

Three Myths from the Language Acquisition Literature

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Three popular assertions have hindered the promotion of an empiricist approach to language acquisition: (a) that Brown and Hanlon (1970) claimed to offer data that parents do not reinforce their children's grammaticality; (b) that Brown and Hanlon also claimed to offer data that parents do not provide negative evidence (i.e., corrective feedback) for ungrammaticality; and (c) that Gold (1967) claimed to offer a formal proof showing that, without negative evidence, a child cannot acquire a language solely from environmental input. In this paper I offer introductory comments on the nature–nurture distinction (including interactionism, and the nativists' claim to have found a gene for language). Next I debunk the three aforementioned assertions by arguing that the authors (Brown & Hanlon; Gold) never made the claims attributed to them; review evidence on the role of reinforcement and corrective feedback in language acquisition; and offer some concluding comments.

Key words: language acquisition, Brown and Hanlon (1970), positive evidence, negative evidence, Gold's theorem

According to West and King (1987), the terms *nature* and *nurture* were first used by a teacher, Richard Mulcaster, in 1582 to identify factors that influence child development. Plomin (1994; cited in Moore, 2001) noted that the first use of these terms by a scientist was Francis Galton's usage centuries later. For Galton (1883/1907), "the bulk of the respective provinces of nature and nurture are totally different, ... and we are perfectly justified in attempting to appraise their relative importance" (p. 131). Further, Galton saw nature's role as preeminent. "When nature and nurture compete for supremacy on equal terms ... the former proves stronger (Galton, 1875, p. 9). In the ensuing decades, the partitioning of nature and nurture grew in popularity, as did the assignment of a dominant role to nature. For example, Goddard (1920) asserted that "the chief determiner of human conduct is ... intelligence ... which is inborn" (p. 1). Similar assertions were offered by others (e.g., Terman, 1922; Wiggam, 1923).

More recently, the partitioning of nature and nurture has been eschewed and, in its place, *interactionism* has become the doctrine professed by many developmentalists (e.g., Anastasi, 1958; Johnston, 1987, 1988, 2002; Moore, 2001) within the life sciences. According to Anastasi, efforts to determine

the relative contributions of nature and nurture have not been successful. Instead of assuming that "hereditary and environmental factors combine in an additive fashion, ... a more tenable hypothesis is that of interaction" (p. 197). Rooted in the earlier work of others (e.g., Carmichael, 1925; Kuo, 1921; Lehrman, 1953; Schneirla, 1956), contemporary interactionism maintains that, although both hereditary and environmental factors jointly determine an organism's traits, "the extent of their respective contributions cannot be specified for any trait" (Anastasi, p. 197). Rather than asking how much is attributable to heredity, and how much to environment, "a more fruitful approach is to be found in the question 'How?'" (p. 197).

To avoid misunderstanding, critical attention must be paid to what one means by *interactionism*. Differing, often competing, definitions of this term have been offered (for discussion, see Johnston, 1987; Moore, 2001; Oyama, 1985, 2001), but one widely held view is that interactionism connotes at least two principal claims. First is the claim that "all traits reflect the necessary contributions of both genetic and environmental factors" (Moore, p. 10). As Oyama, Griffiths, and Gray (2001) put it, "every trait is produced by the interaction of many developmental resources" (p. 2). For example, according to Moore, the commonly held view that blue eyes are "caused strictly by recessive genes" (p. 12) is wrong. Eye color is no more determined exclusively by genes than is

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any other trait (for arguments and evidence that all traits are determined by both genetic and nongenetic factors, see Gottlieb, 1998). Second, one cannot assess the relative contributions of heredity and environment, or any other dichotomy (e.g., innate vs. acquired, biology vs. culture) offered to partition the causes of development. For example, not only is eye color not determined exclusively by the genes, it is not even primarily determined by them (Moore).

Perspectives on development consistent with this definition of *interactionism* include developmental systems theory (e.g., Griffiths & Gray, 1994; Moore, 2001; Oyama, 1985), the dynamical systems approach (e.g., Thelen & Smith, 1994; Thelen & Ulrich, 1991), probabilistic epigenesis (e.g., Gottlieb, 1970, 1976, 1997, 1998), developmental connectionism (e.g., Elman et al., 1996), emergentist theory (e.g., Bates et al., 1998; Karmiloff-Smith, 2005), and the behavioral systems approach (Novak & Pelaez, 2004).

Despite the increasing influence of these interactionist perspectives, the nature–nurture distinction remains popular in many quarters, most notably among many prominent, nativist approaches to language acquisition. Indeed, the two basic, competing approaches to explaining language acquisition (nativism and empiricism) are still often couched in terms of nature and nurture, respectively. Bruner (1983) has credited George Miller with labeling nativism the *miraculous* theory and empiricism the *impossible* theory. On this view, nativism appears to assert that “language ‘just happened’ in human children without formal training—seemingly a miracle” (Novak, 1996, pp. 163–164). On the other hand, empiricism appears to assert that, by employing the same general learning strategies (e.g., induction) used to learn any task, children can learn the complexities of language in a scant few years, which is seemingly impossible. For nativist psycholinguists, what seems miraculous appears less so when one posits the possession, by children, of specific innate knowledge of the general structure of language. In short, nativists have accorded a greater role to nature than to nurture in language acquisition.

Contemporary interactionist views notwithstanding, this nativist tradition continues. For example, Pinker has argued that “there is suggestive evidence of grammar genes ... whose effects seem most specific to the development of the circuits underlying parts of grammar” (1994, p. 325), and “brain circuitry, mostly in the left hemisphere, ... appears to be designed for language” (1995, p. 139). Similarly, according to Calvin and Bickerton (2000/2001), language is “innate, species-specific, and supported by task-dedicated circuits” (p. 204), and “the probability is that a number of genes indirectly conspire to yield language” (p. 210). Finally, despite numerous changes in his linguistic theory over the last half century, Chomsky (2005) recently reconfirmed his continuing commitment to nativism by asserting that “genetic endowment ... determines the general course of the development of the language faculty” (p. 6).

Ingold (2001) has observed that, generally speaking, “in the literature of cognitive psychology, the postulation of innate structures is taken to require no more justification than vague references to genetics and natural selection” (p. 270). This observation also appears to apply generally to the nativist language acquisition literature. However, one prominent exception has been the alleged discovery of what, in the press, has been called a “gene for language” (Cohen, 1998, p. 77). In this case, instead of “vague references to genetics and natural selection” (Ingold, p. 270), specific, gene-based evidence is being offered. What is the evidence? Hurst, Baraitser, Auger, Graham, and Norell (1990) reported that some members of a large, extended London family (the KE family) were afflicted with an inherited speech disorder (developmental verbal dyspraxia). Further, the pattern of genetic transmission was apparently autosomal dominance, and therefore was Mendelian in nature. Also, the afflicted individuals reportedly exhibited normal intelligence and hearing. Based on her study of the KE family’s language skills, Gopnik (1990) judged that the impairment was chiefly a deficit in regular inflectional morphology; specifically, difficulty mastering regular past tense verbs (e.g., *miss*, *missed*) and plural endings (e.g., *hat*, *hats*).

As a result of studying the KE family, as well as of others suffering from dysphasia, Gopnik and Crago (1991) entertained an "interim hypothesis" (p. 47), namely, that "a single dominant gene controls for those mechanisms that result in a child's ability to construct the paradigms that constitute morphology" (p. 47). Sounding a cautionary note, Pinker (1994) observed that "the mere fact that some behavioral patterns run in families does not show it is genetic. Recipes, accents, and lullabies run in families, but they have nothing to do with DNA" (pp. 48–49). However, Pinker then provided his own counterargument by asking, rhetorically, "if the cause were in the environment ... then why would the syndrome capriciously strike some family members while leaving their near age-mates (in one case, a fraternal twin) alone?" Thus, Pinker concluded that "a genetic cause is plausible" (p. 49). In short, Pinker entertained the plausibility of a gene for language and, more narrowly, a gene for grammar. In the ensuing years, the locus of the gene was found and designated SPCH1 (Fisher, Vargha-Khadem, Watkins, Monaco, & Pembrey, 1998). The gene itself was dubbed FOXP2 (Lai, Fisher, Hurst, Vargha-Khadem, & Monaco, 2001).

After the initial studies of the KE family (e.g., Gopnik, 1990; Gopnik & Crago, 1991; Hurst et al., 1990), subsequent research substantially weakened the plausibility that FOXP2 served as a language gene, let alone a grammar gene. For example, after conducting their own investigation of the KE family, Vargha-Khadem, Watkins, Alcock, Fletcher, and Passingham (1995) concluded that "the evidence from the KE family ... provides no support for the proposed existence of grammar-specific genes" (p. 930). Elaborating, Vargha-Khadem et al. stated,

Our ... investigations of the same (KE) family indicate that the affected members' disorder transcends the generation of morphosyntactic rules to include impaired processing and expression of other areas of grammar, grossly defective articulation of speech sounds, and, further, a severe extralinguistic orofacial dyspraxia. In addition, the affected family members have both verbal and performance intelligence quotient (IQ) scores that are on average 18–19 points below those of the unaffected members.

