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CHAPTER 15

INPUT AND LANGUAGE ACQUISITION

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I. THREE METAPHORS

Three metaphors illustrate different conceptions of how input—usually parental speech—influences language acquisition. The first is a copy metaphor. The child copies what she hears, imperfectly at first and with greater precision as development proceeds, guided by input cues. The second is a hypothesis-testing metaphor. The child forms and tests hypotheses which may be innate or developed later; input serves as evidence confirming or disconfirming those hypotheses. The third is a trigger metaphor. The child is innately set to choose between two alternatives; input tips the choice one way or the other. Each metaphor incorporates a different picture of the child's innate structure and learning mechanism. Although the metaphors can apply to several aspects of acquisition, the focus in this chapter is syntax.

On the *copy metaphor,* the child gradually aligns her speech with that of her language community. Some biological substrate is necessary, as well as some predisposition to learn, but the focus is on an active role for input. Input embodies what the end state should look like and shapes—by an as yet ill-understood mechanism—an approximation of that state. Copy theories assume little linguistic knowledge in the initial state and fairly shallow linguistic knowledge at the end state. Much of the empirical work reviewed here on input and reply studies was initially motivated by the copy metaphor. The aim was to demonstrate that the input was both richer and cleaner than nativists had supposed, thus reducing the need for extensive innate knowledge.

The *hypothesis-testing metaphor* provides a different picture of acquisition.

Here, acquisition is nonconscious theory construction. The child forms and tests hypotheses about what structures exist in her language. Input is implicitly evaluated by the child in a way similar to the way experimental data are explicitly evaluated by scientists in theory construction, including an appreciation for the fact that the data may be misleading. On this metaphor, the child is not copying the input but is developing a theory that will explain the regularities in the input. Input is important but neither shapes nor molds the child. Hypothesis-testing theories vary in what kind of innate linguistic knowledge they assume the child has. The version I will concentrate on assumes an innate endowment consisting of the linguistic universals.

A brief discussion of universals will be useful to clarify what universals are and how they relate to different conceptions of language acquisition. Some universals are absolute. One example is that, in every language, a tensed verb has a subject. Other universals, referred to as parameters, are relative. An example of this is that the subject of a tensed verb can take one of two values. In some languages, such as English, the subject must be overt and explicit, as in (1), whereas in other languages, such as Italian, the subject can take an abstract, unpronounced form that appears null, as in (2). The direct English equivalent of (2), namely (3), is ungrammatical (and for that reason is marked with a star or asterisk). Hypothesis testing assumes the child innately knows that she must choose between the two possible values of the parameter concerning subjects and must make similar choices for the other dimensions of language.

(1) she is eating pasta
(2) mangia pasta
(3) * is eating pasta

The *trigger metaphor* has commonalities with hypothesis testing (for discussion and a new conception of triggers, see Fodor, 1998) but assumes more innate knowledge and a narrower and more restricted role for input. It is primarily used to explain how the child establishes parametric values and is thus more limited in its scope than copy or hypothesis-testing metaphors. On the trigger metaphor the child neither copies the input nor evaluates it. Rather, a given piece of input "triggers" the correct parametric value. Learning, as usually conceived, is not how language is acquired. Some writers, such as Goodluck (1991), have considered parameter setting and hypothesis testing to be two different names for the same model. However, the models do appear to differ in what kind of acquisition mechanism they entertain, even if they are alike in what innate endowment they assume.

The three metaphors do not exhaust the possible ways that input might affect language acquisition. But they summarize the principal *cognitive* roles that input could play. The main *motivational* role that input could play is to facilitate language acquisition by making the environment conducive to learning. Anyone who has attempted to learn another language after puberty finds that some circumstances encourage learning whereas others make it almost impossible. A smile from a native speaker rewards one's efforts, and a frown makes one stumble. Comparable influences may be at work in first language acquisition. They are, however, outside the focus of this chapter. The focus here is the impact of input on the *cognition,* rather than the *motivation,* of language acquisition.

Each of the three metaphors is considerably more complex than this brief introduction suggests. To develop them further requires looking at the data on the role of input in language acquisition. Data come from three types of studies. In *input studies* investigators examine characteristics of parental speech to children and correlate those characteristics with the child's development. The basic question is what features of the input affect the style or rate of acquisition. In *reply studies* investigators examine characteristics of parents' replies to children's speech in order to determine whether and how parents might subtly inform children that their speech is flawed and should be corrected. In *intervention studies* experimenters provide children with specially designed input in order to determine what features of input are most important in acquisition and how much flexibility there is in the acquisition process.

I will review the empirical data after discussing several terms concerning input that have come into common parlance via the linguistics and learnability literatures. The terms are *positive evidence, negative evidence,* and *indirect negative evidence.*

II. FORMS OF EVIDENCE

A. Positive Evidence

Positive evidence is sequences of words, or word strings (a string of words may or may not be grammatical), perhaps analyzed in whole or in part. Positive evidence confirms *or* disconfirms a parameter setting or a generalization. Positive evidence can be illustrated using the null subject parameter.

A child who is learning English will encounter sentences like (4). The subject of the sentence, *it,* is sometimes called expletive *it,* or dummy *it,* because it does not refer to anything. The *it* seems to be there solely to occupy the subject position. Expletive *it* does not exist in languages such as Italian and Chinese. Sentences like (4), as well as (1), are positive evidence.

(4) it seems that she likes pasta

Sentences like (4) are relevant to the null subject parameter, because the child who hears them has evidence that her language is not a null subject language. If the child had begun acquisition with the null subject value as the preset value, the evidence from (4) would help to disconfirm that value and force a resetting to the

other value. If the child had begun with the English-type setting, she would remain with that setting; the evidence would help to confirm that setting. Sentences with an expletive subject are positive evidence whether they disconfirm or confirm a parameter setting.

The term *positive evidence* can be confusing because the linguistic use is specialized. In a philosophy of science, or theory-construction, perspective, positive evidence is evidence that confirms a hypothesis, and negative evidence is evidence that disconfirms a hypothesis. In hypothesis testing, the term *evidence* is a completely theory-relative term. For one hypothesis a given piece of data is positive because it confirms the hypothesis, and for another it is negative because it disconfirms the hypothesis.

Linguists also refer to positive evidence as *primary linguistic data* and take it to be the only data that the child has at her disposal. That is, the linguist sees the child's input as consisting only of strings of words, plus whatever structural analysis the child can attach to those strings. From the perspective of hypothesis-testing, there are no a priori limits on the type of data the child will use as evidence. Her hypotheses determine what counts as evidence.

Whereas Chomsky (1965) proposed that the child's data consisted of strings, whether grammatical or ungrammatical (perhaps with associated structural descriptions), some authors specifically define positive evidence as grammatical strings (e.g., Berwick, 1985, pp. 85–86; Berwick also, however, recognized that the data may be noisy, pp. 94–96), and others are not specific about the composition of positive evidence. In general linguists and learnability theorists idealize the input as grammatical. But a psychologically plausible model of acquisition must include procedures for reducing the potential for harm that nongrammatical input poses.

To summarize, positive evidence consists of strings plus whatever syntactic analysis the child can attach to those strings. The strings, sometimes in combination with universal principles, entrench or dislodge a parameter setting or generalization. The hypothesis-testing theorist attaches a different meaning to positive evidence than does the linguist or learnability theorist; it is evidence of any sort, word strings or otherwise, that confirms a hypothesis. Positive evidence, as the linguist construes it, clearly exists. The child is surrounded by speech. *Input studies* (see the following) examine the role of positive evidence in children's language development. We do not know how the child makes use of speech, nor do we know if speech is the only datum the child attends to. Most linguistic accounts assume that it is.

B. Negative Evidence

Negative evidence, as linguists use the term, is information that a sequence of words, such as (3), is not in the language (e.g., 3) or is ungrammatical. It is a string which, as it were, is marked with an *.

There are various ways that a string could be tagged as ungrammatical in the input. If the child produced something ungrammatical, the parent could explicitly label it as incorrect, and even provide the correct alternative. The parent, for example, could say (5) to the child. Although many parents believe that they do just that, all studies that have investigated parental reactions show that parents of 2-year-olds do not overtly correct their children's speech (Brown & Hanlon, 1970; Demetras, Post, & Snow, 1986; Hirsh-Pasek, Treiman, & Schneiderman, 1984). Explicit correction is rare at any age and tends not to occur at all for children younger than 4 years old.

(5) don't say *Want banana;* say *I want a banana*

Nor do adults produce ungrammatical strings for children and then label them as ungrammatical. Parents might tell their children not to repeat a swear word they have just uttered, but they do not tell them not to repeat an ungrammatical string they (or others) have just uttered. It is agreed that explicit negative evidence is not available to children who are at the onset of learning language.

