

No Negative Evidence Revisited: Beyond Learnability or Who Has to Prove What to Whom

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Gordon (1990), in his commentary on Bohannon and Stanowicz (1988), argued that (a) the original Gold (1967) learnability proof bears little relevance for innateness of language, (b) the Bohannon and Stanowicz results do not justify abandoning innate restraints on language learning, and (c) there may be cases in which such feedback is unavailable. In this reply, the relevant and irrelevant aspects of both the original Gold proof and more modern attempts at learnability are discussed. Uniqueness, a concept central to all modern formal models, is also adaptable to account for the negative evidence available in the child's input language. Rates of feedback found in Bohannon and Stanowicz are shown to be sufficient to spur learning in many species, including concept formation tasks in humans, and anecdotal counterevidence against the universality of negative evidence is discounted. It is suggested that using innate factors as a "default" explanation is a dangerous and counterproductive scientific endeavor.

For close to 20 years, language development researchers have taken it to be an established truth that parents fail to provide any form of corrective feedback when children make grammatical errors. Learnability theorists (Gold, 1967; Pinker, 1984; Wexler & Culicover, 1980) have used the purported nonavailability of negative feedback as support for nativist accounts of language learning. However, recent reports have found that when adult repetitions are considered as feedback, the assumption that children receive no negative feedback can be called into question. The latest study (Bohannon & Stanowicz, 1988) has reported both easily discriminable adult responding to children's language errors and differential child behavior following these adult responses. Gordon (1990), in his commentary on Bohannon and Stanowicz (1988), argued that (a) the original Gold proof bears little relevance for the innateness of language, (b) the Bohannon and Stanowicz results are not strong enough to justify abandoning innate restraints, and (c) there may be cases in which such feedback is unavailable. Rather than reviewing the original results of Bohannon and Stanowicz and other studies (Demetras, Post, & Snow, 1986; Hirsh-Pasek, Treiman, & Schneiderman, 1984; Penner, 1987), we address Gordon's basic contentions, in turn.

Reasonableness of the Gold Assumptions

We agree with Gordon (1990) in questioning the assumptions underlying the Gold (1967) paradigm. We have made similar

points ourselves elsewhere (Bohannon & Hirsh-Pasek, 1984; Bohannon & Warren-Leubecker, 1988, 1989; MacWhinney, 1987a). Like Gordon, we have problems accepting the way in which Gold dissociated language learning from the learning of the words of the language. We entirely agree with the way in which Gordon uses the example of the theoretical ease of learning the Boston telephone directory as evidence of the unrealistic learning assumptions underlying the Gold paradigm. Indeed, one of us (MacWhinney, 1987b) has gone as far as to argue that grammar is acquired by learning how to deal with particular lexical items. As Gordon's example indicates, such lexicalist views of language acquisition fall outside the Gold paradigm. We also worry about Gold's characterization of the language learner as a grammar *tester*. It seems clear that the language learner is not comparing candidate grammars to one another as complete alternatives; rather, the learner is using data to consider the correctness of rather specific rules or the organization of minor domains within the total language system. It is true that the Gold proofs apply with roughly equal force whether the learner is seen as a tester or a generator. However, it is not clear that thinking of the 2-year-old as being able to generate all sorts of candidate grammars is realistic in information-processing terms. One critical problem is the assumption that real-world language learning works on the whole grammar simultaneously, rather than on a circumscribed "problem space." Negative evidence may have its effect within this narrow problem space, rather than calling the entire grammar into question (see also Valian, 1986, for an extensive treatment of this problem).

There are other aspects of the Gold (1967) framework that did not appear to worry Gordon (1990), but that worry us considerably. Gold's willingness to wait throughout finite time for a final verdict on the identity of the language strikes us as requiring too much patience. Finite time is remarkably long, and the child learns language in only a few short years. If we cannot

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guarantee learning within the time of childhood, we do not have a language-learning method that is of much interest to empirical science. We also worry about the notion that once a language is identified, the child's guess about it never changes (see Valian, 1986). The Gold-like metaphor of choosing one language or another language as if alternate languages are different colored jelly beans in a bowl strikes us as less useful than a metaphor of molding clay or sculpting marble. At any given instant, only one part of the system is actually being worked on, although in the end a single whole is indeed achieved. In reality, furthermore, language learning may involve getting closer and closer to adult competence, but the actual shape of adult competence is a moving target and no one ever manages to acquire blemish-free command of all aspects of language.

Is Gold Relevant?