This psychological profile indicates that the inherited disorder does not affect morphosyntax exclusively, or even primarily; rather, it affects intellectual, linguistic, and orofacial praxic functions generally. (p. 930)

Others (e.g., Elman et al., 1996; Karmiloff-Smith, 2005; Lieberman, 2006) offered similar criticisms. Even some nativists have joined the chorus. For example, Fitch, Hauser, and Chomsky (2005) noted that the effects of the FOXP2 gene are "pleiotropic, including pronounced effects on oro-motor praxis that are independent of its effects on speech articulation" (p. 190). Thus, Fitch et al. concluded that "the consequences of its expression are not specific to speech or language" (p. 190). Lastly, Pinker, who had earlier surmised that "a genetic cause is plausible" (1994, p. 49), subsequently entertained a more modest claim: that the gene was somehow "*implicated* [*italics added*] in speech and language" (2001, p. 466).

The failure of nativists to prove the existence of a language-specific gene has frustrated their attempts to defeat empiricist accounts of language acquisition. However, the proponents of nativism have wielded other weapons in their arsenal. Specifically, they have tried to establish nativism by disproving empiricism. To that end, Cowie (1999) has observed that "one central means" (p. 31) employed by nativists in this endeavor has been their use of a particular negative argument, an enduring classic: the poverty-of-the-stimulus argument (hereafter, POSA).

According to POSA, for many knowledge domains (e.g., facial recognition; see Fodor, 1983) there is a dearth of information available in the environment; therefore, much of what a child knows about these domains must be innately present in the mind. Applying this argument to language acquisition, POSA maintains that the input provided by the linguistic environment is too meager to explain the language learner's knowledge of language. Indeed, POSA has been called "a cornerstone of strong nativist claims about language" (Elman et al., 1996, p. 385). For example, POSA has been used by Chomsky (e.g., 1965, 1980) and others (e.g., Pinker, 1994) to support the innateness hypothesis. Chomsky (1965, p. 58) argued

that the “degenerate quality and narrowly limited extent of the available data” provide a major reason for asserting that the learner necessarily possesses innate knowledge about the “general character” of grammar, an argument that he later acknowledged was “a variant of a classical argument in the theory of knowledge, what we might call ‘the argument from poverty of the stimulus’” (Chomsky, 1980, p. 34).

Of course, as previously noted, the nativist grants that learning also plays a crucial role. A child must learn what is not innately given: the specific lexicon and grammar of his or her target language. However, even here, distinct innate factors come into play. According to the nativist, in learning a language, the learner employs innate, *domain-specific* language learning mechanisms, for example, Chomsky’s (1965) “language-acquisition device” (p. 47) or Pinker’s (1994) “language instinct” (p. 83). As Marchman and Thal (2005) have put it, “a minimal role for input is core to the traditional view that the acquisition of grammar depends on innate, domain-specific mental structures” (p. 142). These mental structures are domain specific in the sense that they are “uniquely suited to and configured for” the acquisition of language and not some other learning task (Elman et al., 1996, p. 36).

For example, consider Chomsky’s (1980) nativist account of a child’s mastery of the expression *each other*. According to Chomsky, children learning English acquire a rule that indicates which sentences employing *each other* are well formed and which are not. Thus, after mastering this rule, a child knows that *the candidates wanted each other to win* is a well-formed sentence, whereas *the candidates wanted me to vote for each other* is not. According to Chomsky, “it can hardly be maintained that children learning English receive specific instruction about these matters or even that they are provided with relevant experience that informs them that they should not make the obvious inductive generalization” (p. 43). Instead, “this is information that children themselves bring to the process of language acquisition, as part of their mode of cognition” (p. 44). In other words, all children possess innate, domain-specific knowledge

that allows them to acquire rules specific to their native language, rules that are triggered, but not shaped, by their linguistic environment.

On the other hand, empiricists maintain that language learners are exposed to linguistic data that, in combination with domain-general multipurpose learning mechanisms, are sufficient to explain the achievement of linguistic competence. For example, according to Sundberg (2007), “Skinner (1957) proposed that language is learned behavior, and that it is acquired, extended, and maintained by the same type of environmental variables and principles that control nonlanguage behavior (e.g., stimulus control, motivating operations, reinforcement, extinction)” (p. 528). Because these “environmental variables and principles” control both language and nonlanguage behavior, they constitute domain-general learning mechanisms. In addition to Skinner, others (e.g., Bloom, 1991; Elman et al., 1996; Putnam, 1983) have offered accounts of language acquisition that employ domain-general learning mechanisms (although proponents differ on the specific nature of those mechanisms). The nativists do not deny that these mechanisms are adequate to explain some of what we know. However, the nativists argue that, given the meagerness of the input, much of what we end up knowing cannot be accounted for as a product of these general learning mechanisms. Therefore, empiricism must be wrong, leaving nativists to conclude that the mind inherently possesses additional, domain-specific learning faculties.

Contemporary nativist accounts of language acquisition that employ the POSA drew considerable inspiration from two sources. One is Brown and Hanlon’s (1970) study; the other, Gold’s (1967) formal proof. As I will argue, three popular claims are accepted as truisms within the traditional psycholinguistic community (representing the nativists). These claims are (a) that Brown and Hanlon (1970) offered data to demonstrate that parents do not reinforce their children’s grammatical utterances; (b) that Brown and Hanlon also offered data to demonstrate that parents do not provide negative evidence (e.g., corrective feedback) when their children speak ungrammatically;

and (c) that Gold offered a formal proof that, when negative evidence is lacking, a child cannot acquire a language solely from environmental input. All three claims have played a dominant role in arguments against an empiricist account of language acquisition. Specifically, the first and second claims allegedly provide empirical support for POSA, and the third claim allegedly provides a priori support. When taken together, these three claims have signaled the death knell for acceptance of an empiricist account of language acquisition (as epitomized by Skinner, 1957) by traditional linguists and developmental psychologists.

The literature is replete with arguments and evidence debunking these claims. In this paper, I will also attempt to debunk them, but by taking a different path; that is, I will show that these authors never made the claims commonly attributed to them. In short, I will attempt to show that these attributions are myths. Specifically, each myth will be discussed, in turn, along with my arguments and evidence denying authorship of these myths to those to whom they are commonly attributed (Brown & Hanlon, 1970; Gold, 1967). I will also review some of the research evidence and arguments against these myths. Finally, in my concluding comments, I suggest what implications follow for those who are interested in promoting a wider acceptance of an empiricist account of language acquisition.

THREE MYTHS FROM THE LANGUAGE ACQUISITION LITERATURE

Myth 1

Brown and Hanlon (1970) claimed to offer evidence to demonstrate that parents do not reinforce their children's grammatically correct utterances.

Myth 1 examined. In 1962, psycholinguist Roger Brown and colleagues began a longitudinal study of the development of grammatical speech in 2 preschoolers, whom they called Adam and Eve. Later a 3rd child, called Sarah, was included in the study. At the beginning of the study, Adam and Sarah were 27 months old; Eve was 18 months. After 1 year, Eve's family moved from the

area and her participation in the study ended. Adam and Sarah continued to participate for an additional 4 years. The study's data consisted of transcriptions of spontaneous, naturally occurring conversations between the children and their mothers and, on occasion, between the children and their fathers and others. A minimum of 2 hr per month of transcribed conversations were collected for each child (Brown, 1973).

Recognized as a "landmark study" (McLaughlin, 1998, p. 307), Brown's research program subsequently proved to be very influential. Indeed, one of its often cited, alleged findings, specifically credited to Brown and Hanlon (1970), is that the parents (i.e., principally the mothers) in their study reinforced their children's utterances for truthfulness but not grammaticality. Not only has this interpretation of Brown and Hanlon been well established, it has also frequently been recast in terms that suggest that it is externally valid; in other words, Brown and Hanlon's "no reinforcement for grammaticality" findings typify parent-child interactions in general. For example, according to Penner (1987), Brown and Hanlon concluded that parents do not use reinforcement "to favor well-formed over ill-formed child utterances" (p. 376). Similarly, Mook (1989) stated that "reinforcement theory ... says this: Children acquire grammatical sentence structure because parents reinforce grammatically correct speech. ... But Brown and Hanlon found that that simply is not what happens" (p. 27). Many others (e.g., Rutter, 1987; Sigelman & Rider, 2005) demonstrate the widespread adoption of this view. In short, as a consequence of the prevalence of this interpretation of Brown and Hanlon, including especially its supposed external validity, "reinforcement ... is denied or at least disregarded in much of the psycholinguistic literature" (Moerk, 1990, p. 297).