Generally, when linguists and psychologists speak of negative evidence they are referring to strings that have been tagged, in some way, as ungrammatical. Sometimes negative evidence refers to input in which each string is labeled as grammatical or ungrammatical. If ungrammatical strings are tagged as such, the remainder must be grammatical. That way of speaking about input is also described as informant presentation. It is as if a native speaker of the language informed one about the grammatical status of each string that one heard. (See Gold, 1967, for a learnability discussion of the different consequences of text [positive evidence only] versus informant [negative evidence as well] presentation.)

Negative evidence is not the opposite of positive evidence, except that positive evidence is an unlabeled string whereas negative evidence is a string the grammaticality of which is labeled. Input that is unlabeled with respect to its grammaticality—purely positive evidence—is less informative than input in which the ungrammatical strings are labeled as such. If negative evidence existed it would serve the dual functions of protecting the child from possibly being misled by ungrammatical data and giving the child information that a generalization is incorrect.

In large part because of Brown and Hanlon's (1970) findings, linguists have assumed that the only evidence available to the child is positive evidence. One consequence of the assumption of positive evidence only is that the child's initial parameter settings and generalizations are required to be such that positive evidence can overturn them if they are wrong for the language the child happens to be born into.

Several investigators, however, have suggested that parents provide children with more subtle and implicit forms of negative evidence (Bohannon & Stanowicz, 1988; Demetras et al., 1986; Hirsh-Pasek et al., 1984; Penner, 1987). Parents indicate by their requests for clarification, corrective repetitions, or failures to continue the conversation that a child's utterance is ungrammatical. The child can

decode such parental cues, and tag their faulty utterance as ungrammatical. *Reply studies* (see the following) examine the existence and role of such cues. Those cues, if verified, would constitute implicit negative evidence.

From a hypothesis-testing perspective, negative evidence has a different definition than the linguistic one. Negative evidence is data of any sort that disconfirm a hypothesis. What the linguist calls negative evidence the hypothesis-tester would classify as positive evidence, negative evidence, or no evidence at all, depending on its relation to the hypothesis being tested. Discovering that a string is ungrammatical could confirm a child's hypothesis about the nature of his language, could disconfirm it, or could be irrelevant to it, depending on what the hypothesis was.

C. Indirect Negative Evidence

Indirect negative evidence is the absence of a string which is "expected" to occur (Chomsky, 1981). Conceptually, it is the opposite of positive evidence and is unrelated to explicit or implicit negative evidence. Positive evidence is the presence of a string; indirect negative evidence is the absence of a (predicted) string. Because the set of absent strings is infinite, the qualification *expected* or *predicted* is necessary. Only the absent strings that the child expects to occur are indirect negative evidence. Thus, the question of whether indirect negative evidence exists turns on the question of whether the child has expectations.

To understand how indirect negative evidence is used, consider again the null subject parameter. Imagine that it is innately set for all children to the nonnull setting (the setting that requires overt subjects). The child therefore *expects* to hear expletive subjects, in the sense that her grammar predicts their presence. If her language were in fact Italian, she would never hear a sentence with an expletive subject. The absence of the expected input would constitute indirect negative evidence. Continued absence could thus result in a resetting of the parameter to the correct value (Chomsky, 1981; Lasnik, 1989). The child would infer that the absence of the expected strings was due to their ungrammaticality. The child could thus tag the absent sentences as ungrammatical, making them a form of negative evidence.

From a hypothesis-testing perspective, indirect negative evidence is indeed negative evidence. If the child has a hypothesis that a form is grammatical and will therefore occur occasionally and the form fails to occur, the child will infer that her hypothesis is incorrect and will alter it accordingly. (Hypothesis testing allows for other forms of negative evidence as well, such as the existence of a form that is contrary to prediction.) The category of indirect negative evidence is thus the only category where the linguist's usage and the hypothesis tester's usage coincide.

Having considering three metaphors for language acquisition and definitions of different types of evidence, we can now review the data on the nature of parental speech to children.

III. INPUT STUDIES

In input studies investigators tape-record child-parent (usually child-mother) pairs at a minimum of two different intervals, which can be referred to as Time 1 and Time 2. Typically, Time 1 is very early in development. The child is 2;0 (2 years, 0 months) or even younger, and the child's average utterance length (mean length of utterance, or MLU, measured in morphemes, whether bound or free; see Brown, 1973) is 1.5 morphemes or even fewer. Time 2, depending on the study, is anywhere from two to nine months after Time 1. At each time investigators measure a variety of characteristics of both the parent's speech and the child's speech.

Investigators are thus looking at the input to determine what the features of the child's positive evidence are, and how those features correlate with the child's development. The basic question input studies ask is whether there are any features of the parent's speech at Time 1 that predict how much progress the child will make in language development between Time 1 and Time 2. The basic answer, which will be elaborated later, is no. The inconsistencies among the findings, the small number of significant correlations, and the relatively large percentage of uninterpretable findings all suggest that those relations that have been reported are due to chance (Scarborough & Wyckoff, 1986; Schwartz & Camerata, 1985).

There are major conceptual, design, and statistical issues in input research, such as the choice of child and parental variables to measure, the size of each parent-child corpus, and problems introduced by variability among children at their initial measuring point (Furrow & Nelson, 1986; Furrow, Nelson & Benedict, 1979; Gleitman, Newport, & Gleitman, 1984; Newport, Gleitman, & Gleitman, 1977; Scarborough & Wyckoff, 1986; Schwartz & Camerata, 1985). The reader is referred to the sources just mentioned for extensive discussion of such issues.

Compared to speech to other adults, parental speech to children is shorter, is more intelligible, has fewer declaratives and more questions, and has fewer clauses per utterance (Newport et al., 1977; Snow, 1977). A natural first step in looking at the effects of input is to examine whether those special aspects of speech to children facilitate children's linguistic development. In addition to such features, there are other features of parents' speech, such as how frequently verbs are used, which are easy to measure and which might be relevant in acquisition, even if they are not especially characteristic of speech to children.

Scarborough and Wyckoff's (1986) study provides examples of commonly measured parental and child variables. Scarborough and Wyckoff looked at 17 features of parental speech and 5 features of children's speech. The measures of parental speech included the average length of the parents' utterances, how frequently parents produce different types of utterances (e.g., declaratives, imperatives, questions), how frequently parents use different parts of speech (e.g., verbs, pronouns), and how frequently parents expand their children's utterances or repeat their own utterances. The parental variables are heterogeneous, in that some, such as verbs per utterance, involve structural properties; others, such as self-repeti-

tions, involve discourse properties. In most cases, parental frequency of usage is the way a variable is measured.

The measures of children's language development, like the parental measures, are heterogeneous and typically involve frequency of usage. Progress in verbs, for example, is measured by the increase in how often the child uses a verb. The implicit assumption is that the more often the child includes a verb in an utterance, the greater the child's understanding of the syntactic and semantic properties of verbs. The child variables include average length of utterance, number of verbs per utterance, number of noun phrases per utterance, number of auxiliaries (auxs) per verb phrase, and noun inflections (plurals and possessives).

Newport and colleagues (1977, whose data were reanalyzed in Gleitman et al., 1984) motivated their choices of parental measures in part by taking variables that might be predicted to be important if one followed the logic implicit in foreign language teaching, where, for example, students first receive exposure to single-clause, affirmative, declarative sentences. The idea would be that language learning proceeds, as does other learning, from short, simple, basic structures to longer, more complex structures. That commonsense reasoning was explicitly subscribed to by Furrow and colleagues (1979) and can be seen as a version of the copy metaphor. First give the learner something easy to copy, and then progressively provide more complicated material to copy.

Newport and colleagues (1977) noted a difficulty with the simple-to-complex model. It seems unlikely that the model embodied in foreign language teaching is a good one for first language acquisition. (It may not be a good idea for foreign language learning, either.) Further, as Newport and colleagues pointed out, simplicity is not easy to define. There is no theory-neutral way of defining some structures as easy and others as complex. Imperatives, for example, are short and might therefore be considered simple. But imperatives also leave the subject *understood* and therefore might be considered more complex.

Many of the parental variables roughly fit the simple-first hypothesis. Declaratives are simpler than questions, single-clause utterances are simpler than multiclause utterances, short utterances are (generally) simpler than long utterances. One might also propose that measures like verbs/utterance or nouns/utterance measure complexity: the fewer the simpler.

But, even though questions are syntactically more complex than declaratives, they might be more attention-getting and thus be better input. Finally, not having a verb in an utterance makes it simpler in the sense that the utterance will probably be shorter but makes it more complex in that the meaning might be harder to discern. Also, because full grammaticality requires a verb, presenting a child with a large number of verbless utterances may mislead a child into thinking verbs are optional.

Versions of the copy metaphor animated a number of early studies, but because it, like the other metaphors, makes no explicit predictions about what features of

the adult input would facilitate or retard language development, most studies have been exploratory, looking to see if any input variables affect acquisition.