Having stated our agreements with Gordon (1990), it is now time for us to consider where we disagree. There are two major areas of disagreement. The first involves Gordon's claim that Gold's (1967) analysis is irrelevant to current discussions of learnability. What Gordon says is that "the issue of whether a class of languages is learnable from text of informant presentation is quite orthogonal to the issue of whether learning those languages requires innate knowledge" (p. 217). It is certainly true that there are interesting empirical ways of establishing innate knowledge that do not rely on set-theoretic analyses. One can study neonates' ability to segment phrasally or examine the similarities of twins reared together and apart, or one can do empirical cross-linguistic work on language acquisition. We would be the last to deny that there are sound empirical ways of establishing nativist claims. What we deny is that these two issues are separable and irrelevant to each other within the framework of learnability theory. To view these issues as orthogonal within learnability theory would be to ignore decades of nativist argumentation (Chomsky, 1972; Pinker, 1984; Wexler & Culicover, 1980) that has used the purported poverty of the stimulus as evidence for a rich view of the inborn capacities of the child's language organ. We would be willing to dismiss this immense literature as having made no substantive contribution to our understanding of the logical problem of language learning, if there were some indication that anyone in the nativist camp besides Gordon were willing to do so as well. However, the fact that learnability theorists regularly cite the problem in their reports, often in the first few paragraphs (e.g., Pinker, LeBeaux, & Frost, 1987), indicates that the issue is very much alive and well. For these reasons, we must take issue with Gordon; negative evidence is still a pivotal point for current theories that rely on innate factors.

An Equation in Three Unknowns

Simplifying quite a bit, we can view both earlier and recent learnability work within the Gold (1967) paradigm as attempting to solve an equation with three unknowns:

$$(1) \quad (2) \quad (3) \\ \text{[the input]} + \text{[the child's innate knowledge]} = \text{[grammar]}$$

If any one of the two components on the left-hand side of the

equation is disabled, the other must be beefed up to compensate, especially if the component on the right side of the equation is assumed to be particularly complex and weighty. The analysis that has predominated in learnability circles is one in which the input is held to be degenerate and the grammar is held to be quite complex. To compensate for these two weaknesses, the theorist then blesses the child with a variety of innate endowments. As Gordon (1990) himself has stated, (learning theorists) "need innate knowledge in their learnability proofs . . . primarily because they can't get them to work without such assumptions" (p. 217). Such assumptions serve as an innatist's fudge factor—a spoonful of innate restraints makes the learnability go down, as it were. Learnability theorists need to believe in complex grammar and a degenerate input to motivate their claims about innate constraints. In this equation with three unknowns, there are two ways to reduce assumptions regarding innate knowledge of grammatical constraints. One way is to reduce the grammar variable by viewing the structure of language as a complex structure composed of simpler building blocks. This is the approach taken in MacWhinney (1987b, 1989). Horning (1969) also takes this approach when he shows that, if one assumes that the rules of grammar are probabilistic, learning can be guaranteed without innate constraints and without negative feedback. Again, this reasonable assumption matches the proposals regarding probabilistic rules found in MacWhinney and Bates (in press).

The other way to deal with this equation in three unknowns is to look more closely at the availability of negative evidence (and, indeed, of other sorts of support for learning in the input). If negative evidence is available, the original Gold proof shows us that language can be learned without further nativist assumptions. Of course, one can do better than the Gold proof. For example, Valiant (1984) has demonstrated learning in relatively little time (as compared with Gold, 1967), given feedback and a probabilistic view of grammar. In either case, the relation between the empirical facts and the motivations for nativist assumptions within this equation in three unknowns is clearly related (that is, non-orthogonal).

Working With Learnability Theory

Although we reject the specific assumptions of Gold's (1967) framework as empirically inaccurate, and although we question the entire learnability enterprise, we are willing to think within that framework to explore the degree to which it is in fact logically and empirically consistent. Having himself rejected Gold, Gordon (1990) nonetheless explicitly demands that we embrace the learnability framework and construct a learnability analysis to support our claim that language is learnable with feedback. This invitation for us to play a game whose rules we reject is perhaps a bit ingenuous. We hope that Gordon is not assuming that nativism assumptions are correct unless proven to be wrong. According to such a view, Gordon would be allowed to make no-cost nativist assumptions wherever they were needed to account for language learning. It would then be our job to prove these assumptions wrong. In the particular case, it would be our task to construct a learnability model that proved that language could be learned without innate assumptions on the basis of cues actually available in the input. One could go on

