A Philosopher's War on Poverty of the Stimulus Arguments:
A Review of Fiona Cowie's
What's Within? Nativism Reconsidered

Ted Schoneberger
Stanislaus County Office of Education

In *What's Within? Nativism Reconsidered* (1999) Fiona Cowie addresses three questions: (1) What is nativism? (2) What is meant by calling some trait "innate"? and (3) What types of evidence should be offered when claiming innateness? This review concentrates on these questions as they pertain to Chomsky's faculties-based account of language acquisition. In particular, this review focuses on Cowie's critique of three versions of the poverty of the stimulus argument (POSA): (1) the *a posteriori* POSA, (2) the logical problem POSA, and (3) the iterated POSA. In addition, counter arguments to her critique, and Cowie's response, in turn, to some of those counter arguments, are also reviewed.

Key Words: nativism, innateness, Chomsky, domain specificty, negative evidence, hypothesis testing, structure dependence.

In *What's Within? Nativism Reconsidered* (1999), the author, philosopher Fiona Cowie, addresses three principle questions:

1. "What is nativism?"
2. "What does it mean to say some trait is innate?"
3. "What kinds of evidence should support such a claim?" (p. viii)

Focusing, in particular, on nativist accounts of the mind, the book is divided into three parts. Part I examines the historical controversy regarding "innate ideas," part II concerns Jerry Fodor's nativist account of our knowledge of concepts, and lastly, part III considers Noam Chomsky's faculties-based account of language acquisition. In this review I concentrate on part III; in particular, on the author's comprehensive exegesis of three versions of the argument from poverty of the stimulus, as used by Chomsky and other nativists. I begin with her examination of the nature of nativism.

Ted Schoneberger, Stanislaus County Office of Education. In this book review I quote copiously from the book. Any given paragraph frequently contains a number of quotes from a specific page. To promote easier reading, such paragraphs generally cite the page number(s) for these quotes only once.

Address correspondence to Ted Schoneberger, P.O. Box 157, Turlock, CA 95381; e-mail: CalifBehavior@aol.com.

WHAT IS NATIVISM?

Historically, nativism has been identified with the doctrine of innate ideas. As the author (p. 3) expresses it, this doctrine maintains that "the character of our mental furniture is to a large extent internally rather than environmentally determined." To say that our ideas are, in large measure, internally (rather than externally) determined, is to say that our ideas are in our minds at birth, that they are a component of our biological endowment. Given this general formulation, the author (p. 4) sees a number of problems of interpretation arising. First, to which of the "several different kinds of cognitive equipment" are nativists ascribing innateness? "In general, 'what is in our minds' receives a threefold classification into ideas (or concepts), beliefs (and other propositional attitudes), and faculties or capacities (such as our ability to reason or to learn a language)." Not surprisingly, nativists do not agree on which of these are innate.

During the seventeenth century, the Cambridge Platonists attributed innateness to many moral and religious beliefs. For example, not only was a belief in the existence of God considered innate, so was the belief that it is wrong to expose ones "obscene parts" to public view. However, the author (p. 4) reports that "Chomsky has not, to my knowledge, considered whether we have any innate opinions as to the probity of indecent exposure." In short,

"although Chomsky and his Cambridge cohorts share the view that *something* mental is innate, they may disagree as to what is innate. We therefore need a characterization of nativism that is both 'deep' and yet general enough to accommodate substantial doctrinal differences like these."

One major explanatory strategy employed by nativists has been the use of metaphor. For example, Descartes compared innate ideas to inherited diseases, while Leibniz compared them to the veins in a slab of marble before being fashioned into a statue. Employment of such metaphors by nativists suggest, to the author, a dispositional account of innate ideas. The author:

> Just as the symptoms of hereditary gout are not present at birth, and just as the statue of Hercules is only implicit in the layout of the faults in the stone, innate ideas are not literally *there* in the mind of the neonate. Rather, their emergence is conditional upon the occurrence of certain other events or processes, in much the same way as uncovering the statue in the marble requires the sculptor's skillful chiseling or the manifestation of the disease requires that the unfortunate individual reach (say) middle age. (p. 5)

However, some other metaphors used by nativists suggest a *non*-dispositional account. For example, the Cambridge Platonists maintained that innate beliefs are "written" in one's bosom. Other nativists liken them to a clothing store's stock of suits. For the author (p. 5), such metaphors suggest that "beliefs and ideas are present in the mind at birth in some quite straightforward and non-dispositional sense." Thus, in addition to a disagreement among nativists about which contents of the mind (concepts, beliefs, or capacities) are innate, there is also "an orthogonal division of nativism into its dispositional and non-dispositional versions."