Four studies, because they use similar measures of parental and child speech, form a good database from which to determine whether any aspects of parental speech benefit or hinder the development of children's syntax. Furrow and colleagues (1979) investigated 7 child-mother pairs. Gleitman and colleagues (1984) reported on 12 child-mother pairs, divided into two groups of 6 on the basis of the children's age. Scarborough and Wyckoff (1986) included 9 child-mother pairs. Hoff-Ginsberg (1986) had the largest sample, 22 child-mother pairs.

Scarborough and Wyckoff's (1986) study is a good starting point for examining effects of input on language development. As already mentioned, they used many of the same variables used in other studies, thus allowing a close replication. In addition, the children they examined were as similar as possible at Time 1. Children in other studies have been considerably more varied at Time 1, thus introducing various statistical problems in interpreting later differences in their development. At Time 1 all of Scarborough and Wyckoff's children were 2;0, and their MLUs (measured in words) varied within the narrow range of 1.30 to 1.42, with an average MLU of 1.36. Time 2 for the children was six months later.

Scarborough and Wyckoff (1986) computed correlations between features of the adult input and children's development to determine, for example, whether greater parental use of questions at Time 1 resulted in more auxiliaries in the child's speech at Time 2 (a result that had been reported earlier). For the parents, 13 syntactic variables (ignoring the breakdown of yes/no questions) and 2 discourse variables were measured at Time 1. Children's increase from Time 1 to Time 2 was measured for 5 syntactic variables: MLU, verbs/utterance, NPs/utterance, Auxiliaries/VP, and inflections/NP. Because every adult variable was correlated with every child variable, that produced 75 correlations. Of those 75, 2 (2.6%) were significant at the .05 level, slightly fewer than the number one would expect by chance. Scarborough and Wyckoff's data suggest only chance effects.[1]

[1]In assessing the results for Scarborough and Wyckoff (1986) and for Gleitman and colleagues (1984), I have used the significance levels for their full corpora, not their split-half correlations. Following Furrow and Nelson (1986), I agree that the calculation of two separate split-half correlations serves only to reduce the database, and the small number of observations per parent-child pair is already a problem. If one were to use Scarborough and Wyckoff's split-half data, none of their results would be significant.

Statistical issues loom large in interpreting effects of input. Without a clear set of predictions, investigators are forced to examine every possible relation between the adult and child variables measured. But the larger the number of relations computed, the greater the likelihood that one will find spurious correlations. Thus, it is necessary to control in some way for that likelihood. One common solution is to take a conventional significance level, such as .05 (meaning that the result would occur 5/100ths of the time by chance), and divide it by the number of tests performed to obtain a new significance level. That would require Scarborough and Wyckoff (1986) to obtain correlations significant at the .0007 level, and none of their correlations came close to that level. Another procedure is to de-

Hoff-Ginsberg's (1986) child sample, at Time 1, ranged in age from 2;0 to 2;6, with an average age of 2;2, and ranged in MLU from 1.5 to 2.82, with an average of 2.05. Thus, both the age range and MLU range were wider compared to Scarborough and Wyckoff's (1986) sample. Hoff-Ginsberg used many of the same parental and child variables as Scarborough and Wyckoff. She observed the child-parent pairs on four occasions, each separated by two months. She computed three sets of 60 correlations between adult variables at Time 1 and child variables at Times 2, 3, and 4, for a total of 180 computations. At Time 4 (six months later), which was Time 2 in several other studies, seven correlations (12%) were significant at the .05 level or better, about double what one would expect by chance. (Hoff-Ginsberg adopted a stricter significance level of .01, to take into account the large number of correlations she was computing; two correlations at Time 4 met that criterion.)

Hoff-Ginsberg's data might thus be interpreted as showing a positive effect of input on rate of language development. But consider the nonreplication of significant findings from Times 2 to 4. Across the entire group of 180 correlations, 14 (7.8%) were significant at the .05 level (of which 6 were significant at the .01 level), slightly higher than one would expect by chance. Only 1 correlation—that between parental use of NPs/utterance and child use of NPs/utterance—appeared in more than one set of correlations. Because there is no theoretical explanation for such inconsistencies over different measuring points, I interpret them as chance effects.

The lack of consistency of effects within Hoff-Ginsberg's study is duplicated by a lack of consistency across studies. Despite the high overlap in the child and parent variables measured in the four studies we are comparing (Furrow et al., 1979; Gleitman et al., 1984; Hoff-Ginsberg, 1986; Scarborough & Wyckoff, 1986), there was dramatically little overlap in findings. Not a single correlation is significant in all four studies, or even in any three studies. In fact, only two significant correlations match in any two studies.

termine how many effects would be due to chance at conventional levels and attend to the results only if a much larger number of correlations were significant. In Scarborough and Wyckoff's case, 3.75 correlations (75 × 5%) would be expected by chance. They had 2, which again suggests chance effects. All effects, then, must be evaluated against the background of the number of tests performed.

An additional complication in input studies is that in general the sample size has been low. For Scarborough and Wyckoff ($n = 9$), correlations more extreme than $\pm.66$ were necessary for significance. For Furrow and colleagues ($n = 7$), correlations more extreme than $\pm.75$ were required. For L. Gleitman and colleagues ($n = 6$ in each of two groups), partial correlations more extreme than $\pm.88$ were necessary. Thus, although many of the reports of correlations appear numerically high, they fail to reach significance. As a result, one might be tempted to attend results more extreme than say, $\pm.50$, whether they are significant or not. Or one might not require significance at the .05 level.

Other data, however, suggest that it would be a mistake to adopt laxer criteria. Hoff-Ginsberg (1986), whose sample size was 22, only needed correlations more extreme than $\pm.42$ for significance at the .05 level, two-tailed. But few of her correlations were significant (see the following discussion for more detail), and the highest was .56. It seems likely that the population correlations are genuinely small, and that increasing the size of the sample will only reduce the size of the observed correlations. For those reasons, and because of the large number of correlations being calculated, it seems desirable to require *at least* the conventional significance level of .05. I have accordingly ignored findings that do not reach that conventional level, two-tailed.

We will examine those two correlations in depth to see how they might be explained. The first finding is that greater parental usage of yes/no questions (apparently including both those questions in which the auxiliary was inverted and those in which it was not) correlates positively with increase in children's use of auxiliaries in Furrow and associates (1979) and for the older of Gleitman and colleagues' (1984) two groups. One possible explanation is that questions are more likely than declaratives to include an auxiliary—the aux *do* only appears in questions (and negatives); the aux is often the first word and could therefore be salient. Both factors could lead to a highlighting of auxiliaries in the input, in turn resulting in faster growth of auxiliaries by the child.

The correlation between parental use of yes/no questions and children's growth of auxiliaries has often been mentioned as a robust finding, but it in fact has not replicated from study to study. Richards and Robinson (1993) noted that *intonation* yes/no questions were significantly correlated with auxiliary development in three studies (Furrow et al., 1979; Barnes, Gutfreund, Satterly, & Wells, 1983; Hoff-Ginsberg, 1986).[2] Intonation questions are those that omit the auxiliary, as in *that your bike?*.

The possible rationale just discussed for a connection between high parental use of yes/no questions and children's development of auxiliaries—namely, increased salience of the auxiliary when it appears at the beginning of an utterance—cannot explain the correlation with intonation yes/no questions, because the auxiliary is absent there. Those correlations are thus something of an embarrassment for a salience hypothesis.

Richards (1990) reanalyzed the data of Barnes and colleagues and included all speech directed toward the child, whether by parents, other adults, siblings, or peers. He also reclassified various input utterances, in particular using a stricter criterion for yes/no questions. His reanalysis showed that inverted yes/no questions in the input did predict auxiliary development nine months later, while intonation yes/no questions did not. Richards and Robinson (1993) cautiously noted, however, that other studies have been inconsistent in that finding. On balance, then, there is no reliable relation across studies between inverted input questions and children's auxiliary development. The finding of a correlation between parental questions and children's increased use of auxiliaries has no obvious explanation and is most likely to be a chance effect.

[2]I do not review the data from Barnes and colleagues (1983) because of the variability of their sample at Time 1. The children's ages ranged from 1;6 to 2;9 and their MLUs ranged from 1.0 to 2.21.

The second finding that matches in two studies is that parents' use of imperatives is negatively correlated with children's increase in verb usage (Hoff-Ginsberg, 1986, at Time 4; the younger of Gleitman and colleagues', 1984, two groups). That finding is also an embarrassment for a salience hypothesis. Just as children's aux use should increase with increased parental use of inverted questions, children's verb use should increase with increased parental use of imperatives. In both cases the relevant word is highlighted at the beginning of the parental utterance—auxs for questions and verbs for imperatives. Yet in the former case the relation, if it exists at all, is positive and in the latter the relation, if it exists at all, is negative.