This portrayal of Brown and Hanlon's (1970) findings can also be found in the empiricist literature. Specifically, consider the following statements of two prominent behavior analysts. Novak (1996) reported that "some have suggested that reinforcement is not important in language development (e.g., Brown & Hanlon, 1970)" (p. 177). Similarly, according to Salzinger (1994),

Apparently, Brown's rejection of parental effect is based on a view of conditioning ... in which only obvious events (such as the presentation of candy) are accepted as signifying the occurrence of reinforcement. With such a definition of reinforcers, it is no wonder that Brown could have concluded that reinforcement failed to occur or had no effect. (p. 325)

In what follows, I will argue that this portrayal of Brown and Hanlon, found in both nativist and empiricist camps, is incorrect. Given its widespread distribution and acceptance, this misinterpretation has attained the status of myth. Specifically, I will attempt to debunk this myth by arguing that a careful reading of Brown and Hanlon demonstrates that they never claimed to be offering evidence that parents do not reinforce their children's grammatically correct utterances.

Myth 1 debunked. This misinterpretation of Brown and Hanlon (1970) typically begins with a critical misreading of their definition of *reinforcement*; that is, that they endorse a narrow view of what can count as reinforcement. Then, because Brown and Hanlon reported finding no instances of such narrowly conceived reinforcement being systematically used to reinforce grammaticality, they are mistakenly interpreted as claiming that their findings show that grammaticality was not systematically reinforced in the 3 children in their study. As evidence of this common misreading of Brown and Hanlon, consider Salzinger's (1994) claim that in Brown and Hanlon's study "only obvious events (such as the presentation of candy) are accepted as signifying the occurrence of reinforcement" (p. 325). Similarly, Wright (2006) claimed that Brown and Hanlon counted only "an expression of approval such as *Good job*" (p. 155) as reinforcement, in apparent disregard of "other parental responses" that can reinforce a child's speech.

It is true that, in their study, Brown and Hanlon (1970) examined whether or not explicit verbal approval (e.g., saying "that's right" or "correct") was delivered by parents contingent on "syntactically correct utterances" (p. 45) by children. So, it is easy to see why Salzinger (1994), Wright (2006), and many others have the mistaken view that

Brown and Hanlon narrowly defined reinforcement. However, contrary to Salzinger, Wright, and others, Brown and Hanlon provided definitions of reinforcement that belie this interpretation. Specifically, they state that a *positive reinforcer* is "generally defined as *any event* [italics added] which, being made contingent upon the emission of an antecedent response, increases the frequency of that response" (p. 45). On the other hand, "an event subsequent to a response" qualifies as a *negative reinforcer* "when the *withdrawal* of that event, being made contingent on the emission of the response, causes the response to increase in frequency" (p. 46). Clearly, nothing in these definitions supports assertions that Brown and Hanlon limited the range of possible reinforcers to "obvious events" like "the presentation of candy" (Salzinger, p. 325) or an "expression of approval" (Wright, p. 155).

Although it is the case that Brown and Hanlon (1970) endorsed a very broad definition of *reinforcer*, Wright's (2006) interpretation is at least partly correct. In the study itself, Brown and Hanlon limited their inquiry to the potentially reinforcing role of explicit verbal approval, to the exclusion of other possible forms of reinforcement. Why such a narrow focus? Brown and Hanlon state,

We know that certain events are likely to be reinforcers ... for a given response because we have seen that they have this effect on many other responses. Money is supposed to be such a conditioned "generalized reinforcer" and social approval is supposed to be another. (p. 46)

To further support their decision to investigate the possible role of social approval, they provided the following passage from Skinner (1953):

Another person is likely to reinforce only that part of one's behavior of which he approves, and any sign of his *approval*, therefore, becomes reinforcing in its own right. Behavior which evokes a smile or the verbal response "That's right" or "Good" or any other commendation is strengthened. We use this generalized reinforcer to establish and shape the behavior of others, particularly in education. For example,

we teach both children and adults to speak correctly by saying "That's right" when appropriate behavior is emitted. (p. 78)

Of course, although these passages explain why Brown and Hanlon believed that social approval might function as reinforcement, the question remains: Given their acknowledgment that reinforcement should be broadly defined, why did they limit their investigation of potential reinforcers to obvious events like explicit verbal approval? They explain, "Because events subsequent to child speech are indefinitely various (or better, susceptible of being conceived in indefinitely various ways) one can never be sure that there is no event which functions as a reinforcer" (p. 46). Indeed, to falsify the claim that reinforcement plays a determinative role in language acquisition, an investigator "would have to show that there is *no event (or better, no way of conceiving events)* [italics added] which increases the frequency of syntactically correct utterances" (p. 46). Given that such potential events are "indefinitely various," Brown and Hanlon selected contingent verbal approval not only because of its supposed status as a generalized reinforcer but also for the practical reason that this more narrow hypothesis "is a testable one" (p. 47). Clearly, they limited their consideration of possible reinforcers to explicit verbal approval for practical rather than theoretical reasons. Brown and Hanlon offered data in support of the claim that children's syntactically correct utterances do not generally receive contingent explicit parental approval and therefore are not reinforced. Instead, they found that such approval is contingent on the truthfulness of the utterance.

For example, when Eve said "Mama isn't boy, he a girl" (a true but grammatically incorrect utterance), her mother responded "That's right," thereby expressing approval (Brown & Hanlon, 1970, p. 49). On the other hand, when Eve said "There's the animal farmhouse" (a grammatically correct but untrue utterance), her mother expressed disapproval by saying "No, that's a light-house" (p. 49). However, Brown and Hanlon did not assert that their study showed that parents fail to provide reinforcement, broadly and appropriately defined by them, for

appropriate grammar. On the contrary, they maintained that "strictly speaking, there is no way to disconfirm" (p. 46) the claim that parents provide such reinforcement. Indeed, 3 years later, Brown (1973) was still willing to entertain the hypothesis that parents use explicit approval, let alone reinforcement more broadly defined, to reinforce grammaticality. Specifically, despite his and Hanlon's own findings to the contrary, Brown (1973) said that the reinforcement-for-grammaticality hypothesis still sounded "sensible and may be correct" (p. 410).

The evidence for reinforcement. By Brown and Hanlon's (1970) own implicit admission, then, their study left unresolved the issue of whether or not parents reinforce their children's grammatical utterances. Specifically, with respect to the role that reinforcement may play, their research left two questions unanswered: Can reinforcement be shown, by means of experimental studies, to facilitate grammaticality in children? If the answer to the first question is yes, then what role (if any) do parents (i.e., principally mothers) play in providing such reinforcement? Regarding the first question, research published in the 1960s and 1970s (i.e., around the same time as Brown and Hanlon) has provided evidence of the efficacy of positive reinforcement in children's acquisition of grammaticality. For example, Guess, Sailor, Rutherford, and Baer (1968) used reinforcement procedures to teach a severely retarded girl the generative (i.e., indirectly trained, novel; see Alessi, 1987) usage of singular and plural phonemes:

During training trials, reinforcement was presented contingent upon correct imitation of singular and plural verbalizations by the experimenter, in response to objects presented to the subject singly and in pairs. A generative productive plural usage resulted, the girl correctly labeling new objects in the singular or plural without further direct training relevant to those objects. (p. 297)

Similarly, reinforcement was used to establish the generative usage of descriptive adjectives by disadvantaged preschool children (Hart & Risley, 1968); complete sentences in a speech-deficient child (Wheeler & Sulzer, 1970); verb usage in retarded

children (Schumaker & Sherman, 1970); *is* and *the* in a hearing-impaired preschooler (Bennett & Ling, 1972); *is* and *are* by both retarded subjects and developmentally normal toddlers (Lutzker & Sherman, 1974); and adjective–noun combinations and compound sentences by disadvantaged children (Hart & Risley, 1974, 1975). Other studies reporting the use of reinforcement procedures to bolster grammaticality also appeared around this time (e.g., H. B. Clark & Sherman, 1975; Garcia, 1974; Garcia & Batista-Wallace, 1977; Garcia, Guess, & Byrnes, 1973; Hester & Hendrickson, 1977; Heward & Eachus, 1979; Martin, 1975; Sailor, 1971; Stevens-Long & Rasmussen, 1974) and during the ensuing decades (e.g., Charlop, Schreibman, & Thibodeau, 1985; Chase, Ellenwood, & Madden, 2008; Greer & Yuan, 2008; Hernandez, Hanley, & Ingvarsson, 2007; Secan, Egel, & Tilley, 1989; Whitehurst & Valdez-Menchaca, 1988; Wulfert & Hayes, 1988).