Confronted by these difficulties in elucidating nativist doctrine, the author (p. 6) considers an alternative, "more subtle approach." Namely, one can determine what nativists believe by figuring out "what problem they think nativism is a solution to and why they think nativism solves that problem, inferring thence what nativism must be in order that it solve that problem for that reason." The author reports that one of the principle problems to which nativism has attempted a solution is answering the question "Where does what is in our minds come from?" As an answer to this question, nativism is simply "the view that what is in our minds did not (in any very interesting sense) *come to be there* at all. Rather, it always was there; it was born with us; it is innate." By contrast, empiricism maintains that "the contents of our minds are not (in any very interesting sense) born with us. Instead, they come 'from experience' or 'from the senses'" (p. 16).

According to the author (p. 16), this "popular account" of nativism is exemplified by Godfrey-Smith's (1996) distinction between internalist vs. externalist accounts of an organism's properties. According to Godfrey-Smith (1996, p. 30), an internalist explains "one set of organic properties in terms of other internal or intrinsic properties of the organic system," while the externalist explains the "properties of organic systems in terms of properties of their environments." Thus, empiricists (e.g., Locke and Hume) are externalists because their central claim is that "the contents of thought are determined, directed or strongly constrained by the properties of experience" (p. 32), while nativists (e.g., Leibniz and Chomsky) are internalists because their claim is that "there is no way ideas which come into the mind from outside can be formed into beliefs and judgments without the operations of specific internal mechanisms. Inputs will not just coalesce into beliefs" (Godfrey-Smith, 1996, p. 39).

While this treatment of the nativism/empiricism distinction is, for the author (p. 17), "more or less the standard one," she sees it as "clearly inadequate" because "rhetoric aside, *both* empiricists *and* nativists are *both* internalists *and* externalists about the origins of what is in our minds." Nativists concede that "very special sorts of interactions with the environment are necessary for the acquisition of a mental life," while empiricists acknowledge that "were it not for our possession of some rather special inborn equipment, we, like most of the natural world, would have no mental lives at all." In short, both see the mind as "a product of a highly complex *interaction* of the experiential with the inborn."

Godfrey-Smith (1996, p. 51) has acknowledged the author's point, observing that both internalists and externalists alike admit that

"some role" is played by both internal and external factors. However, Godfrey-Smith has reported that there are nonetheless differences between the two positions; namely, that the nativist accords a larger role to innate factors, while the empiricist sees experience as more important. While the author (pp. 21–22) views this clarification of the differences between the two schools as "more nuanced" and "seductive" than the previous characterization, she nonetheless concludes that, in the end, it is not helpful because it misses the "deeper things at issue between nativist and empiricist" (p. 22). For the author, these "deeper things" (p. 22) become manifest by examining the nativist's "negative" arguments—arguments that are negative in the sense that they try to establish nativism by denying empiricism (p. 25). To that end, the author observes that "one central means" (p. 31) employed by nativists in this endeavor has been their use of a particular negative argument; namely, the "celebrated" (p. 25) poverty of the stimulus argument (POSA).

In general terms, the POSA asserts that much of what is found "in our minds" is "too rich or too complex" to have originated "from the outside" (the author, p. 25). Therefore, it must be, in some sense, innate. Expressed concisely by Chomsky (1965, p. 58), the POSA states that the "narrowly limited extent of the available data . . . leaves little hope that much of the structure of the language can be learned by an organism initially uninformed as to its general character." On the other hand, the empiricists maintain that the mind possesses general-purpose learning mechanisms (e.g., induction, deduction, and abstraction) which are sufficient to explain our mastery of *any* complex task. 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 experiential input" (the author, p. 32), 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 (the author, p. 49).

The author (p. 151) offers Chomsky's view on language acquisition as a contemporary example of the nativist view that there are domain-specific faculties "for the acquisition of particular kinds of knowledge." In her discussion of Chomskyan theory, she begins with a brief historical overview and then proceeds with a discussion of what she identifies as Chomsky's five core claims. As we shall see, Chomsky's version of the POSA plays a continuing, prominent role in his theorizing.