The comparison of the effects of yes/no questions and imperatives is important for several reasons. Investigators do not want to have to rely on post-hoc explanations for each significant correlation. What would lend credibility to the sporadic findings of input effects would be a unifying explanation, like salience. If salience of an item in the input is hypothesized to lead to faster learning of that item by the child, that theory can be tested by comparing the effects of input utterances in which an item is salient with those in which it is not salient. If the child learns auxiliaries more rapidly because the input more often presents them at the beginning of a sentence, then the child should learn verbs more rapidly if the input more often presents them at the beginning of a sentence. Finding contradictory results in the two cases suggests that the salience hypothesis is false, or that the input effects are unreliable, or both. Conversely, if auxiliary salience is important to learning, then a large number of declaratives with auxiliaries should be negatively correlated with children's auxiliary development. That particular relation has never been tested.

Richards and Robinson (1993), in an attempt to test a salience hypothesis, used exactly the logic just described. They focused on the verb *be* used as a main verb and hypothesized that the more input children received in which *be* was salient (as in a greater proportion of inverted yes/no questions or as the final word of a sentence), the greater the children's later use of *be* would be. Correspondingly, they predicted that some input would *not* correlate with children's development of the use of *be*. For example, use of auxiliaries in yes/no questions should not correlate with *be* development, because auxiliaries are a different category from main verb *be*. Unfortunately, they did not predict any negative relationships, such as between use of *be* in non-salient positions and children's development in use of *be*.

Richards and Robinson (1993) analyzed data collected by Barnes and associates (1983), restricting the analysis to 33 children between 1;9 and 2;0 and with an MLU range between 1.30 and 2.05. There were four tapings at three-month intervals. They included as input all utterances addressed to the child, whether by parent, adult, sibling, or peer, and examined the effects of 15 different variables. They correlated the input at Time 1 with the child's use of *be* at Times 2, 3, and 4. Because children produce both full and contracted forms of *be,* the two were examined separately. For full child forms, then, Richards and Robinson computed 45 correlations, and for contracted child forms they computed another 45 correlations.

There were no significant correlations among the 45 computed for children's full forms, even at Time 4, when children were producing a fair number of full forms. Of the 45 computed for contracted forms, 2 were significant at the .05 level, two-tailed, which is what one would expect by chance. (An additional 4 were significant at the .10 level, but 13% is only slightly above what one would expect by chance at the .10 level.) Richards and Robinson (1993) suggested, however, that the number of significant correlations, though they perhaps suggest null findings, has to be considered in the light of their pattern of predictions.

In particular, Richards and Robinson (1993) noted that the correlations they predicted would not be significant were not, and of the 5 which they predicted would be significant, 3 were. Richards and Robinson have been cautious about the reliability and generalizability of their findings. But even more caution may be needed. Given that most input studies find few correlations, predicting the absence of a correlation is not making a very strong prediction. The absence of a correlation is the norm, and although 3 of the 5 predicted correlations were confirmed, only 1 was significant at the .05 level or better. Thus, rather than 60% of the predictions being borne out, 20% were. (Further, another significant correlation had not been predicted, although Richards and Robinson have a plausible post-hoc explanation for it.) Taken all in all, the salience hypothesis has little evidence of its favor.

One might suggest that, across all studies, it would be fruitful to look for similarities in the direction and strength of correlations and relax the criteria for accepting significant correlations. Even so, one finds that inconsistencies in strength and even in direction of findings are more common than similarities. Take, for example, parental MLU. Gleitman and colleagues (1984) showed a strong positive relation between it and increased use of auxiliaries for the younger of the two groups of children whom they observed. Scarborough and Wyckoff (1986) reported an insignificant positive correlation between those two variables. Furrow and colleagues (1979) reported an insignificant negative correlation, and Hoff-Ginsberg (1986) showed correlations close to zero at all three time periods. Parental MLU appears unrelated to children's development of auxiliaries.

As another example, Furrow and colleagues (1979) showed a strong negative relation between parental MLU and children's MLU, children's verbs/utterance, and children's NPs/utterance. Other investigators show no relation or, in one case, a strong *positive* relation between parental MLU and children's increase in NPs/utterance (Hoff-Ginsberg, Time 4, 1986). Such inconsistencies with very basic measures suggest that the strong effects that are reported are chance effects.

Even individual correlations are hard to understand. Furrow and colleagues (1979) reported that the more verbs parents use, the *slower* is the children's rate of increase in use of verbs. Scarborough and Wyckoff's (1986) strongest correlation is uninterpretable. The more inverted yes/or questions a parent uses, the greater

the increase in children's noun inflections. Gleitman and colleagues (1984) found that the more *un*intelligible the parents' speech was, the more rapid was the child's increase in number of verbs per utterance. The correlation was an astonishing .99. Taken all in all, the studies suggest no relation between the parental and child variables that have been measured.

Hampson and Nelson (1993) have suggested that the effects of input may only be evident very early in language development, and only for some children. Hampson and Nelson visited 45 children first when they were 13 months and then when they were 20 months. They found that some features of parental speech predicted children's MLU development, but only for children who were characterized as nonexpressive (i.e., used nouns for more than 40% of their vocabulary). They suggest that the noneffects so frequently observed are due both to looking at children who are too old and to looking at children as a whole, rather than at subgroups of children.

The most important implication of Hampson and Nelson's (1993) results is that, even where effects of parental input are found, they are minimal. The nonexpressive and expressive children they observed were equal in their MLU development; neither group progressed faster than the other. Further, the parents in the two groups were very similar in their provision of different types of input. The finding is that, within the nonexpressive group, greater or lesser provision of certain features of the input predicted the children's MLU development, whereas in the expressive group development was unrelated to the input. Some children may depend on certain features of the input more than other children, and for those children only, having more of those features will result in faster development.

If Hampson and Nelson (1993) are correct, one would not expect syntax development to be affected by parental input, because little syntax is present at the early ages at which they have found effects. It may be, however, that, for older children, there are other consistent individual differences, so that if one could partition the older children, parental effects would emerge. It remains to be seen to what extent individual differences will be important in accounting for the effects or noneffects of parental input.

To summarize, there is no evidence that any of the syntactic input variables has any effect on any child measure. The few correlations that have been reported appear best explained as chance effects. Scarborough and Wyckoff (1986) noted that parental input may so plentifully contain the examples children need to learn language that the variations in parental frequency are of no consequence. The child may need a certain low level of input examples in order to learn language, which every environment provides. More input, beyond the necessary minimum, may be irrelevant.

Thus far the discussion has focused on syntactic properties of parents' utterances, but inconsistencies also hold for the effects of discourse properties of parental input. Many investigators have noted parents' tendency to repeat part or

all of a child's utterance, with additional material that looks syntactically informative. For example, the child might say (6), and the parent might reply with (7), repeating the child's utterance and adding the missing verb. Such parental responses have gone by various names and have had different operational definitions, including *expansions* (Brown, 1973) and *recasts* (Baker & Nelson, 1984). There is also a category called *extensions* (e.g., Barnes et al., 1983), in which the parent might reuse an important lexical item the child had used, but add significantly to it.

(6) that the last one
(7) yes, that's the last one

Gleitman and colleagues (1984) reported that the more expansions parents used, the larger the increase in children's auxiliaries, but only for their younger age group. Scarborough and Wyckoff (1986) and Hoff-Ginsberg (1986) reported no effects of expansions on any aspect of children's development. Barnes and associates (1983) reported that extensions, but *not* expansions, correlated with a more rapid increase in children's MLU (but not with other child measures). Here, too, the scattered significant results appear to be chance effects.

In addition to repeating part or all of the child's utterance, parents sometimes repeat part or all of their own utterance. Ringing the syntactic changes on a theme could be informative to the child. Again, results are contradictory. Hoff-Ginsberg (1986) reported that parental self-repetition was positively related to children's development of MLU and VPs/utterance at Time 2 (but not at Times 3 and 4); Scarborough and Wyckoff (1986) showed no relation; Gleitman and colleagues (1984) showed a strong negative correlation between parental self-repetition and children's development of MLU and Auxs/VP for their younger group. Again, the pattern of results is what one would expect on the basis of chance.

On balance, what is overwhelming is the absence of interpretable effects within each study and the absence of consistent effects across studies (see Pine, 1994, for a similar conclusion). Given the data, affirming that any of the measured parental variables is relevant to any of the measured child variables is unjustified (though see Sokolov & Snow, 1994, for the opposite conclusion). At a minimum, we can conclude that investigators have been looking in the wrong place for effects of input. We know that input has *some* effect, because children grow up to speak the language of their community. But the mystery of how children make use of input will not be elucidated by continuing to look at measures like parental MLU or parental verbs per utterance.