and demand that we also correct in the model (that we are supposed to provide) all of the faulty and unrealistic assumptions found in the models of Chomsky (1972, 1981), Gold (1967), Morgan (1986), Pinker (1984), Wexler and Culicover (1980), and others. One could continue to reject our model as long as it had any element that failed to match the empirical data. Clearly, it is nonsense to permit nativist assumptions to stand unquestioned in the face of contrary empirical evidence, until empiricists have achieved full explanatory models of language development. Constructing a complete and satisfactory model of how children learn language that makes only valid assumptions about innate capacities or the input is a challenge that has yet to be met by anyone. Our collective failure to meet this challenge does not invalidate the empirical fact that corrective feedback exists and may be another universal feature of language input (Niczniekewicz & Bohannon, 1989).

Uniqueness and Competition

In fact, the elements of a full model that Gordon (1990) requests of us are not really all that difficult to provide. Let us look at a principle accepted by Clark (1987), Grimshaw (1981), Morgan (1986), Pinker (1984), Roeper (1981), Wexler and Culicover (1980), and many others. It is an assumption that Wexler and Culicover (1980) called the *uniqueness principle*. This principle holds that, at most, one surface structure could be derived from any given deep structure. For Pinker (1984), the uniqueness principle allows for only one way of expressing a given function. For Clark (1987), the principle goes under the name of the *principle of contrast* and excludes full synonymy. The basic idea underlying these various formulations is that there is only one truly correct way to express a particular intention or function. Given this, when the learner encounters two ways of saying the same thing, it is clear that one of them is wrong. This assumption is not present in the original Gold (1967) analysis, inasmuch as Gold was only looking at the identification of a grammar for uninterpreted strings. However, all recent psycholinguistic learnability analyses depend on this assumption, as do environmentalists' discussions of the role of negative evidence or feedback. It has been pointed out (Snow, 1989) that an adult response should count as negative evidence if and only if (a) it expresses exactly the same meaning as the child's ill-formed utterance, (b) the uniqueness constraint functions as stated, and (c) the child has a rather sophisticated system for understanding adult speech acts. It is the assumption of uniqueness that allows Wexler and Culicover's demonstration of the learnability of transformational grammar without negative instances. Formally, the issue is whether a particular type of mapping is identifiable on positive instances. If the mapping from Set A to Set B is structured so that each element in Set B is constrained to map onto only one element in Set A, then, as Anderson (1978) has shown, learning can be guaranteed on positive data. In more complex frameworks, such as the transformational grammar of Wexler and Culicover (1980) with potentially infinite degrees of embedding, it may be necessary to make further constraints on structure. However, once these further constraints, such as the binary principle, are made, learnability on text presentation still requires the uniqueness principle.

One could argue that, by accepting the uniqueness principle, we have accepted the nativist view of inborn constraints. However, this particular constraint arises not from some arbitrary inborn restriction, but from the most basic aspects of the human information-processing system. MacWhinney (1987a, 1989, in press) has shown that the uniqueness principle is nothing other than the fundamental principle of mental competition. To illustrate this, consider the case of a child who is producing the forms *broke* and *breaked*. The two forms both map onto a single semantic function, thereby violating uniqueness. The crucial point is that if one looks at this in process terms, whenever the learner receives evidence in support of *broke*, the strength of that form is incremented. To the degree that the competition between the forms is a zero-sum game, the gains of one form are the losses of the other. Within the framework of the choice rule of Luce (1959) and McDonald and MacWhinney (in press), an incrementation of the strength of *broke* is equivalent to a decrementation of the strength of *breaked*. The reverse is also true: Support for *breaked* leads to a loss of strength for *broke*. Of course, what happens in development is that *broke* receives massive support and *breaked* receives no support at all. The force of the regular pattern initially provides enough support for *breaked* to allow it to compete. However, the continued exposure to *broke* in comprehension or feedback eventually leads to such a strong overlearning of *broke* that production of *breaked* drops to near zero.