CHOMSKYAN THEORY: AN HISTORICAL OVERVIEW

The author (p. 151) reports that Chomskyan theory is founded on this "remarkable fact": by roughly eight years of age, normal children master the language spoken in their community. Limitations of vocabulary aside, they understand sentences they have never heard before, and can produce an infinite variety of highly complex, novel utterances. What makes this feat "remarkable" is that it is apparently accomplished without formal instruction, and after being exposed to a relatively small sample of sentences. Given this apparent limited input, Chomsky invokes his POSA to conclude that language acquisition can only be explained by positing a domain-specific language faculty as part of the child's innate endowment.

According to the author, Chomsky's early work (e.g., 1965) characterized the language learner as a *de facto* linguist, developing rule-based hypotheses about a language's syntax and semantics based on the relatively sparse initial data, and then testing these hypotheses against subsequent linguistic input. Given the relative speed and effortlessness with which this is accomplished, Chomsky theorized that the child must innately possess principles—a Universal Grammar (UG)—which constrain and guide her in hypothesizing about the language to which she is exposed. Without these constraints, the number and variety of language hypotheses constructed would be so large as to preclude the quick and easy acquisition in evidence all around us.

Chomsky (1975) defined UG as "the system of principles, conditions, and *rules* [italics added] that are elements or properties of all human languages . . . the essence of language" (p. 29). However, current Chomskyan linguistics theory—known as the principles and parameters approach—has abandoned its long established invocation of rules (for a discussion of this change, see Schoneberger, 2000). For example, Chomsky (1995) has stated that "a language is not . . . a system of rules, but a

set of specifications for parameters in an in-
variant system of principles of Universal Gram-
mar (UG)" (p. 388). Thus, the UG is now seen
as a system of principles and parameters which
describe "the 'initial state' of the language fac-
ulty, prior to any linguistic experience
(Chomsky, 1986, pp. 3–4).

In an attempt to clarify the principles and
parameters approach to the UG, Chomsky has
compared the UG to a "switch box" with a fi-
nite number of switches:

> We can think of the initial state of the fac-
> ulty of language as a fixed network con-
> nected to a switch box; the network is
> constituted of the principles of language,
> while the switches are the options to be
> determined by experience. . . . Each pos-
> sible human language is identified as a
> particular setting of the switches—a set-
> ting of parameters, in technical terminol-
> ogy. (Chomsky, 1997, p. 7)

As with earlier versions of this theory,
Chomsky's justification for positing the innate
possession of principles and parameters rests
largely on the POSA. "The parameters must
have the property that they can be fixed by quite
simple evidence, because this is what is avail-
able to the child" (Chomsky, 1986, p. 146).

CHOMSKY'S LINGUISTIC NATIVISM: THE FIVE
CORE CLAIMS

According to the author (pp. 153–159),
Chomsky's nativist approach to language ac-
quisition embodies five core claims:

1. Representationalism
2. Biological Boundedness
3. Domain Specificity
4. Innateness
5. Universal Grammar

Representationalism

Characterized by the author (p. 154) as "the
father of modern representationalism,"
Chomsky has offered an explanation of lan-
guage acquisition that postulates "contentful
mental states and processes involving their
manipulation." For Chomsky, these represen-
tations play a critical role in the production and
understanding of linguistic expressions.

Biological Boundedness

According to Chomsky (1991), our cogni-
tive capacities have "scope and limits." If this
were not the case, these capacities "would
achieve nothing in any problem situation" (p.
40). As the author (p. 154) expresses it, "In
virtue of the inborn structure of the human
mind, there are constraints on the space of
thinkable thoughts." In terms of the principles
and parameters theory, this means that there
are biologically determined limits on the num-
ber and kind of linguistic parameters that can
be set.

Domain Specificity

For Chomsky, some of the constraints placed
upon our cognitive capacities by our biology
are domain neutral. For example, the author
(p. 155) sees Chomksy as maintaining that "we
cannot entertain a proposition (whether about
language or anything else) that would take
more than a lifetime for a human brain to rep-
resent to itself." Likewise, "our biology limits
our ability to achieve full understanding of the
world." However, in addition to these general
constraints on our cognitions, there are specific
constraints for each domain. Specifically, for
Chomsky, what we can come to know about a
language is "constrained by principles specific
to the linguistic domain" (the author, p. 155).