Recall that almost none of the syntactic variables in the input have had any theoretical rationale. Some variables, such as low MLU, are characteristic of parental speech. Others, such as number of verb phrases per utterance, are easy to measure. But, aside from the notions of *simplicity* and *salience,* there has been no conceptual justification for choosing those input variables. We have already seen that what

counts as simple input depends on one's yardstick. From one point of view, short utterances are simple, but from another utterances that clearly display the syntactic parts of a sentence are simple. Salience is also difficult to define. On general cognitive grounds one might propose that having an element at the beginning or end of a string of words will make it salient (Richards & Robinson, 1993; Slobin, 1973). That would certainly be the case if the string of words were an unstructured list. But the language-learning 2-year old is probably treating a string of words as a structured and meaningful utterance. In such a case salience will be determined by linguistic as well as by cognitive principles.

If we return to the three metaphors with which we began, we can see that the copy metaphor has dominated research on input effects. With the discovery that parental speech to children was shorter, cleaner, and clearer than adult-to-adult speech, investigators hypothesized that clean and clear input could render at least some innate knowledge unnecessary (Furrow et al., 1979). Although that idea is still current (e.g., Bates & Elman, 1996), it involves a misunderstanding of the main justification for nativism. The justification is not that the input is noisy but that the input is impoverished. The poverty-of-the-stimulus argument was most recently and succinctly restated by Clark, Gleitman, and Kroch (1997). Language data do not come with instructions on how to analyze them, as the inability of chimpanzees to master a syntactic system makes clear. A learning device can only learn what its structure permits it to learn.

That should not, however, be interpreted to mean that input carries no information for the child. The learner's structure allows it to analyze the input. Without input, learners do not create a full syntactic system. The work of Goldin-Meadow and her colleagues demonstrates that young deaf children who are not exposed to sign language create sign combinations that have a more sophisticated structure than the rudimentary signs of their parents (see, e.g., Goldin-Meadow & Mylander, 1988; Goldin-Meadow, Mylander, & Butcher, 1995). Nevertheless, such children do not create a full syntax. There are limitations to what children can develop in the absence of language input. Innate knowledge is necessary to organize language input; structured input is necessary for the development of full linguistic knowledge.

Thus, although input studies have produced no robust findings, we should not draw the conclusion that input is unimportant. Rather, we must conclude that we have not discovered how to examine the interaction between the learner and the input. From the null findings, we know we must look at measures of adult and child speech other than the global ones examined so far.

The hypothesis-testing and trigger metaphors are alike in treating input in terms of how it will bear on particular choices the child makes about language. For that reason, the focus will be on individual structures and the data needed to understand their structure. For example, the child must learn about subjects and determine

whether the target language is similar to English or Italian. Both metaphors suggest that certain information in the input will be important. Expletive *it,* as in (4), will be one important piece of information, because it only occurs in languages like English (Hyams, 1986; Valian, 1994).

For the trigger metaphor, a single instance of *it* in the input could be enough to trigger the English setting of the parameter, making frequency largely irrelevant. All that matters is that the input provide some baseline number of examples, which is guaranteed if the child is exposed to a native speaker because all native speakers will automatically use a variety of sentence structures.

For the hypothesis-testing metaphor, a single instance is unlikely to be enough input. The child is testing hypotheses against evidence. A single instance could be a random occurrence that does not correctly represent the language. Frequency is likely to be important in the hypothesis-testing metaphor because the more opportunities the child has to confirm or disconfirm hypotheses, the faster the confirmation process will be.

Some research has suggested that sheer amount of input is relevant to acquisition (Barnes et al., 1983; Gathercole, 1986; Huttenlocher, Haight, Bryk, Seltzer, & Lyons, 1991; Naigles & Hoff-Ginsberg, 1988), especially vocabulary acquisition. But no input studies have specifically looked at the frequency of a particular form hypothesized to be important in acquisition of a particular structure, rather than a particular lexical item. We also do not know whether absolute or relative frequency of input is important (Hoff-Ginsberg, 1992). Input studies have not provided data that would allow one to test hypothesis-testing or trigger metaphors of acquisition.

IV. REPLY STUDIES

Despite the limitations and problems in correlational studies, satisfactory alternatives are not obvious. One alternative is parental reply studies, which examine the responses parents make to children's well- and ill-formed utterances. The goal of such studies is to determine whether parents give children useful clues about which of their utterances are ungrammatical and about how to make appropriate changes in their grammar. The search is for subtle forms of negative evidence.

If the child could determine from adult replies that her grammatical utterances were in fact grammatical and her ungrammatical utterances were in fact ungrammatical, the possibility for a direct didactic influence of feedback would be supported. Like any learner, the child who can tell that she has produced something incorrect is in a better position to figure out how to make corrections. Such a picture of language acquisition could be seen as a copy + correction metaphor. The

learner tries to copy the input; when the child succeeds the parent signals that the child was successful, and when the child fails the parent signals that an error was made.

Recall that parents do not explicitly approve or disapprove of 2-year olds' grammatical or ungrammatical utterances. When parents correct children, they correct the factual content of the child's utterance, not its grammaticality (Brown & Hanlon, 1970; Demetras et al., 1986; Hirsh-Pasek et al., 1984). But although adults do not *explicitly* correct children's syntactic errors, they might *implicitly* correct them. That is the interest of reply studies, which look to see whether parents provide children, almost always 2-year olds, with implicit clues. Exactly how the child would learn how to change her grammar on the basis of such clues is unclear, but the first step is to determine whether parents do differentiate between children's grammatical and ungrammatical utterances.

Penner (1987) and Bohannon and Stanowicz (1988) have reported that parents repeat verbatim children's grammatical utterances more than their ungrammatical ones, though some verbatim repetition occurs to each. That finding makes intuitive sense. Parents should be very unlikely to repeat verbatim an utterance that they as native speakers would regard as outside the language.

Penner (1987), Bohannon and Stanowicz (1988), and Furrow, Baillie, and McLaren (1993) have all reported that parents expand or recast children's ungrammatical utterances more than grammatical ones. Again, some expansions occur to both types of utterances. Hirsh-Pasek and colleagues (1984) reported that parents repeated children's ungrammatical utterances more than grammatical ones. Because they included parental expansions in their definition of repetition, their results could primarily be reflecting the effects of expansions. That consistent finding also makes intuitive sense.

Demetras and colleagues (1986) did not examine parental expansions, but did look at how frequently replies of four children's parents continued the conversation, or moved it on, and found that "move-ons" were more frequent to well-formed utterances than to ill-formed ones. That finding has since been replicated by Furrow and colleagues, 1993, with three child-parent pairs. The framework that Demetras and colleagues (1986) provided allows one to understand such a pattern. The child's ill-formed utterance is less likely to be understood by an adult than a well-formed one. Parents will be more likely to expand in some way on the ungrammatical utterance, perhaps in order to establish the child's meaning. Correspondingly, they will be more likely to continue the conversation if the child has spoken grammatically.

Taken at face value, then, the reports of how parents reply to children support the notion that parents treat grammatical and ungrammatical child utterances differently. The reports also provide a communicative framework for understanding how such differential responses could come about. Although the work leaves open how the child would utilize the parental replies to change her grammar, it does suggest that there are frequency asymmetries in parental replies that children could exploit.

There are, however, reasons to doubt the findings. Close examination of the previous studies reveals that different coding schemes were used from one study to the next in determining what counts as a grammatical child utterance and what counts as a parental repetition or expansion. As a result, it is difficult to compare the previous studies to establish whether parents do or do not distinguish children's grammatical and ungrammatical utterances. Similar terms are used from study to study, but they do not refer to the same entities.

More important, the categories used for classifying children's utterances and parental replies may have produced artifactual results. The importance of how one defines grammaticality is obvious. If investigators call strings that are not grammatical grammatical, the results will be different than if they call those same strings not grammatical. Similarly, how one defines repetition or expansion will be crucial. Given the critical importance of the classifications schemes, it may seem surprising that there is not more uniformity from study to study. But the reasons for differences in how investigators code grammaticality become clear as soon as one attempts a classification.

The first issue concerns *kind* of grammaticality. Some investigators (e.g., Demetras et al., 1986) have considered an utterance ill formed if there was *any* type of infelicity, whether phonological, semantic, pragmatic, or syntactic. Others (e.g., Penner, 1987) have focused on syntactic ill-formedness. Parents might well respond differently to different types of infelicities. Only by separately comparing responses to different types can the investigator decide whether they can safely be combined, but no studies have compared responses to different types of errors. For that reason, it is not possible to meaningfully compare studies that merge types of ungrammaticality with those that are confined to syntax.

The second issue concerns *criteria* for grammaticality. Even in studies that are confined to syntactic definitions of well-formedness there are problems in classifying utterances. Although there are many clear cases of grammatical and ungrammatical utterances, there are many unclear cases. Grammaticality is not an everyday commonsense notion. Recourse to some theory is necessary in order to direct the coding scheme.