An alternative for thinking about the role of positive feedback within the constraints of uniqueness is in terms of the competition principle of the competition model. The crucial point is that when two forms are locked in mortal combat for mapping onto a single function, each piece of positive evidence for Form A is equivalent to a piece of negative evidence for Form B. Given this, we might well ask why parents would provide negative evidence at all. There are two answers to this question. The first is that the type of negative evidence discovered by Bohannon and Stanowicz (1988) could be thought of as positive evidence, given the uniqueness assumption. If the parent imitates a correct utterance, that is a dose of positive evidence. If the parent expands an incorrect utterance, the expansion can be seen as reinforcing a correct alternative that then, by the principle of competition, weakens the incorrect alternative. The second reason that parents provide feedback that is overtly corrective is that, in real life, the uniqueness principle is only approximately correct. Often the parent is not entirely sure what the child has in mind. The parent cannot reinforce the correct alternative when it is not clear what the correct alternative should be. Rather than providing a possibly misleading alternative, the parent may merely provide feedback indicating that the option the child selected was wrong. Because negative feedback has the function of weakening the incorrect alternative, this form of feedback is the most accurate in cases in which the child's meaning is unclear, which is exactly what Bohannon and Stanowicz (1988) found when children made more than one error in an utterance. There may also be cases in which the parent believes that the child has developed a deviant internal mapping in which, for example, *broke* and *breaked* are assigned different meanings. Perhaps the child thinks that *broke* has a completive meaning and that *breaked* is noncompletive. In this case, strengthening of *broke* would have little effect on *breaked*, inasmuch as the

two forms are not locked in mortal combat. In these circumstances, it would be better to simply provide overt negative feedback indicating that *breaked* is wrong.

This analysis of the relation between positive and negative feedback goes beyond the assumptions of the Gold (1967) paradigm. However, it still operates within the framework of an equation with three unknowns. By allowing for a greater role for the input, we can live with weaker assumptions about innate knowledge. The greater role we are allowing for the input is that both positive and negative feedback are used in determining the winners in the competitions between forms.

Rates of Feedback and Language Learning

We can now apply this analysis to the question of whether the forms and rates of feedback reported by Bohannon and Stanowicz (1988) are adequate to power language learning. Gordon (1990) is convinced that if and only if 100% of all ungrammatical utterances are followed by recasted feedback, would a child solve the language puzzle. Bohannon and Stanowicz found that only about one third of all grammatical errors were followed by recasted repetitions on the part of the adults. Actually, we have known for years that not all presentations of to-be-processed material need be followed by feedback. Levine (1959, 1963) has shown that concepts may be learned and hypotheses accurately confirmed with less than 25% of the trials using feedback. The competition between hypotheses in concept formation is much like the competition between forms in language learning. What is important is that, over time, there be a clear preponderance of evidence supporting the correct candidate. Learning does not require that every trial should affect the balance.

Gordon (1990) is also worried about the fact that adult recasts of children's errors were only 70% accurate in the Bohannon and Stanowicz (1988) study. He noted that the 30% of the recasts that followed children's errorless utterances could make children discard "good" hypotheses about their native tongues. There are two problems with this reasoning. First, it fails to note that this feedback may be cuing the child to "an alternate way" to say it. Conceivably, children may use sentences that are well-formed grammatically but relatively primitive in their semantic content, grammatical form, or pragmatic force. Such recasts may assist children by introducing newer, more complex, constructions (see Nelson, Denninger, Bonvillian, Kaplan, & Baker, 1984). Second, this concern seems to ignore the results from studies on probability learning. The basic question is whether an organism can settle on a response if only about a quarter (70% of 35%) of targeted responses receive corrective feedback. The answer is *yes* for a range of tasks and species, including humans (Estes, 1959; and many others).

Gordon (1990) also seems to ignore another piece of data reported by Bohannon and Stanowicz (1988) concerning the number of errors present in children's utterances. Bohannon and Stanowicz reported that when a child's utterance contained more than a single error, the incidence of specific evidence was about one half of the rate of feedback when there was but a single error. Thus, bizarre examples such as Gordon's "Who did John overhear the statement that Mary kicked" would probably pass by without specific recasts. In contrast, the adult

may respond with a contingent query such as, "What?" or "Huh?" as did the 10 speech professors who heard that sentence in our impromptu study.

Conclusions: Sufficiency or Necessity?

Bohannon and Stanowicz (1988) found recasts to have the power to change children's speech within conversations; children were 8 times more likely to imitate an adult's recasted correction than an adult's exact imitation of a well-formed utterance. The work of Nelson and his colleagues (Nelson et al., 1984) has further shown recasts to have longer and more specific developmental effects on language. Therefore, the sufficiency of recasts to power developmental advances is not really in question, even by Gordon (1990). What remains to be determined is if they are necessary for all emerging speakers. Our analysis of the equifunctionality of positive and negative evidence given the uniqueness or competition assumption makes this question somewhat moot from the point of view of the current rigid formal analyses. However, within a more realistic model of competition and processing, the question would still be a good one.