Innateness

Simply put, this claim is that the aforemen-
tioned constraints are "in some manner innately
encoded" (p. 155), that they are present as part
of our biological endowment.

Taken together, representationalism, biologi-
cal boundedness, domain specificity, and in-
nateness comprise what the author (p. 155) calls
Weak Linguistic Nativism (WLN). Stated con-
cisely, WLN "is the view that the mind inher-
ently contains a special faculty dedicated to the
task of learning a language." While Chomsky
certainly endorses WLN, he goes beyond its
commitments by "not merely affirming that
task-specific faculty for language exists" but
by also defending "in addition a hypothesis
about the nature of the postulated language
faculty"—the aforementioned UG (the author,
p. 156).

Universal Grammar (UG)

As previously stated, the UG, under current Chomskyan theory, is comprised of principles and parameters operative for all human languages. Then, taking the four components of Weak Linguistic Nativism and adding this additional component, UG, constitutes Chomskyan Nativism (the author, p. 157).

THE POVERTY OF THE STIMULUS ARGUMENT

Chomsky supports the conjunction of domain specificity and innateness by employing the POSA (the author, p. 172). This is critical to Chomsky's theory. Without domain specificity and innateness adequately supported, Weak Linguistic Nativism fails, taking with it Chomskyan nativism as well. Indeed, Universal Grammar entails domain specificity and innateness. If either is false, then, by *modus tollens,* UG is false. Thus, the POSA plays a critical role in supporting both Weak Linguistic Nativism and, more importantly for Chomsky, his linguistic nativism as well.

According to the author, there have been three versions of the POSA offered in support of Chomskyan nativism. The first, taking the author's lead, I name the *a posteriori* POSA. Supposedly empirical in nature, this argument "seeks to show that since language could not in fact be learned from the evidence available in the 'primary linguistic data,' the principles of Universal Grammar must be known innately" (p. 177). The second argument— dubbed by me the *logical problem* POSA— relies little on empirical assumptions, is primarily *a priori* in nature, and is frequently referred to as the "Logical Problem of Language Acquisition" or the "No Negative Evidence Problem." This second argument "seeks to show that language could not in point of logic be learned from the available data, and hence, again, that knowledge of Universal Grammar must be innate" (p. 177). And, finally, there is a third argument—which, again taking the author's lead, I name the *iterated* POSA. According this last argument, whatever else the language learner may be able to acquire from the environment in terms of data specific to her target language, the principles and parameters comprising the UG are not available there. Therefore, the knowledge of UG is innate.

The A Posteriori Poverty of the Stimulus Argument

Although all five core claims are constitutive of Chomskyan nativism, particular attention has been paid by his critics to domain specificity, innateness, and universal grammar.

In what the author (p. 179) calls "one of the earliest philosophical examinations of Chomsky's nativism," she reviews (pp. 179–182) Putnam's (1971) critique of Chomskyan theory, as well as Chomsky's (1975) response. Putnam (1971, p. 138) argued that Chomsky's positing of a domain-specific language faculty is "utterly unfounded." Indeed, according to Putnam, the evidence Chomsky offers is compatible with the opposing view; namely, that language learning is the result of "the strategies that make general learning possible" (p. 139). Consider linguistic universals. For Chomsky, their existence supports the tenet of domain specificity. However, Putnam counters that any organism having limited capacity for memory will resort to using recursive rules when the knowledge domain is (like language) infinite. And any organism preferring simplicity over complexity will prefer grammars employing phrase-structure rules accompanied by transformations. Simply put, the existence of linguistic universals can be explained, in part, by the fact that human brains "are computing systems and subject to the same constraints that affect all computing systems" (p. 135).

Chomsky has also argued for domain specificity on the grounds that, with the exception of some of the mentally retarded, the mastery of language is generally independent of one's IQ. For Chomsky, this independence helps demonstrate the existence of a distinct language faculty. Putnam (1971) has taken issue with Chomsky's claim by noting that there are, in fact, substantial differences among humans in terms of their language skills. Further, Putnam maintained that whatever linguistic competence people do have in common simply demonstrates that any given "normal adult learns what every adult learns" (p. 137), which certainly does not prove Chomsky's nativist assertions. In the end, then, Putnam sees no grounds for positing an innate, specialized language faculty. Of course, "human 'innate intellectual equipment' is relevant to language learning" (p. 134); for example, in learning a language we employ memory and intelligence as part of

our general-purpose learning strategies. But, for Putnam, evidence is lacking in support of Chomsky's insistence that there must be a specialized language faculty.