The most difficult utterances to classify can be illustrated by reference to the following fictitious postcard: "Arrived in Italy on their independence day. What a mistake! Searched for a hotel for hours. Restaurants closed. Still, had a glorious time—impossible not to in Italy. Wish you were here. Come, too, next time?" On a strict definition of grammaticality, in which a string must be a complete sentence in order to be grammatical, not a single string in our postcard is grammatical. Subjects are missing, main and auxiliary verbs are missing, determiners are missing. Yet each "sentence" is *acceptable* in the postcard context.

Everyday speech contexts also allow a relaxation of grammatical constraints.

Adults say things like (8) through (11). In (8), a subject is missing; in (9), both a subject and auxiliary are missing; in (10), the word *what* is missing; in (11), the determiner is missing.

(8) want lunch now?
(9) feeling tired?
(10) time is it?
(11) computer's down again

Adults also produce fragments that are otherwise grammatical as answers to questions, such as responding to (12) with (13). Answers like (13) are acceptable, even if not fully grammatical.

(12) when are you going to California?
(13) on Thursday

Most linguistic theories equate grammaticality with sentencehood. Any string of words that is a sentence is grammatical, and any string of words that is not a sentence is not grammatical. A sentence must have a subject NP (although that subject does not have to be overtly expressed in all languages) and a verb. Depending on the verb, an object NP may or may not also be required. Depending on the noun, a determiner may or may not be required.

How should acceptable examples be handled? Most investigators, beginning with Brown and Hanlon (1970), have classified acceptable utterances as grammatical. They have done so because adults produce utterances of that type. According to a strict syntactic definition, however, acceptable utterances are not completely grammatical.

Nor, of course, are they ungrammatical in the same way that, say, (14) or (15) are. Acceptable utterances occupy a middle ground. No context will render (14) or (15) acceptable. But there are contexts that render the other examples acceptable, even if it is sometimes hard to specify what they are.

(14) * Mary the saw ball
(15) * to whom did they disappear before speaking?
[Base form: they disappeared before speaking to whom?]

A coding system should reflect, on the one hand, the difference between fully grammatical utterances and acceptable ones, and, on the other hand, the difference between out-and-out ungrammatical utterances and acceptable ones. That means a trichotomous division rather than a dichotomous one, a division in which acceptable utterances are a category of their own. Prudence alone would dictate a trichotomy, because with it one is in a position to examine the relations among the three categories, how they change as a function of the child's age and MLU, and whether parents respond differentially to the three types.

If acceptable utterances act exactly like grammatical utterances, the two categories should intercorrelate highly and should be responded to similarly by adults. In that case merger of the two categories is appropriate. But if the categories reflect different aspects of language knowledge and use, the patterns of correlations should be different, and the categories should be kept separate in subsequent analyses.

One reason to think acceptable utterances are *not* like grammatical ones is that most acceptable child and adult utterances are fragments that are typically answers to questions. A parent might reply differently to a child when participating in a sequence of questions and answers than when participating in other types of discourses. If children's acceptable utterances are combined with their grammatical ones, and if parents reply differently to acceptable utterances than they do to either grammatical or ungrammatical ones—because of the special discourse properties of acceptable utterances—then parents may appear to be distinguishing grammatical and ungrammatical utterances when they are doing no such thing.

If acceptable utterances are tabulated separately, it is possible to compare parental responses to purely grammatical, purely ungrammatical, and acceptable child utterances. If parents genuinely distinguish grammatical and ungrammatical utterances, there should be evidence of that even when acceptable utterances are removed. Further, the pattern of replies to acceptable utterances should be the same as that to grammatical utterances.

An in-depth look at children's utterances shows that even a three-part division of grammatical, ungrammatical, and acceptable is insufficient. Children at very low MLUs (e.g., below MLU 2.0) produce a large number of utterances consisting solely of single nouns. The syntactic status of single nouns is very difficult to determine. For that reason some investigators (Brown & Hanlon, 1970; Hirsh-Pasek et al., 1984) eliminate all one-word utterances from analysis. Especially at low MLUs, however, single nouns are a large percentage of children's productions. Eliminating single nouns means eliminating more than a quarter of some children's productions. A better solution to eliminating single nouns is to place them in a separate category.

Finally, both children and their parents produce imitations. Those too are also hard to classify, because imitations are not necessarily the direct output of the speaker's grammar. Imitations, too, should go into a separate category. If those recommendations are followed, there will be five major categories of usable child utterances: grammatical, ungrammatical, acceptable, single nouns, and imitations.

Valian (in press) examined spontaneous speech from 21 child-mother pairs. The children ranged in age from 1;10 to 2;8. The coding scheme developed in my laboratory established the five categories just described. We also developed a coding scheme for classifying adults' replies. The aim of that scheme was to reduce the likelihood of inflating differential responding to children's grammatical and ungrammatical speech. We separated verbatim repetition from *structurally similar replies.* Structurally similar replies could either expand *or* reduce the child's utterance while keeping the basic vocabulary and syntactic structure the same. That

category is similar to others' categories of repetition or expansion (see Valian, in press, for more detail).

The principal findings (Valian, in press) were that parents did *not* respond differently to children's fully grammatical and ungrammatical utterances, but they *did* respond differently to acceptable utterances. For example, parents repeated verbatim children's fully grammatical utterances about 5% of the time and repeated children's errors about 2% of the time, a nonsignificant difference. But they repeated acceptable utterances verbatim about 10% of the time, significantly different from both fully grammatical and ungrammatical utterances. Recall that previous work classified acceptable utterances as grammatical and found that parents repeated grammatical utterances more than ungrammatical ones. That finding appears to have been an artifact of scoring. Acceptable utterances play a special discourse role, often being part of question-answer games between child and parent. The parent's repetition is part of the game.

Parents gave a structurally similar response to children's grammatical utterances about 34% of the time, and to ungrammatical utterances about 39% of the time, again a nonsignificant difference. But they gave a structurally similar response approximately 24% of the time to acceptable utterances. Again, recall that previous work found that parents expanded or provided implicit corrections more often to ungrammatical than grammatical utterances. If acceptable utterances are included with grammatical ones, that will reduce the apparent amount of structurally similar responding to grammatical utterances.

In sum, parents do not appear to distinguish between children's grammatical and ungrammatical utterances. What the linguist calls negative evidence does not exist in either explicit or implicit form. Previous reports to the contrary are due to artifacts (see Valian, in press, for more detail). Children cannot exploit parental differential responding in order to determine which of their utterances is grammatical or ungrammatical, because parents do not differentially respond.

Even if parents did respond differentially, children could not in principle make use of the types of differences that have been reported. Assume (counterfactually) that parents do provide structurally similar responses more frequently to ungrammatical utterances than to grammatical ones. On that scenario, the child produces an utterance the grammaticality of which he is unsure, and the parent gives a structurally similar reply. From that response the child cannot tell whether the parent's repetition belongs to the smaller group of grammatical utterances that get altered or the larger group of ungrammatical ones that get altered. The child could not, in principle, make use of frequency asymmetries. (See Gordon, 1990; Penner, 1987; and Pinker, 1988, for similar points.) Therefore, the didactic role of parental feedback cannot possibly be one involving frequency asymmetries for different types of responses. Such asymmetries do not exist, and that is just as well because the child would not be able to make use of them even if they did.

Demetras and colleagues (1986) and Bohannon, MacWhinney, and Snow (1990) have objected to criticisms of the value of implicit parental corrections. Demetras and colleagues state that children learn other pieces of grammatical information that are not categorical. For example, they point out that although determiners occur before nouns, they do not always occur—some nouns are bare. Similarly, although *-ed* occurs after verbs, some verbs have no endings. Yet children learn to distinguish them. The observations are correct, but do not meet the criticism. Even if the syntactic markers are not omnipresent, they are reliable. When they do occur, they are good indicators. In that respect, they are unlike parental expansions, which are not reliable indicators.

The second response Demetras and colleagues (1986) made was directly addressed to the criticism. They noted that some markers, such as the *-s* ending, are present on both nouns and verbs, yet the child learns to distinguish nouns and verbs. But that response presupposes that the child would be able to learn the difference between nouns and verbs if the only marker either of them had was a marker that they shared. The presupposition is unproved and could well be false. Even if the presupposition were true, the response would still not meet the criticism because the child would not be learning the difference between nouns and verbs on the basis of the *-s* ending, but despite the confusion engendered by the shared ending.

Bohannon and associates (1990), also addressing the criticism, alluded to probability learning, in which only a subset of an organism's responses receive correction. That allusion is not relevant to the criticism. The criticism is not that only a subset of the child's *ungrammatical* utterances are expanded, but that a subset of the *grammatical* ones are also expanded.