Gordon (1990) offered two cases as a basis for arguing that negative evidence is not universal: the case of children exposed to pidgin languages who create creoles, and an anecdote from a rural, Black community member who stated that repeating children's utterances is senseless (Heath, 1983). The second of these cases hardly constitutes believable counterevidence. The Heath quotation could be relevant if and only if (a) the woman's attitude is reflective of the entire community of speakers, (b) actual speech to children in the community parallels the expressed attitude, (c) children in this community only speak with adults who have this attitude, and (d) repetitions are the only source of negative evidence. If any of these are false, the point is rendered specious. It is, of course, relatively easy to cite isolated opinions from almost any speech community that would seem to contradict more general principles of adult-child interaction. However, the use of such anecdotes in isolation of a clear understanding of the more general principles of social interaction is dangerous and misleading. The Black, rural community speakers may indeed frown on repeating children's utterances, but they may insist on children being clear and comprehensible in other ways. Presumably, they may also provide negative evidence for the children's ill-formed speech in the process. The robustness of the language acquisition process suggests that the system is redundantly buffered environmentally as well as constitutionally. Thus, the absence of a particular form of feedback in a particular community does not belie its utility to those children who do receive it, nor does it mean that no form of feedback is necessary for language learning to proceed normally (see Bohannon & Warren-Leubecker, 1989; Snow, 1984, 1985, 1989; Snow & Gilbreath, 1983; Snow, Perlmann, & Nathan, 1987).

The case of children producing creoles on the basis of input from pidgin-speaking parents is a fascinating one, about which we have too little data to speculate extensively. The romantic innatist view of this process assumes no sociolinguistic variability in the use of the pidgin. The impoverished pidgin form is assumed to be transformed into a fully developed, natural language by virtue of being processed through the child's brain

during acquisition. This perspective, although appealingly simple, is unlikely to fit the facts. First, the most impoverished pidgin forms are unlikely to serve as an adequate communication system for entire households or communities. It is also recognized among creolists that pidgins develop in response to increased communicative demands as well as in the process of acquisition (e.g., Woolford, 1983). Children learning from pidgin-speaking parents are likely to be living in communities in which many versions along the pidgin-creole dimension are spoken. Second, data about the form of the pidgin actually presented to creole-learning children, or the real nature of parent-child interaction in pidgin, are unavailable. Without data on the actual varieties of speech to which creole-learning children are exposed or the forms of the interactions, the case of pidgin input cannot be taken as casting serious doubt on the possible universality of negative evidence.

Better evidence for Gordon's (1990) position would come from first-born hearing children of deaf parents who are exposed to language only through the medium of television, from children who have received tracheotomies and purportedly acquire language without the ability to speak (Lenneberg, 1967), or from children who are exposed to a home language they themselves do not use productively. In these cases, children are presented with language data aplenty, but cannot have their errors corrected. However, evidence for extensive language acquisition in these cases is quite tenuous. Studies by Sachs and Johnson (1972) of hearing children who interact exclusively with their deaf parents (but watch a lot of TV) show considerable language delay and deviance. In all cases studied, convergence on a normal language system does not occur until the child is provided with a different linguistic environment. Cases of tracheotomies lasting throughout the entire period of language acquisition are unknown. Children in Europe who watch considerable amounts of television in other languages are not reported to acquire much if any productive skill from this experience (Snow, 1977). There is, to our knowledge, no data about the productive abilities or grammatical systems of children who have acquired some comprehension skill in a language that they merely overhear. Our guess is that the result would be vanishingly limited.

Gordon (1990) has stated that the burden is on us to prove that language is learnable given the observed rates of feedback. We have shown that there is an important reciprocal relation between positive and negative evidence both within the current learnability paradigm and within more empirically based accounts such as the competition model. Given the extensive evidence for informative feedback reported by Bohannon and Stanowicz (1988) for both parents and nonparents, and given that children were 8 times more likely to imitate a recast of their errors than an exact adult repetition of their errorless speech, it is clear that the current dogmatic rejection of the role of corrective feedback should itself be rejected. Gordon correctly reminds us that language development proceeds under widely varying conditions. But we must also recognize that learning proceeds under widely varying rates. Is it possible that the presence of corrective feedback in the input could affect the rate of acquisition? The next step is to directly assess the contribution of corrective feedback to language development, comparing the power of more frequent and reliable feedback as predictors of

language gains in children. In any event, Gordon's arbitrary and dogmatic dismissal of the empirical evidence against the existence and relevance of negative evidence is fraught with logical and scientific peril. We conclude: Yes Virginia, parents do correct their children's language mistakes and it is probably important that they do so.

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