Chomsky responded to Putnam by offering a version of the *a posteriori* POSA. He begins with Putnam's contention that, as part of our general learning strategies, we will show a preference for simplicity over complexity when developing hypotheses about the grammar of our language. Consider the prospect of learning the rule for forming polar (i.e., yes/no) questions. Suppose a child hears, over time, someone say (1) "Ali is happy"; (2) "Is Ali happy?"; (3) "That man can sing"; and (4) "Can that man sing?" Given this linguistic data, here are two possible hypotheses that the child might form regarding the rule of question formation:

H_1 (structure-independence): Find the first occurrence of a verb and move it to the front of the sentence.

H_2 (structure-dependence): Find the first occurrence of a verb following the subject noun phrase and move it to the front of the sentence.

Speaking in accordance with H_1 (structure-independence) would yield the grammatically correct questions (2) and (4) above. But consider this sentence: The man who is happy is singing. Here following H_1 produces the grammatically incorrect question: Is the man who happy is singing? On the other hand, following H_2 (structure-dependence) yields the grammatically correct sentence: Is the man who is happy singing? In other words, by following H_2, the child picks the first occurrence of a verb which follows the subject noun phrase *the man who is happy* and moves it to the front of the sentence to yield the correctly formed question.

For Chomsky, the primary linguistic data is too meager to provide the child the grounds for rejecting H_1. And yet she does not, according to Chomsky, ever make the errors which would result from following H_1. How does one explain this? Chomsky's explanation is that the child, in fact, *never* entertains H_1, so she is in no need of evidence to falsify it. However, if Putnam is right that language learning is the result of general learning strategies—which show a preference for simplicity over complexity—then the child should show an initial preference for the simpler hypotheses, H_1. But she does not, as evidenced by her lack of making those errors consistent with H_1. Therefore, lan-

guage is not acquired by means of general, multipurpose learning, but rather by a specialized faculty. According to Chomsky (1975), "The only reasonable conclusion is that . . . the child's mind . . . contains the instruction: Construct a structure dependent rule, ignoring all structure-independent rules." Further, this rule "is not learned, but forms part of the conditions for language learning" (p. 33).

Chomsky's argument rests, in part, on his claim that H_1 is simpler than H_2. Utilizing "any reasonable standards," H_2 "is far more complex and unlikely than" H_1 (Chomsky, 1975, p. 32). Chomsky's rationale for asserting that H_1 is simpler hinges on its being structure-independent. A structure-independent rule "involves analysis into words and the property 'earliest' ('leftmost') defined on word sequences." On the other hand, a structure-dependent rule "involves analysis into words and phrases, and the property 'earliest' defined on sequences of works analyzed into abstract phrases. The phrases are 'abstract' in the sense that neither their boundaries nor their categories (noun phrases, verbal phrase etc.) need be physically marked" (p. 32).

Interpreting Chomsky, the author (p. 189) stated that what apparently makes a structure-independent rule simpler is that it is "stated in terms of observables" (e.g., picking the leftmost verb) while structure-dependent rules are "framed in terms of unobservables" (e.g., picking the first verb occurring after the abstraction "noun phrase"). However, according to the author, this characterization flies in the face of Chomsky's critique of American structuralism and behaviorism. For Chomsky, "grammatical hypotheses that advert merely to observables tend to be less simple, less elegant . . . than those framed in terms of unobservables" (the author, p. 189). On these grounds, a general-purpose learning mechanism—with its apparent penchant for simplicity—would demonstrate a preference for H_2, not H_1—thus obviating the need to postulate a special faculty for learning language.

The author considers a second argument Chomsky offers to support his claim that using general-purpose strategies would result in picking H_1. As previously pointed out, picking H_2 requires that the learner must be able to form representations about a sentence's syntactic properties (e.g., noun phrases). However, according to Chomsky (1975), syntactic proper-

ties are "only remotely related to experience by long and intricate chains of quasi-inferential steps" (p. 32) and are thus not presented to the learner in experience. Therefore, picking H_2 requires a special faculty innately present in the learner. In response, the author notes that the claim that syntactic categories are unlearnable has a long history of controversy.