In addition to the numerous studies that have employed experimenter-delivered reinforcement, research has also demonstrated that automatic reinforcement (Vaughan & Michael, 1982) can strengthen vocal behavior in children (e.g., Carroll & Klatt, 2008; Miguel, Carr, & Michael, 2001/2002; Smith, Michael, & Sundberg, 1996; Sundberg, Michael, Partington, & Sundberg, 1996; Yoon, 1998; Yoon & Bennett, 2000). Unlike direct reinforcement, which requires the deliberate delivery of reinforcement by another person, automatic reinforcement occurs without the direct mediation of another (Vaughan & Michael). For example, Sundberg et al. conducted an experiment with 5 participants (4 with language delays and 1 who was typically developing) ranging in age from 2 to 4 years. All 5 participants acquired “new vocal responses without the use of direct reinforcement, echoic training, or prompts” (p. 21). Sundberg et al. described the training trial protocol: “A familiar adult approached the subject and emitted a specific vocal sound, word, or phrase (the targeted response) immediately followed by the delivery of an established form of reinforcement (e.g., tickles, praise, clapping, bouncing in a parachute held by adults, animated parental attention).” As the result of this training, “for all subjects the pairing of a vocal sound, word, or phrase

with reinforcement resulted in the unprompted emission of that response in the postpairing condition” (p. 25).

In the aforementioned studies, researchers have offered evidence that automatic reinforcement can effectively promote the acquisition of relatively simple vocal responses (i.e., individual sounds, words, and phrases) in children. In addition, other studies (e.g., Silvestri, Davies-Lackey, Twyman, & Palmer, in preparation; Wright, 2006) have examined whether automatic reinforcement can be similarly effective in promoting a child’s acquisition of complex grammatical structures. For example, Wright investigated the role played by modeling and automatic reinforcement in the acquisition of the passive voice. The participants in the study were 6 children who ranged in age from 3 to 5 years. During the modeling condition, the experimenter showed the child a picture (e.g., of an elephant pulling a mouse) and described it for the child using the passive voice (e.g., “The mouse is being pulled by the elephant”). The study’s results “indicated that the children began using the passive voice only after the experimenter modeled passive sentences. Furthermore, the usage of the passive voice increased with repeated exposure to the experimenter’s verbal behavior” (p. 153). According to Wright, “repeated exposure to examples of the passive construction ... established the intraverbal frame *the Z is being &-ed by the X*” (p. 164), which the participants then used to describe novel pictures. Because use of the the passive voice was not explicitly reinforced (i.e., the children did not receive social praise or other forms of explicit reinforcement from the experimenter), Wright concluded that “their behavior was automatically reinforced for using the passive voice” (p. 153).

Taken together, this research suggests that reinforcement can be effective in strengthening grammaticality in children during language acquisition. Now consider the second question. What role (if any) do mothers play in providing reinforcement for their children’s grammatical verbal behavior? Returning to the topic of automatic reinforcement, the behavior of mothers (and other caregivers) provides an explanation, based on automatic reinforcement, of a child’s acqui-

sition of verbal behavior. Smith et al. (1996) explain,

An example of this type of conditioning occurs when a mother's verbal behavior becomes reinforcing because it is associated with other strong reinforcers ... (e.g., food, warmth) ... the child's reproduction of some aspects of her speech is automatically reinforcing in that "it sounds good" to sound like one's mother. (p. 40)

Novak and Pelaez (2004) have characterized the establishment of automatic reinforcement in children as a five-step process:

1. During parenting, the parents use the sounds of the child's native language.
2. Because of their association with other reinforcers provided by the parents, the sounds of the native language become secondary reinforcers themselves.
3. When the child babbles, by luck, one of these sounds (a response) is emitted.
4. The consequence of the response is hearing the sound (the secondary reinforcer).
5. The response (producing the sound) is automatically reinforced (by hearing the sound produced by the babbling behavior). (p. 268)

Thus, just as children's production of the phonemes of a language is attributable (at least in part) to automatic reinforcement, so too, by extrapolation, is their production of grammatically correct utterances. Children's well-formed utterances are presumably automatically reinforced because they sound like sentences their parents have uttered.

As previously noted, Brown (e.g., 1973; Brown & Hanlon, 1970) and his colleagues conducted a longitudinal study of the development of grammatical speech in 3 preschoolers (Adam, Eve, and Sarah). The study's data consisted of transcriptions of their verbal interactions with their mothers and, at times, their fathers and others. Using the original transcripts, Moerk (1990) conducted an exhaustive reanalysis of the verbal interactions of 1 of the 3 children (Eve) and her mother (at times, others joined the

conversation). As a result, Moerk found considerable evidence of direct reinforcement provided by the mother. Central to Moerk's study was a search for "three-term contingency patterns" (p. 293) and their "functional relationships" (p. 294). Defining the term *reinforcement* in the "Skinnerian sense" (p. 299), meaning, in this case, that "the probability of the filial response class should be increased" (p. 299), Moerk found evidence that two different types of responses delivered by the mother functioned as reinforcers: agreement and expansions.

The first type of reinforcement (*agreement*) Moerk (1990) operationally defined as Eve's mother saying "'yes,' 'yeah,' 'right,' or an equivalent response" (p. 298) after an utterance by Eve. According to Moerk, "many linguistic skills are first modeled by the mother; they are more or less directly imitated by the child and rewarded by a maternal 'yes' or a closely equivalent reinforcing response" (p. 298). Moerk found that Eve's mother used maternal agreement not only to teach vocabulary but also during "grammatical exercises" that placed a "strong emphasis on basic syntactic training" (p. 298). In the transcribed mother-child verbal interactions that Moerk analyzed, he found that this type of reinforcement was provided by Eve's mother approximately once every 2 min.

The second reinforcer type (*expansions*) has figured prominently in published accounts of parental responses to children's verbal behavior (e.g., Brown & Bellugi, 1964; Slobin, 1968). Expansions are "utterances that repeat a preceding filial utterance, retaining close topographical similarity in the major constituents but adding items that were omitted by the child" (Moerk, 1990, p. 300). For example, when Eve said "Eve lunch," her mother responded with the expansion "Eve is having lunch" (Brown, 1973). Based on his reanalysis of the data, Moerk found that these expansions, by imitating the major constituents appropriately used by Eve, signified approval and functioned as reinforcers for her use of those constituents. Specifically, by responding "Eve is having lunch," Eve's mother reinforced her use of those elements that were correct: (a) saying "Eve" and "lunch," as well as (b) saying "Eve" before saying "lunch," as is appro-

priate when speaking in the active voice. In addition, expansions also served as corrective feedback, a topic to be considered below.

Summary. Brown and Hanlon (1970) have been commonly misinterpreted as asserting that the parents in their study did not reinforce their children's grammatically correct utterances. A careful reading of that study reveals that its authors never made this claim. To be sure, they reported finding no evidence that explicit verbal approval (e.g., saying "that's right" or "correct") functions as reinforcement for grammaticality. However, they acknowledged that any of the "infinitely various" events that follow child speech could function as reinforcement. Indeed, during roughly the same time period in which Brown and Hanlon conducted their study, experimental evidence (e.g., Guess et al., 1968) demonstrated that grammatical development is susceptible to reinforcement. More recently, evidence has been offered to support the claim that reinforcement promotes language acquisition during naturally occurring parent-child verbal interactions. For example, Sundberg et al. (1996) have offered evidence to suggest that automatic reinforcement advances speech acquisition in children. Further, based on a reanalysis of some of Brown's original transcripts, Moerk's (1990) found that two types of maternal responses (agreement and expansions) functioned as reinforcement during language acquisition.

Myth 2

Brown and Hanlon (1970) claimed to offer evidence to demonstrate that parents do not provide negative evidence (i.e., corrective feedback) for their children's ungrammatical utterances.

Myth 2 examined. In the language acquisition literature, the term *negative evidence* has typically been used in a manner roughly synonymous with *corrective feedback*. For instance, Pinker (1995) defined negative evidence as "information about which strings of words are not grammatical sentences in the language, such as corrections or other forms of feedback from a parent that tell the child that one of his or her utterances is ungrammatical" (p. 153). An example of negative evidence is the use of explicit verbal

disapproval (e.g., saying "that's wrong") to punish a child's ungrammatical utterance. Like Myth 1, the supposed absence of negative evidence is yet another finding commonly credited to Brown and Hanlon (1970). As a case in point, Hoff (2005) has asserted that Brown and Hanlon's study found that "the mothers did not correct their children's ungrammatical utterances" (p. 229). In addition, just as the alleged lack of reinforcement has been viewed by many as externally valid, so too has the alleged no-negative-evidence finding. For instance, according to Osherson, Stob, and Weinstein (1989), Brown and Hanlon's results suggest that "negative information is not systematically available to the learner; for example, children are not systematically corrected for ungrammatical speech" (p. 23). Similarly, in Penner's (1987) view, Brown and Hanlon "concluded" that punishment does not function "to favor well-formed over ill-formed child utterances" (p. 376). Many others (e.g., Crain, 1991; Gleitman & Gleitman, 1986) have also claimed that Brown and Hanlon reported finding an absence of negative evidence. Further, the supposed lack of negative evidence has played a critical role in arguments for a nativist account of language acquisition. For example, consider the following nativist argument (adapted from Johnson, 2004):

Premise 1: If there are no innate constraints on language acquisition, then either (a) children have access to negative evidence or (b) natural languages are unlearnable.

