of language acquisition with which we seem to have made little progress, despite advances in other areas. This is the representation of *meaning* in the language learner.

Without certain assumptions about children's meaning representations, no theory can get off the ground. For example, the models of Berwick (this volume), Wexler and Culicover (1980), and Pinker (1984) all depend on children's constructing, independent of what they know about syntax, a correct representation of the thematic structure of simple sentences. Similarly, MacWhinney et al.'s simulation procedures give the learner a set of semantic features that are tailored to the characteristics of the sentences whose structure is to be learned. Langley and Carbonell, this volume, give a to-the-point critique, noting that all existing modeling systems cheat by "hand-crafting" the input to the model. The input contains exactly the right features for the language being learned, and often *only* those features, which reduces the learner's search problem.

How to get around this problem is not at all clear, but it is too important to neglect or push aside until other problems are solved. No model of how children learn to talk can make a strong claim to success until it is firmly rooted in a plausible theory of where children's meaning representations come from and how they are called on in the course of acquisition.

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