The author (pp.190–191) first considers the "Motherese" hypothesis (e.g., Snow & Ferguson, 1977; Newport, Gleitman, & Gleitman, 1977). According to this view, mothers "pause between words, phrases, and (especially) sentences, thereby helping the child to segment the acoustic signal into syntactically salient portions" (p. 190). Mothers also reportedly "exaggerate the rising terminal intonation on questions and imperatives" (p. 190) thereby helping the child distinguish these sentence types from declarative sentences. As a result of these and other features of Motherese, the child is supposedly aided in achieving eventual mastery of syntactic categories.

The author reports that the Motherese hypothesis appears to have lost much of its popularity. However, for her, its significance lies not in its claims about the special significance of child-directed, maternal speech, but rather "in its assumption that children can use statistical information about the input stream in order to abstract syntactic concepts from it" (p. 190). For example, the author cites "a stunning series of experiments" reported by Saffran, Aslin, and Newport (1996) that "demonstrate that children as young as eight months can determine where word boundaries occur in a stream of speech by attending to the statistical features of the input to which they are exposed" (the author, p. 191). According to Saffran et al.'s findings, infants appear to utilize statistical regularities evidenced in speech to distinguish between (a) recurring sound sequences which constitute words and (b) less frequent sound sequences spanning word boundaries.

Saffran et al. make particular use of the concept of the *transitional probability* of sequence of sounds. Defined by the author (p. 191), "The transitional probability of a sound sequence x#y is the conditional probability of y given x." For any given language, the sequence of two sounds within a word generally has a higher transitional probability than that occurring across word boundaries. For example, consider the sound sequence pretty#baby.

There is a greater transitional probability from *pre* to *ty* than from *ty* to *ba* (Saffran et al., 1996).

The participants in Saffran et al.'s study were 8-month-old infants being raised in American-English language environments. During the experiments they were exposed to a continuous speech stream of words from an artificial language made up of American-English phonemes. The only cues about word boundaries provided to the participants were differences in the transitional probabilities occurring between syllable pairs. Specifically, the transitional probabilities were higher within words than they were between words. After only two minutes of exposure to synthesized speech stimuli, the participants' showed significant discrimination between words and non-words.

As a result of Saffran et al.'s study, as well as the works of others (e.g., Braine, 1971; Wanner & Gleitman, 1982; Maratos, 1982; Read & Schreiber, 1982; Sampson, 1989; Pullum, 1996), the author finds it plausible that children can learn syntactic categories by means of general learning mechanisms. Thus, to return to the issue of structure-dependent hypothesis H_1, the author concludes that there is "good evidence" that children "are perfectly able to acquire the 'abstract' syntactic concepts that they need to form such hypotheses through statistical analysis of the speech they hear around them" (the author, p. 193).

In further evaluating the *a posteriori* POSA argument, the author reviews several others who argue that Chomsky is wrong when he claims that sufficient evidence of particular grammatical structures is not present in the primary linguistic data. For example, consider again the structure-dependent rule (see H_2 above). Sampson (1989) has pointed out that near the beginning of a "Wonder Questions" list in a children's encyclopedia he inspected, there appears a yes/no question which supports H_2 over H_1. Similarly, Pullum (1996) conducted a computer-based survey of the *Wall Street Journal* corpus and, within the first five hundred questions, found several that disconfirm the structure-independent rule. He found additional disconfirming evidence in a much shorter source, Wilde's *The Importance of Being Earnest*.

While conceding that these two sources—the *Wall Street Journal* and Wilde's play—are unlikely to be a child's major source of English grammatical constructions, Pullum argues

nonetheless that they may be viewed as representative of the primary linguistic data of English. Extrapolating from this data suggests that "the utterance tokens that could provide the crucial data apparently make up something between 1% of interrogatives and over 10% of polar interrogatives in running text" (Pullum, 1996, p. 508). Further, during language acquisition "a child would hear hundreds of thousands of questions" and therefore she "must hear thousands of examples that crucially confirm . . . structure-dependence" (p. 508).