Premise 2: Children don't have access to negative evidence.

Premise 3: Natural languages *are* learnable.

Conclusion: There are innate constraints on language acquisition.

In this (and similar) arguments, Brown and Hanlon's findings are frequently offered in support of Premise 2, that children lack negative evidence for ungrammaticality.

Myth 2 debunked. As I argued in discussing the first myth, Brown and Hanlon (1970) have been wrongly interpreted as denying that reinforcement plays a role in a child's acquisition of grammar. Similarly, they have

also been misinterpreted as denying a role to negative evidence. What, then, did they deny? In their study, Brown and Hanlon denied finding “even a shred of evidence” that contingent parental disapproval (e.g., saying “that’s wrong,” or “that’s not right,” or “no”) occurred following syntactically incorrect utterances. However, just as they did not narrowly define reinforcement as explicit approval, they also did not define punishment narrowly as explicit disapproval. Rather, *punishment* is the “*presentation of a negative reinforcer*” that results in depressing the frequency of the response it follows (Brown & Hanlon, p. 46).

Brown and Hanlon (1970) also noted that “strictly speaking, there is no way to disconfirm the following proposition: ‘Syntactically correct utterances become ... less frequent because of punishment’” (p. 46). Their rationale? “Because events subsequent to child speech are indefinitely various (or better, susceptible of being conceived in indefinitely various ways) one can never be sure that there is no event which functions as ... punishment” (p. 46). Why, then, did Brown and Hanlon limit their examination of the role of punishment to explicit disapproval? They gave two reasons. First, as reported in our discussion of the first myth, they surmised that explicit approval supposedly functions as a generalized reinforcer. So, they reckoned, “by extension, it seems reasonable to think that signs of disapproval would be generalized punishments” (p. 46). Their second reason? Limiting one’s investigation of the role of punishment to explicit disapproval yields a hypothesis that is “a testable one” (p. 46).

The evidence for negative evidence. As Brown and Hanlon (1970) implicitly acknowledge, their study left unanswered the question of whether or not parents provide negative evidence following their children’s ungrammatical utterances. Specifically, with respect to negative evidence, their study left two questions unanswered: Can negative evidence be shown, by means of experimental data, to reduce the occurrence of ungrammatical utterances in children? If the answer to the first question is yes, then what role (if any) do parents play in providing negative evidence?

With respect to the first question, experimental studies have provided evidence of the efficacy of negative evidence in weakening inappropriate vocalizations. For example, Ahearn, Clark, and MacDonald (2007) used response interruption and redirection to reduce the levels of vocal stereotypy, primarily “repeated words, word approximations, and noises” (p. 265), in 4 children (3, 7, 7, and 11 years old) who had been diagnosed with autism spectrum disorder. When the child engaged in vocal stereotypy, the trainer interrupted these behaviors by stating the child’s name and then issuing prompts for appropriate language. Ahearn et al. speculated that response interruption probably functioned as either punishment or sensory extinction. Other studies have reported the efficacious use of negative evidence to weaken a vocal tic in an 11-year-old boy (Valleley, Shriver, & Rozema, 2005); inappropriate vocalizations in an 18-year-old man with autism (Falcomata, Roane, Hovanetz, & Kettering, 2004); and speech dysfluencies in college students (Siegel, Lenske, & Broen, 1969). Taken together, this research suggests that the contingent use of negative evidence is a plausible mechanism for weakening errors in speaking.

Next consider the second question: What role (if any) do parents play in providing such negative evidence? As previously noted, expansions are one type of negative evidence that has received considerable attention. Brown and Bellugi (1964) originally introduced the term *expansions* to refer to a parent’s “interpretation” of a child’s *telegraphic* speech (utterances like “Eve lunch” and “cookie gone”). Brown (1973) explained,

Any given telegraphic utterance out of context is susceptible of a variety of interpretations. ... However, the utterance never is out of context, and the adult uses context to decide on one out of the set of possible expansions. The adult glosses the child’s utterance as just that simple sentence which, in view of all the circumstances, the child ought to have said and presumably did mean. When Eve said “Eve lunch” ... her mother might, in the basis of the words alone, have provided such expansions as “Eve has had lunch” or “Eve will have lunch,” but on this occasion it was the noon hour and Eve was at the table

eating, and so the expansion had to be:
 "Eve is having lunch." (pp. 105–106)

Brown and Bellugi viewed expansions "as a potentially valuable training technique ... an especially valuable kind of feedback" (Brown, p. 106). In short, they hypothesized that expansions might function as corrective feedback. To that end, Cazden (1965), another colleague of Brown's, conducted an experiment to study the efficacy of expansions as corrective feedback. Summing up Cazden's results, Brown stated that Cazden "did not find any evidence that expansions were effective" (p. 106).

However, despite Cazden's (1965) findings, Brown and Hanlon (1970) continued to grant that "repeats of ill-formed utterances usually contained corrections and so could be instructive" (p. 43). As Moerk (2000) wryly observed, this "position regarding corrections was ... quite accepting and positive. But this admission of corrections was almost never referred to by later investigators: an interesting case of selective attention" (p. 111). Unfortunately, although Brown and Hanlon still acknowledged that expansions could be effective in promoting grammaticality in children, their study did not address this issue. Indeed, Zukow (1990) has reported that Hanlon, in a personal communication, stated that her study (Brown & Hanlon) was not designed to examine the role of all forms of correction, but only those operationally defined as explicit disapproval. For example, if a child said "Doed it!" a parental response of "That's wrong!" counted as a correction, but responding with "You DID it!" was not so scored (Zukow, 1990, footnote, p. 719).

As discussed above, Moerk's (1990) reanalysis of Brown's transcripts revealed that expansions provided both reinforcement and corrective feedback (i.e., negative evidence). In short, expansions "fulfill a double function" (Moerk, p. 300). By imitating "the correct part of the child's utterance" (Novak & Pelaez, 2004, p. 284), maternal expansions signified maternal approval and functioned as reinforcers. In addition, in their role as corrective feedback, expansions "function as discriminative stimuli in the form of prompts" for grammatically correct responses by the child (Novak & Pelaez, p. 284).

Moerk also identified a "third function of maternal expansions," namely, expansions can "conclude a verbal exchange" (p. 301), thus revealing a second way in which expansions provide corrective feedback. Recasting this third function in behavior-analytic terms, "expansions ... also may punish an immediate child repetition. The data show that maternal expansions often terminate an interaction" (Novak & Palaez, p. 284). Thus, Moerk's reanalysis of Brown's data offers evidence that Eve's mother provided expansions that, as negative evidence, functioned to prompt well-formed utterances while simultaneously punishing those that are ill formed. However, as Hanlon's personal communication to Zukow (1990) reveals, the Brown and Hanlon (1970) study was not designed to examine negative evidence so broadly conceived. Rather, they ignored maternal responses in their role as prompts, and, except for explicit disapproval, ignored their role as punishers. In short, their study did not address, let alone deny, the existence of negative evidence.

Subsequent research has demonstrated that negative evidence, in its various forms, plays a robust role in language acquisition. For example, in the 1980s, a number of researchers (e.g., Bohannon & Stanowicz, 1988; Demetras, Post, & Snow, 1986; Hirsh-Pasek, Treiman, & Schneiderman, 1984; Morgan & Travis, 1989) demonstrated that parents provide negative evidence. For instance, Hirsh-Pasek et al.'s study found that parents are more likely to repeat ill-formed utterances than those that are well-formed, and that these repetitions frequently include corrections. By the late 1980s, the accumulating evidence led the field of language acquisition studies "to at least question the old dogma" (Moerk, 2000, p. 108) denying the existence of negative evidence. In addition to studies that showed that negative evidence exists, the 1980s also ushered in evidence to demonstrate its effectiveness in facilitating linguistic competence (e.g., Becker, 1988; Goldstein, 1984; Scherer & Olswang, 1984).