Probability learning has been investigated in animal learning experiments. An animal is reinforced according to two different schedules for pressing two distinct bars, one on the left and one on the right. The animal already knows the difference between the left-hand bar and the right-hand bar. The animal is not learning how to tell the difference between the two bars, but is learning something about how often each produces a reward when pressed. In that situation the animal ends up pressing both bars, spending more time on the bar with the greater probability of reinforcement (referred to as the matching law, Herrnstein, 1970).

The analogue to the animal's left-hand bar and right-hand bar is the child's grammatical and ungrammatical speech. The analogy presupposes that the child has already distinguished grammatical and ungrammatical utterances. But if that is the case, the parental difference in expansions is not teaching the child what is grammatical. More important, probability learning shows that an organism's likely response to having two behaviors reinforced at different rates is to produce those two behaviors at different rates, corresponding to the reinforcement rates. On that model, the child would never eliminate, or even come close to eliminating, ungrammatical utterances.

A separate criticism of implicit negative evidence concerns the mechanism. The child is supposed to compare the syntactic structure of her utterance with the syn-

tactic structure of the parental response. From a mismatch the child concludes that her utterance was in error and she changes her grammar so that it fits the adult reply. Parents present their children with a large number of structurally similar responses, just under a third of all parental replies (Valian, in press). Those structurally similar responses include additions, substitutions, reductions, and changes of sentence type, *as well as* corrections. There is no simple way that a child can distinguish a correction from, for example, an addition (Grimshaw, 1986; Valian, 1986, in press).

Say the child produces a determiner-noun sequence and the adult repeats it, but inserts an adjective. The child must *not* conclude that the parent's addition of an adjective is a correction and signifies that adjectives are required in noun phrases. But that situation is formally identical to one in which the child produces a subject NP followed by an object NP, and the adult repeats the child's utterance, but inserts a copula. In that case, the child *should* conclude that verbs are necessary in sentences.

The insertion of an adjective is just an addition; the insertion of a copula is a correction. But the child cannot distinguish the two cases unless he already knows or suspects that adjectives are optional in noun phrases and already knows or suspects that verbs are mandatory in sentences. Yet if he already knows or suspects that, then the so-called corrective input is not informing the child that his utterance was ungrammatical. Rather, it is supplying the child who already hypothesizes that an utterance is ungrammatical with the correct means of producing it. Therefore, the didactic role of this type of corrective parental feedback cannot be to inform the child that an utterance is ungrammatical.

A naturalistic study has attempted to demonstrate that children benefit from parents' implicitly corrective responses. Farrar (1992) looked at parental responses to specific morphemes in the children's speech. The responses could be (a) a corrective recast, in which the parent corrected a specific error in the child's previous utterance, (b) a noncorrective recast, in which the parent recast what the child said but did not correct it, (c) a topic continuation that modeled a target morpheme but was not a correction, or (d) a topic change that modeled a target morpheme. The first type of response could occur only to ungrammatical child strings, but the final three types could occur to ungrammatical or grammatical strings. Each type of parental response is somewhat more distant from the original child utterance than the preceding.

Three types of children's responses could follow an adult response. First, the child could imitate the adult's response, thereby indicating a benefit of the adult's response. Second, the child could repeat her original utterance, thereby indicating a lack of benefit of the adult's response. Third, the child could make a response of some other sort, again indicating a lack of benefit. Each response type was analyzed separately.

Farrar (1992) reported that children's imitations of the adult are most likely to follow a corrective recast, next to follow a noncorrective recast, next to follow a topic continuation. Children are very unlikely to imitate the adult if the adult makes a topic change. Farrar interpreted that result as showing that children attend to and probably benefit from parents' implicit corrections.

What Farrar's (1992) data also show, however, is that the same pattern holds to the same degree for children's repetitions of their own utterance. The child is most likely to repeat its own—incorrect—utterance following a parental corrective recast. (Complementary results are found for the category of children's other responses: They are most likely to follow a parental change of topic.)

The result of interest looks like an artifact. Children are most likely to repeat either their own utterance or their parent's reply if the parent's response is maximally *similar* to the child's original utterance. They are most likely to produce a different response if the parent's response is maximally *different* from the child's original utterance. It is as if, when the parent provides a corrective recast, the child is saying, "Okay, my parent seems stuck for some reason; let me help her by repeating what just happened." The child randomly chooses either the parent's response or her own original as the form to repeat. When, instead, the parent provides a topic change, it is as if the child says, "Ah, that's interesting, let's follow up on that." The data provide no evidence that the child changes her speech to copy the adult model.

Scherer and Olswang (1984) reported that children are more likely to imitate adult expansions than other adult replies. That result is consistent with Farrar's (1992), and suggests that there is something about a highly similar adult response that causes children to stay focused at that particular point. There is, however, no evidence that such a focus benefits children's language learning.

Morgan, Bonamo, and Travis (1995) performed an in-depth analysis of articles (such as *the* and *a*) in child-parent interchanges for three children. They examined the children's increase in the use of articles and the connection between that increase and parental replies that implicitly corrected the child's utterance by including an article when the child had failed to use one. They found no relation or a *negative* relation between how often parents replied with an article and the children's rate of improvement. They similarly found no relation between children's acquisition of *wh*-questions and parental implicit corrections. Children's short- and long-term development appeared unrelated to rate of corrections (but see Bohannon, Padgett, Nelson, & Mark, 1996, for a challenge to that conclusion).

As with input studies, work in reply studies was motivated by the question of whether the input could guide the child's acquisition by providing implicit negative evidence. The results, I have suggested, are again largely null. It appears that neither explicit nor implicit negative evidence exists, however otherwise informative parental replies may be. The data argue against a copy metaphor and are largely irrelevant to hypothesis-testing and parameter-setting theories of acquisition. Despite the null results so far, we cannot conclude the parental replies pro-

vide the child with no useful information. Instead, as with input, we should conclude that the particular child and adult variables we have examined are not the right ones and that the questions have not been framed in the right way.

V. INTERVENTION STUDIES

A logical way of examining effects of inputs is to manipulate the child's input and see whether the child benefits from the manipulation. (Researchers try to make sure that effects will not be harmful, but either neutral or beneficial.) The omnipresence of expansions in parental speech was noticed early on in the study of language acquisition. Cazden (1965) was the first to expose children to a concentrated dose of expansions in order to determine whether children's acquisition would be accelerated. She, and later, Feldman (1971), found no benefits from adult expansions of child utterances.

Cazden (1988) more recently noted that her study contrasted expansions with extensions and thus, in effect, used the wrong control. One group of children received expansions of their utterances, and the other received topic extensions. Expansions had been predicted to accelerate children's language development. But because both forms of reply were semantically related to the child's prior utterance, they may both have been effective to the same extent, and thus no difference would be observed between the two groups. That, however, would only support a diffuse motivational impact of parental input. The children were interested in parsing any reply which was directly related to their utterance, and both expansions and extensions fit that definition. (Because Cazden, 1965, did not target specific constructions to recast, and did not measure development of specific constructions, the dependent variables may also have been insensitive measures.)

Nelson (1977) obtained positive results in accelerating children's production of specific new syntactic structures, by using recasts of child utterances and by modeling new forms. Baker and Nelson (1984), using a small sample and a lengthy training period (and no control group), tried to distinguish the effects of simple modeling and recasting; they found both to be effective, with recasting more so. If the results are taken at face value, they support the position that the child can learn auto-didactically, but will benefit slightly more from feedback that is directly related to her own utterance. Again, this supports the diffuse interpretation of the didactic role of parental feedback. Children will attempt to parse a certain amount of ambient input, and thus can learn language without any special feedback. If parental replies are directly related to the child's utterance, that increases the likelihood that the child will attempt to parse the input and therefore increase the speed of learning.

Just how ineffective high ambient frequency of a form may be is apparent from a study by Shatz, Hoff-Ginsberg, and MacIver (1989). They modeled the modal *could* to 2-year olds who were producing few if any modals. The children heard 60 sentences using *could* in each of six play sessions spaced a week apart. Over a six-week period, then, the children heard 360 *could*s. The children who heard *could* did not produce more modals or more auxiliaries in post-intervention sessions than did children who heard no *could*s at all. Even the production of *could* itself appeared unaffected. (The experiment includes subgroups among whom there were differences, but no subgroup differed from the control group.) The problem here may have been the use of a single example (*could*), rather than multiple examples of a particular structure. However, at the least, the study shows that under some circumstances children are impervious to input.