The 1990s produced more studies (e.g., Bohannon, MacWhinney, & Snow, 1990; Saxton, 1992, 1993; Smith et al., 1996) that demonstrated the existence of negative evidence, as well as studies (e.g., Farrar, 1990, 1992; Moerk, 1990) that showed its

effectiveness. With respect to the former, Moerk (2000) noted that “probably close to one hundred such studies exist supporting the existence of corrections” (p. 117). Evidence for its effectiveness has continued to accumulate (e.g., Chouinard & Clark, 2003; Strapp, Bleakney, Helmick, & Tonkovich, 2008; Strapp & Federico, 2000).

Summary. Brown and Hanlon (1970) have been misinterpreted as claiming that the parents in their study did not provide negative evidence contingent on instances of ungrammatical utterances by their children. To be sure, Brown and Hanlon offered data suggesting that parents do not use explicit disapproval to punish their children’s syntactically incorrect utterances. However, Brown and Hanlon did not assert that parents fail to provide punishment, broadly and appropriately defined, for ungrammaticality. On the contrary, just as they maintained that “strictly speaking, there is no way to disconfirm” (p. 46) the claim that parents reinforce grammatical utterances, so they acknowledged that there is no way to disconfirm claims that ungrammaticality is punished. Indeed, Hanlon’s personal communication to Zukow (1990) brings to light the fact that Brown and Hanlon’s study was not designed to examine other forms of negative evidence (e.g., other types of punishment and prompts) and thus cannot be properly cited by others as evidence that such corrective feedback is nonexistent. In the years following Brown and Hanlon’s study, accumulating evidence indicates that not only does negative evidence exist (e.g., Bohannon et al., 1990), but that it is effective in weakening inappropriate vocalizations (e.g., Ahearn et al., 2007) and ungrammaticality (e.g., Goldstein, 1984). Indeed, Moerk’s (1990) reanalysis of Brown’s original data offers evidence that parents provide negative evidence that functions (a) to punish those elements of a child’s preceding utterance that were ungrammatical and also (b) to prompt the child to emit a corrected form of that utterance.

Myth 3

Gold (1967) claimed to offer a proof to demonstrate that, without negative evidence,

a child cannot acquire a language exclusively from the linguistic environment.

Myth 3 examined. In his seminal paper, Gold (1967) offered a mathematical model of language learning in which two different training methods were simulated. The *informant* method presented the “learner,” conceptualized as a mathematical function instantiated as a Turing machine, with both grammatical and ungrammatical sentences (labeled as such for the learner during training). The *text* method presented only grammatical sentences (not labeled); in other words, no ungrammatical sentences were provided to the learner. Thus, the first method provided negative evidence in the form of ungrammatical sentences (labeled as such), but the second method did not. As a result of this simulation, Gold demonstrated that, with the first method, the learner can learn an *infinite* language (i.e., one like English, comprised of an infinite number of sentences). But when negative evidence is lacking (as with the second method), an infinite language like English cannot be learned. The proof that an infinite language is unlearnable when negative evidence is lacking has come to be called *Gold’s theorem* (hereafter, GT).

GT has gained widespread acceptance. For example, according to Elman et al. (1996), “Gold was able to show that formal languages of the class which appear to include natural language cannot be learned inductively on the basis of positive input only” (p. 342). Further, GT has also been widely regarded as applying not just to a learner qua mathematical function but to children as well. For instance, citing Gold (1967) as a principal source, Pinker (1995) has asserted that “in the absence of negative evidence, children who hypothesize a rule that generates a superset of the language will have no way of knowing that they are wrong” (p. 153). Many other examples (e.g., A. S. Clark, 2001) illustrate the pervasiveness of this view. Within mainstream psycholinguistics, language acquisition has been commonly viewed as the process by which children develop hypotheses regarding the grammars of the languages to which they are exposed. According to this view, without constraints (innate or learned)

children are likely to develop hypotheses that yield ungrammatical construction.

According to Crain and Thornton (1998), “it is conceivable that constraints could be learned by children, assuming the usual mechanisms of induction, only if the relevant kind of evidence is available. This evidence is called *negative evidence*” (p. 20). However, when GT is coupled with Brown and Hanlon’s (1970) supposed no-negative-evidence finding, the following question arises: How does one explain that, unlike Gold’s learner, children do, in fact, acquire language even though they presumably lack negative evidence? As Scholz (2004) succinctly expressed the problem, GT “entails that learning natural languages is impossible. Yet children do learn them—contradiction” (p. 959). The popular nativist solution has been to posit, within children’s brains, “innate, domain specific restrictions of the class of possible grammars” (Matthews, 1989, p. 13).

Employing the tools of Turing machine modeling and mathematical logic, Gold (1967) constructed “a precise model for the intuitive notion ‘able to speak a language’ in order to be able to investigate theoretically how it can be achieved artificially” (p. 448). For those who lack formal training in mathematical logic, GT is difficult, if not impossible, to follow. Assuming that most readers lack this knowledge, a nonmathematical account of GT is provided here. Unfortunately, this approach also presents a challenge to the reader, that is, understanding a mathematical argument presented in non-mathematical language. However, given the centrality of GT to the nativists’ critique of empiricist accounts of language acquisition, at least a passing familiarity with the details of GT is needed by nativists and empiricists alike. Two versions of a nonmathematical account of GT are offered here. The first version provides a relatively short, easy to follow version drawn from Pullum and Scholz (2003). The second version, based on an account provided by Johnson (2004), is a more detailed, lengthier summary of GT. Because this second version can be difficult to follow, the reader may elect to skip it (while still achieving an adequate understanding of GT by having read the first version). Consider now the first version.

Gold’s theorem: Version 1. Pullum and Scholz (2003; Scholz, 2004) offered an account of the general logic that underlies GT by describing the plight of two imaginary learners, Bold Bonnie and Cautious Connie. While being exposed to sentences from her target language, and lacking negative evidence, Bold Bonnie sometimes mistakenly “hypothesizes a grammar for an infinite proper superset of a finite target language” (Scholz, p. 960). In other words, she guesses a grammar that not only fits the finite language she is learning but also includes grammatical rules that are part of a larger, infinite language, one that is not her target language. As a consequence, lacking negative evidence, “she can never recover, since no text can refute her over-liberal hypothesis” (Scholz, p. 960). Cook (1988) makes the same point by noting the importance of negative evidence to the learning of games:

After years of watching snooker on television, I know from observation some of the sequences of colours in which balls are hit. ... I have no idea what sequences are impossible because I have only seen sequences in which the rules are obeyed rather than those in which they are broken. ... An adequate knowledge of snooker involves knowing what *not* to do as well as what do do. To learn snooker properly, I would require some other type of evidence. One possibility is to see players breaking the rules. ... Furthermore, to recognize a sequence as a mistake, something must indicate that it is wrong, such as a penalty from the referee, or the hissing of the crowd; otherwise it is another permissible sequence to add to my stock. A further possibility is to learn from the mistakes I make while actually playing. (p. 59)

Next consider the case of Cautious Connie, who “never hypothesizes a grammar for an infinite language if some grammar for a finite language is consistent with the sequence of strings presented so far” (p. 960). As Cautious Connie is exposed to “successively larger finite languages,” she hypothesizes grammars consistent with the data presented. However, when presented with an infinite language, this strategy fails. Thus, there is at least one member of the superfinite class the learner does not identify correctly. According to Scholz (2004), “Gold showed that EV-

ERY strategy has either Bonnie's problem or Connie's" (p. 960).

Gold's theorem: Version 2. In his proof, Gold (1967) employed mathematical "surrogates" to represent (a) language, (b) the learner, (c) a criterion of successful learning, and (d) the learner's environment (Johnson, 2004, p. 573). Each of these will now be presented in turn. Gold defined *language* as "a distinguished set of strings" (p. 448). A *string* is "a sentence, or part of a sentence" (Trask, 1993, p. 261). Specifically, each string of a language is a finite sequence of elements drawn from a finite alphabet. To produce a language as defined above, one must first select "some finite alphabet, Σ , and then create Σ^* , the set of all the finite sequences of elements of Σ , and then define a language as any subset of Σ^* " (Johnson, p. 573). Gold acknowledged that "such a language is too simple to do anything with (for instance, to give information or to pose problems)" (p. 448). Nonetheless, a language, so defined, served its purpose in Gold's study because (according to him) it "has enough structure to allow its learnability to be investigated" (p. 448). Gold's *learner* is "any function that takes finite initial sequences of an environment as input, and yields as output a guess as to the target language" (Johnson, p. 574).

Next consider Gold's (1967) *criterion of successful learning*. In his a priori inquiry, Gold did not investigate the learnability of individual languages but rather classes of formal languages. Specifically, the formal language classes investigated for their learnability included recursive, recursively enumerable, primitive recursive, context-sensitive, context-free, regular, finite cardinality, and superfinite languages (Gold, p. 452; for a concise discussion of these language classes, see Trask, 1993). Prior to exposure to linguistic input, there exists within the learner a "naming relation which assigns names (perhaps more than one) to languages. The 'learner' identifies a language by stating one of its names" (Gold, p. 449). As Gordon (1990) explicated Gold's learner, "For all intents and purposes, the learner already knows the functions (i.e., grammars) that generate the languages within the class. All it has to do is figure out which one it is being presented with" (p. 217). "At each time t the

learner is presented with a unit of information ... concerning the unknown language L " (Gold, p. 449). Further "At each time t the learner is to make a guess ... of a name of L based on the information it has received through time t " (p. 449). Hence,

A class of languages will be considered learnable with respect to the specific method of information presentation if there is an algorithm that the learner can use to make his guesses, the algorithm having the following property: Given any language of the class, there is some finite time after which the guesses will all be the same and they will be correct (p. 447)

Making the same, correct guesses each time a sentence (from any language within the specific language class) is presented to the learner is called "language identification in the limit" and constitutes Gold's criterion of successful learning.

Finally, consider the *learner's environment*. Like a human learner, Gold's (1967) learner does not learn a language by having its rules explicitly provided to it; instead, like its human counterpart, Gold's learner has to "learn its rules from implicit information" (p. 448) present in the environment. In Gold's model, the learner's environment consists of a "training program" that delivers "implicit information, such that it is possible to determine which of the definable languages is being presented" (p. 448). Gold compared two different methods, the *text* and *informant* methods, for presenting implicit information to the learner about the rules of the target languages. Thus, two different types of linguistic environments were employed. Both methods present "the strings of the language in any order such that every string of the language occurs at least once" (p. 447). However, with the text method, only grammatically correct strings are presented, whereas the informant method presents both grammatical and ungrammatical strings. Further, with the informant method, the informant "tells" (Gold, 1964) the learner when a string is grammatically correct and when it is not.

Employing these four surrogates representing language, the language learner, a criterion of successful learning, and the learner's environment, Gold (1967) provided

arguments to support a number of theorems regarding language learnability, the most famous of which (GT) can be succinctly stated as follows: “No class of languages that is superfinite ... is identifiable in the limit from text” (Scholz, 2004, p. 959). In other words, lacking negative evidence, no superfinite class of languages can be learned. But what is a superfinite class of languages? To answer this question, first consider the distinction between languages of finite versus infinite cardinality. A language of finite cardinality contains a finite number of sentences. On the other hand, a language of infinite cardinality (e.g., English) contains an infinite number of sentences. Now the question previously posed can be answered. “A *superfinite class of languages* denotes a class which contains all languages of finite cardinality and at least one of infinite cardinality” (Gold, p. 452).

According to Johnson (2004), “the logic of Gold’s theorem shows that there are many logically possible classes of languages that no learner can learn” (p. 574). To explain the logic of GT, Johnson offers the following argument of his own. Let C be a language class that contains an infinite number of languages: L_∞ , L_1 , L_2 , L_3 , and so on. Further, L_1 is a proper subset of L_2 , which is a proper subset of L_3 , and so on. Finally, L_∞ contains all the other languages (i.e., L_1 , L_2 , L_3 , etc.). Hence, L_2 contains all the sentences of L_1 plus additional sentences, L_3 contains all the sentences of L_2 (and thus, all the sentences in L_1) plus some more, and so on. Similarly, L_∞ contains all the sentences of the languages subsumed under it. Given these properties, C (some language class) is said to have the “Gold property” (p. 574). Indeed, any class of languages that satisfies these conditions can be said to have the Gold property. Given this definition, Johnson then offers a proof of what I call *Johnson’s theorem* (hereafter, JT): “Any class of languages with the Gold property is unlearnable” (p. 575). Johnson offered JT as a way of explicating the “general logic” (p. 576) behind GT’s proof.

Consider a learner \emptyset . To prove JT, we must show that there is “some language L in C and some environment E from L such that \emptyset does not learn L given E ” (Johnson, 2004, p. 575). Here *learn* means “identify in the limit.” To that end, \emptyset is provided a training

program that employs the text method of information presentation in which only grammatically correct sentences are presented to the learner. According to Johnson, to prove JT, one has to show that,

There must be some language L in C and some environment E from L such that \emptyset does not learn L given E We start by giving \emptyset sentences from L_1 , and continue doing so until it ‘converges’ onto a guess that L_1 is the target language, and will not change its guess as long as it receives only sentences from L_1 Once \emptyset has converged onto a guess that L_1 is the target language, we then start adding sentences from L_2 to this environment. Since L_2 contains all the sentences in L_1 , plus some more, the initial sentences we gave \emptyset to get it to converge to a guess of L_1 are also contained in L_2 . Now we give \emptyset sentences from L_2 until it converges onto a guess that L_2 is the target language. ... In general, we get \emptyset to converge onto a guess that L_n is the target language, and then we start giving it sentences from L_{n+1} . Since L_n is always a proper subset of L_{n+1} , we are always building a legitimate environment from L_{n+1} (until we switch to L_{n+2}). Moreover, since L_∞ contains all (and only) the sentences from the entire sequence of languages, at no point in this strategy will we give \emptyset sentences that are not part of L_∞ . (p. 575)

Given this training program, the learner confronts the following “insuperable dilemma” (p. 575). On the one hand, the learner must keep changing its guess an infinite number of times as it successively masters the infinite, nested series of languages (i.e., L_1 to L_2 , to L_3 , etc., ad infinitum). But if \emptyset guesses an infinite number of times, then, by definition, \emptyset will not learn a given target language in a finite amount of time. In other words, “if a learner changes its mind infinitely many times, then it is impossible to fix on a particular language at some finite point in time, and thereafter never change its mind” (K. Johnson, personal communication, December 30, 2007). So, suppose instead that the learner only changes its guess a finite number of times. For example, suppose that after giving \emptyset sentences from L_n , it correctly guesses L_n and then does not change its guess, despite giving it sentences from L_{n+1} forever (as required by our strategy). This means it will fail to acquire

L_{n+1} , but also fail to acquire L_{n+2} , ..., L_{∞} . Similarly, suppose that we give \emptyset sentences from L_n and it guesses L_{∞} . Again, our strategy requires us to continue providing sentences from L_n forever. And again, because the learner has stopped changing its guesses despite this input, the learner has failed to learn L_n as well as all of the other larger subsets of L_{∞} (e.g., L_{n+1} , L_{n+2} , etc.). Regardless of which guessing strategy is employed, there is always some language (or languages) within C that the learner fails to identify. So, JT has been proven. Further, except for "some technical details that are not relevant," JT's proof "characterizes the general logic behind GT's proof" (p. 576).

Myth 3 debunked. Within traditional psycholinguistics, GT has been widely interpreted as applying to language acquisition in children. For example, Demopoulos (1989) has reported that Gold's (1967) text method, which presents the learner with grammatical strings but no ungrammatical strings and no negative evidence, as well as Gold's criterion of success (language identification in the limit) "are generally regarded as plausible idealizations of the human learning situation" (p. 79). More specifically, according to the mainstream view, GT proves that, lacking negative evidence, a child cannot acquire a language by exposure to a linguistic environment alone. The myth, which I will argue against, is that Gold himself made this assertion. To that end, consider whether the supposed unavailability of negative evidence (as modeled in GT) represents a plausible idealization of a child's linguistic environment. Contrary to the myth, Gold acknowledged that it may not. To clarify this acknowledgment, recall how negative evidence is portrayed in GT. Under the informant condition, both grammatical and ungrammatical strings are presented to the learner. Every time the learner is presented with an ungrammatical string, the informant "tells" the learner that the string is "not in the language" (Gold, 1964, unpaginated), thereby providing negative evidence. Unfortunately, Gold does not provide an example of such negative evidence. However, Elman et al. (1996) have offered the following as an exemplar of the type of direct negative evidence that Gold had in mind: In response to a child's saying "Bunnies is cuddly," the

parent responds "The following sentence, 'Bunnies is cuddly,' is not grammatical" (p. 342). Based on Elman et al.'s exegesis, GT's model of negative evidence represents a form of explicit corrective feedback, reminiscent of how Brown and Hanlon (1970) operationally defined *negative evidence*.

Did Gold (1967) assert, then, that such explicit corrective feedback was the only form of negative evidence that could be available to children? If he did, then he appears to be asserting that children can only learn a language if parents provide explicit corrective feedback for ungrammaticality. However, he did not so limit what may function as negative evidence. On the contrary, he granted that children, unlike the learner in his theorem, might receive negative evidence "by being corrected in a way we do not recognize" (p. 453). Gold's admission presages Brown and Hanlon's (1970) later admission that any of a parent's "indefinitely various" (p. 46) responses to a child's ungrammatical utterances could function as negative evidence. In other words, Gold implicitly acknowledged that the mathematical surrogates for negative evidence that appear in his proof may not accurately represent some of the forms of negative evidence available to children. Unfortunately, as the persistence of the myth attests, this acknowledgment has not found currency in the traditional psycholinguistic literature.