On the other hand, some studies demonstrate that input can be strikingly effective. Roth (1984) successfully taught relative clauses to children aged 3;6 to 4;6, using only 24 sentences, 8 presented at each of three training sessions. She required children to listen to a sentence, listen again and watch while the experimenter used toys to act out the events described in the sentence, and then listen again and act out the sentence themselves. Children's comprehension of relatives increased from 16% to over 50%. Children in a control condition, who received the same type of training, but on coordinate structures, showed no increase in comprehension of relatives. The features of the experiment—having the child watch the experimenter act out the sentence and having the child herself then act out the sentence—undoubtedly increased the likelihood of the child's attempting to parse the input.

de Villiers (1984) successfully used an elicited imitation task to accelerate comprehension and production of passives. She trained 3-year olds who failed a comprehension test on passives by having the children imitate passives that described pictures they were shown. The children heard and imitated 20 sentences. They were also asked to describe other pictures, with no instructions given as to the form of description. A few days later they imitated the original 20 sentences again and described a second new set of pictures. Finally, a few days later, the children were given the initial comprehension test. The children passed the comprehension test they had previously failed and also spontaneously produced passives in describing the pictures during the training sessions. The control group failed the comprehension test both times and did not spontaneously produce passives. Again, elicited imitation probably increases the likelihood that children will actively try to process what they are hearing. The fact that the children spontaneously produced passives further suggests that they were in fact actively trying to assign a structure to the passives they were hearing.

In sum, in some cases massive exposure to a form has been ineffective (Shatz et al., 1989), in other cases effective (Baker & Nelson, 1984); in yet other cases, minimal exposure has been effective (de Villiers, 1984; Roth, 1984). One gener-

alization that appears to cover all the studies is that situations that encourage the child to filter the input through her grammar will facilitate language development more than situations that do not.

Although controls in intervention studies have also been problematic, there is more evidence here in favor of distinct effects of input than in input studies. The facts that successful studies target a particular structure and concentrate the input relative to that target may both be important. The fact that a child *can* use specially provided input is, however, not the same as demonstrating that the child needs such input to acquire language in a timely way (Marcus, 1993) or that the child operates in the same way on the more diluted input she receives in everyday life.

Thus, although the results of laboratory intervention studies are promising and suggestive, it is not clear how they work when they do work, nor why they do not work at other times.

Interestingly, cross-cultural reports (see a brief review in Cazden, 1988) indicate that parental speech to children, although very different in some ways from culture to culture, uses structurally similar replies, elicited imitation sequences, or both. All cultures studied thus far do something that encourages children to parse their input. The encouragement is not intentional, but is a byproduct of other parent practices. (Gordon, 1990, suggested that there are cultures in which adults do not interact conversationally with children, let alone provide tacit encouragement for children to filter the speech they hear through their grammar, but as Bohannon et al., 1990, noted, Gordon only appealed to a single quote by a single parent.)

Middle- and upper-middle-class white parents in technologically advanced societies use expansion-like replies very frequently. Although we do not know how common such replies are across different cultures, it would be as ethnocentrically presumptuous and premature to suppose that only economically secure, technologically advanced peoples respond to children with expansion-like replies as it would be to suppose that such replies are universal.

Recent cross-cultural studies show both that expansion-like replies are not confined to white middle-class Westerners and that a variety of conversational styles with children exist. Watson-Gegeo and Gegeo (1986), for example, have described the parental speech of the Kwara'ae, a "Melanesian people of Malaita in the Solomon Islands, speaking an Austronesian language" (p. 17). In the three villages Watson-Gegeo and Gegeo studied, the "populations are very poor and . . . support themselves primarily through subsistence gardening" (p. 18). Although they present no figures, Watson-Gegeo and Gegeo stated that "between age 9 months and about 2½ years, heavy use of the caregiver speech register and repetitions of infant utterances and of the caregiver's own utterances characterize caregiver-infant interactions" (p. 19). In short, Kwara'ae parents provide repetitions and expansions.

Elicited imitation is also cross-culturally common. Schieffelin & Ochs (1983) reported that Kaluli mothers (the Kaluli are a people in Papua New Guinea) sit alongside their children and interact with others in a group on behalf of the children. The mother will produce a sentence and then say to the child, *say like that.* The Kaluli appear to train features of language via a natural form of elicited imitation. The Kwara'ae also make extensive use of elicited imitation (Watson-Gegeo & Gegeo, 1986), as do the Basotho (Demuth, 1986).

Middle-class white parents in our culture use a limited form of elicited imitation, primarily as a way of introducing lexical items (*That's a bicycle. Say "bicycle."*). The percentage of elicited imitation attempts in adult speech toward children has not been reported, and thus its role in parental speech in general is difficult to assess. Because observers have been struck by both the frequency and the length of elicited imitation routines, however, we may assume that elicited imitation is more common in cultures other than middle-class white Western ones.

Elicited imitation is as close as one can come to direct linguistic tuition and simultaneously maintain an agreeable social interaction. Unfortunately, there are no data as yet on the relation between naturally occurring elicited imitation and syntax development. The de Villiers (1984) experiment, however, does reinforce the idea that elicited imitation is an effective means of facilitating language acquisition.

In sum, under experimental conditions in which children receive massed exposure to particular structures and are encouraged to parse the utterances they hear (e.g., by imitating them or by acting them out), children demonstrate that they can utilize input very effectively and make rapid improvements in their grammars. In the natural situation, there is little evidence available one way or the other. It is noteworthy, however, that many cultures have ways of responding to children that encourage the children to parse the input they receive.

VI. CONCLUSION

From the input studies we know that the variables that have been measured do not correlate reliably with children's syntactic development. Two reasons for the nonfindings are possible. First, the variables that have been measured have had no theoretical motivation. The best way to look at input effects is (a) to target a particular developing structure (such as syntactic subjects or inversion in *wh*-questions) or target a particular category (such as auxiliaries), (b) to have a hypothesis about the necessary and sufficient input for development of that structure or category, and then (c) to examine the relation between the hypothesized relevant input and development of the structure or category. Future studies may take that approach.

Second, it is possible that natural variation in input is too small to affect syntax development. The natural environment seldom offers extremes in linguistic input. When impoverished input or extremely rich input occurs, it is likely to be accom-

panied by other kinds of impoverishment or enrichment. That makes it difficult to separate linguistic effects from more general cognitive and motivational effects. However, the sheer amount of parental speech (Huttenlocher et al., 1991) has correlated with children's vocabulary development, and the amount of particular forms in the environment (e.g., the present and past participle, Gathercole, 1986) has correlated with acquisition of those forms.

From reply studies we have little evidence that parents distinguish between children's well- and ill-formed speech, despite earlier results suggesting such effects. Parental replies may well be syntactically useful to the child, but they cannot be useful in the manner originally envisioned. Children cannot determine, from a single parental reply, what the status of their own utterance is.

From intervention studies we have evidence that input—sometimes very small amounts of input—can be effectively used by the child to acquire new structures. Studies which require the child to imitate the input or act it out show rapid gains.

Intervention studies have the most potential for isolating input effects and providing sensitive and precise information about the role of input. They allow the investigator to provide different types of input in different circumstances. Although such studies are extremely time-consuming and difficult to perform, they can repay their investment by allowing one to test fine-grained models of how input works.

Consider again, for example, the design of the study examining the development of auxiliaries (Shatz et al., 1989). It compared the efficacy of presenting *could* in medial position alone, in first position alone, and half in each position, against a control group which heard no examples of *could.* In principle, such a design could tell us whether the position of the auxiliary in the input matters. Although the study found that no group outperformed the control group, I have suggested that that was for two reasons. First, modeling of more than one auxiliary may be necessary; second, the children were not required to parse the input. A similar study using mixed auxiliaries and requiring the children to repeat the experimenter's sentences might have found different results.

In sum, from the past two decades of research on input, we have learned a lot about where not to look. Because of that research, the next two decades should be more fruitful.

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CHAPTER 16

MODALITY EFFECTS AND MODULARITY IN LANGUAGE ACQUISITION: THE ACQUISITION OF AMERICAN SIGN LANGUAGE

Diane Lillo-Martin

I. INTRODUCTION AND BACKGROUND

There have been two main themes in studies on the acquisition of American Sign Language (ASL) over the past 20 years. One is exemplified in the following quotation:

> The purpose of this paper is to argue for the inherent interest to linguistic theory of the acquisition of sign language by deaf children. (Gee & Goodhart, 1985, p. 291)

The second theme is related to the first, although in some instantiations the two could be considered contradictory. Two relevant quotes follow:

> One might have every reason to believe that such surface differences between signed and spoken languages might influence the course of language acquisition. . . . the change in transmission system (from the ear to the eye, from the vocal apparatus to the hand) might in itself be expected to influence the course of acquisition. (Bellugi & Klima, 1982, p. 3)

> [T]he modality in which the language is conveyed plays a significant role in language learning. (Reilly, McIntire, & Bellugi, 1991, p. 22)

It might be thought that the modality difference between signed and spoken languages makes signed languages *un*interesting to linguistic theory. What responsi-

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