In addition to his acknowledgment that forms of negative evidence other than explicit disapproval may play a role in language acquisition, Gold (1967) made an even more striking admission. Regrettably, this admission, too, has failed to gain currency among mainstream psycholinguists. Specifically, he recognized that what is typically referred to as *indirect* negative evidence (e.g., Chomsky, 1981) may also play a role in language acquisition in children. According to Gold, "the child may learn that a certain string is not acceptable by the fact that it never occurs in a certain context. This would constitute a negative instance" (p. 454). For example, children learning English typically never hear grammatical constructions such as "The dog the cat chased" or "I have dinner eaten." As Scholz (2004) explained, the

learner infers “from the absence of some word form or construction X ... that X is ungrammatical in the target language” (p. 961). In short, the learner makes an “inference from absence of evidence to evidence of absence” (p. 961). Gold recognized that, unlike the learner modeled in GT, human learners may not only profit from forms of *direct* negative evidence other than explicit disapproval but also from *indirect* negative evidence as well.

Additional criticisms of Gold’s theorem. Besides Gold (1967) himself, others have found grounds for arguing against the applicability of GT to language acquisition in children. For instance, Johnson (2004) has argued that there is a “serious problem” with GT in terms of its “psychological utility” (p. 585). Consider the distinction between “two very different criteria of learnability” (p. 585): language *identifiability* and language *acquirability*. Gold’s proof addresses the identifiability, not the acquirability, of languages. According to Johnson, in GT a class of languages C is identifiable if and only if “there exists a function ... such that for any environment E for any language L in C, ... [that function] permanently converges onto the hypothesis of L as the target language after some finite time” (p. 585). In contrast, a class of natural languages C is acquirable if and only if “given almost any normal human child and almost any normal linguistic environment for any language L in C, the child will acquire L (or something sufficiently similar to L) as a native language between the ages of one and five years” (p. 585). This distinction between language identifiability and acquirability raises major questions about the applicability of GT as a model for language acquisition in children.

For example, consider the issue of the amount of time it may take to identify versus to acquire a language. In Gold’s (1967) model, language identifiability can occur quickly. Indeed, “the first guess could be correct” (Gordon, p. 1990, p. 218). Alternatively, it could take “several billions of years” (p. 218) to identify the target language. On the other hand, language acquirability in children occurs within a much more constricted time frame, that is, during the preschool years. Another issue is

the difference in the range of possible environments. Johnson has noted that identifiability applies to “all environments, however odd or repetitive,” whereas acquirability applies almost always to normal linguistic environments. Because acquirability addresses fewer environments than identifiability does, it is less likely that problematic linguistic input (e.g., input that lacks frequent and diverse types of negative evidence) will occur when acquiring (vs. identifying) a language. In addition, with identifiability, the learner demonstrates successful learning when, given adequate exposure to the requisite linguistic inputs, it successfully names each language. In contrast, with language acquisition, successful learning is demonstrated when the child successfully speaks and understands the language of his or her verbal community.

Finally, consider the distinction between finite and infinite languages. The Boston telephone directory is an example of a finite language. When exposed to the text method of training, Gold’s (1967) learner can learn it. Here *learn* means that the learner can recognize whether or not a particular string is in the language (i.e., in this case, in the Boston directory). However, among human learners, “only the most accomplished mnemonist” (Gordon, 1990, p. 218) could possibly learn it. On the other hand, the class of all possible phone books (an infinite language) can easily be learned by humans. As Gordon explained, “anyone who has experience with telephone directories could tell whether a particular string is a potential listing within a potential telephone directory” (p. 218). Thus, given the disparities between language identifiability and language acquirability (Gordon; Johnson, 2004), the relevance of GT’s pronouncements about negative evidence, when applied to language acquisition in children, appears to be doubtful.

Summary. In offering what has come to be called Gold’s theorem, Gold (1967) has been interpreted as demonstrating that, without negative evidence, a child cannot acquire a language from environmental input alone. When combined with Brown and Hanlon’s (1970) supposed finding that parents do not provide negative evidence, linguistic nativists have concluded that children therefore

possess innate, domain-specific knowledge about language. However, Gold never interpreted GT as proving that which has been popularly attributed to it. In GT, negative evidence takes the form of explicit corrective feedback. However, Gold acknowledged the possibility that “the child receives negative instances by being corrected in a way we do not recognize” (p. 453).

Even more striking, however, is Gold’s (1967) admission that “the child may learn that a certain string is not acceptable by the fact that it never occurs in a certain context. This would constitute a negative instance” (p. 454). In short, Gold recognized that his mathematical model may not accurately represent the types of negative evidence available to children. Unfortunately, these admissions have not found their way into the mainstream psycholinguistic literature. Lastly, others have offered additional arguments against the applicability of GT to language acquisition in children. For instance, Gordon (1990) and Johnson (2004) have argued that GT’s criterion of learnability (language identifiability) represents a substantial departure from the criterion of learnability for children (language acquisition). Thus, GT’s findings regarding negative evidence are of questionable relevance when applied to language learning in children.

CONCLUSION

The two basic approaches to language acquisition, nativism and empiricism, have been traditionally framed in terms of the nature–nurture distinction. Both approaches agree that nature and nurture play necessary roles, but nativists place greater emphasis on inborn knowledge (nature), and empiricists place greater emphasis on information available in the linguistic environment (nurture). Both agree that, without limits (linguistic constraints) placed on permissible grammatical structures for the language being acquired, the child will acquire a grammar prone to yield grammatical errors.

Those who study language acquisition from an empiricist perspective have argued that evidence (both positive and negative) available in the linguistic environment provides adequate constraints when learning a language. For example, children are provided

positive evidence (a) when their grammatically correct utterances are directly reinforced by adults (e.g., Rheingold, Gewirtz, & Ross, 1959; Weisberg, 1963); (b) when their grammatically correct utterances are indirectly reinforced by adults by means of automatic reinforcement (e.g., Smith et al., 1996; Sundberg et al., 1996); and (c) when adults provide grammatically correct exemplars (e.g., Moerk, 1983). Further, they are provided direct negative evidence when their grammatically incorrect utterances result in corrective feedback (e.g., Moerk, 1991) as well as indirect negative evidence by usually not being exposed to grammatically incorrect utterances (e.g., Gold, 1967).

Of course, the nativists have not remained quiescent. In particular, they have offered (a) research (e.g., Brown & Hanlon, 1970) to suggest that children’s grammatically correct utterances are not reinforced; (b) research (e.g., Brown & Hanlon) to suggest that children are not provided with negative evidence when speaking ungrammatically; and (c) a mathematical proof (Gold, 1967) that allegedly demonstrates that without negative evidence, a child cannot acquire a language solely from environmental input. All three offerings have played particularly vigorous roles in nativist arguments. Typically, those who are sympathetic to an empiricist account of language acquisition have responded to the Brown and Hanlon study by critiquing it on methodological grounds, for example, by noting that there were too few participants in the study (e.g., Zukow, 1990) or by providing evidence that both reinforcement and negative evidence not only occur but are efficacious in strengthening grammaticality and weakening ungrammaticality during language acquisition (e.g., Moerk, 1990). Similarly, the common response to Gold has been to offer evidence in support of the existence and efficacy of negative evidence.

These counterarguments are important and powerful, but the empiricist case would have been more persuasive if it had been noted, from the outset, that Brown and Hanlon (1970) offered evidence against only one form of reinforcement (explicit approval) and one form of negative evidence (explicit disapproval) while they explicitly left the door open for the investigation of additional

forms of both. In a similar manner, the argument for an empiricist account would have been strengthened by pointing to Gold's (1967) statements that suggested the inapplicability of his theorem to language acquisition in children. Unfortunately, behavior analysts, as well as others sympathetic to an empiricist account of language acquisition, appear to have made a strategic error when they failed to characterize Brown and Hanlon's study and Gold's theorem more accurately. Had these works been more accurately described, subsequent research on the role of reinforcement and negative evidence could have been characterized as an extension of Brown and Hanlon's findings, and therefore perhaps viewed more favorably by those who resist an empiricist account. In this paper I have argued that (a) Brown and Hanlon never claimed to offer data to demonstrate that adults fail to reinforce children's grammatical utterances. Further, I have also argued that (b) Brown and Hanlon never claimed to offer data to demonstrate that adults fail to provide negative evidence, and (c) Gold (1967) never claimed to offer a proof to show that without negative evidence, children cannot acquire a language solely from environmental input. Correcting the record enhances the armamentarium available to those who argue for an empiricist approach to language acquisition.

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