The author finds the claims of Sampson and Pullman congenial. She states, "If these claims are valid, . . . there need be no mystery about how a data-driven or general-purpose learner would learn the correct rule for forming yes-no questions." Indeed, "if Pullman's right about the frequency of the appropriate forms, there's a good chance that one of the next hundred or so questions the learner hears will be a sentence that can do the trick" (p. 187). Of course, she admits that Sampson and Pullman's findings are "merely suggestive" and that "We still lack the data we need to settle this question" (p. 187). Nonetheless, based on these findings, she argues that "the stimulus is not in fact as impoverished as Chomsky would have us believe" (p. 196).

In addition to Chomsky, the author also examines other uses of the *a posteriori* POSA within nativist literature. For example, she examines the work of Crain and Thornton (1991), who hypothesize that there are innate constraints on contractions within American English. As an example of a contraction, speakers often say "wanna" instead of "want to," as in "Who do you wanna kiss?" However, while these contractions are rampant, there are also instances where they are forbidden. For example, speakers do *not* use the contraction "Who do you wanna kiss you?" in place of "Who do you want to kiss you?" Similarly, some forms of the *is* contraction are permitted (e.g., "Do you know what that's doing up there?") while others are not (e.g., "Do you know what that's up there?) (the author, pp. 197–198).

According to Crain and Thornton (1991), the foregoing constraints on contractions are observed by two year old children.

Crain (1991, p. 603) finds it "difficult to see" how such knowledge "could have been acquired through exposure to environmental in-

put at any age. . . . The logic of the situation would suggest that they must know it innately." Specifically, lacking corrective feedback (as Crain assumes) means that "children who make the false generalization would not be informed of their mistake, and would not attain the adult grammar." However, "it seems that children never make this error" and so do not need the missing corrective feedback. Instead, they rely on "assistance from innate linguistic principles" to master the grammar of their language" (p. 603). Crain finds further evidence of innately acquired constraints in the form of "rules governing the coreference of noun phrases, pronouns, and other referring expressions . . . within sentences" (the author, p. 199).

The author responds to Crain by citing some of the latter's critics. For example, she quotes Powers' (1991, p. 630) observation that "The problem of proving that language learning cannot be or have not been learned from the available input . . . requires an analysis of all input (and its order of presentation) and a formal result that no learning of language from these data is possible." Consonant with Power's observation, Berman (1991), in referencing her own work, reported that a close inspection of the linguistic input provided by the parents of two year old children reveals that it is not as impoverished or as uniform as Crain suggests. Further, Sokolov, and Snow (1991) impugn the supposed universality (and thus, the innateness) of the constraints Crain identifies by noting that in Crain's own research he documents instances of some children making the errors he says do not occur.

The author concludes her discussion of the *a posteriori* POSA by reviewing the two major criticisms of it she has offered. Ironically, the first criticism flows (according to the author) from the argument's "great virtue" which is "its insistence that the truth or falsity of nativism about language learning is a thoroughly empirical matter" (p. 203). This willingness to let argument "stand hostage to the vagaries of empirical fact" has resulted in its being "seriously undercut." Why? Because the argument's supporting data is "as impoverished as it alleges the data for language-learning to be" (p. 203). For the author, the proponents of this argument have failed to provide adequate evidence in support of their "intuitions" that the primary linguistic data is impoverished. Thus, she sees these intuitions as "at worst

outright false, and at best highly dubious." She concludes that the *a posteriori* POSA "is impotent to establish any form of nativism about the faculties responsible for language learning" (p. 177).

The author's second criticism faults the proponents of the *a posteriori* POSA for doggedly underestimating "the resources available to the empiricist learner" while overestimating "the difficulty that such a learner would have in acquiring knowledge of syntactic rules" (p. 204). In particular, she is critical of their failure to recognize the "explanatory power" of a version of empiricism she calls "enlightened" empiricism. Like the nativists, enlightened empiricists find it plausible that "something like a principle of structure dependence is constraining the learner's choices between the two rules of question formation," H_1 and H_2 (see earlier discussion). Thus, they concede that "learners' grammatical decisions at a given moment will be strongly conditioned by their prior linguistic knowledge." However, enlightened empiricists part company with nativists when the latter insist that the learners' prior knowledge is innately endowed. Rather, they see it as "arising out of their earlier experiences, linguistic and otherwise, and the cognitions based thereupon" (p. 205).

The Logical Problem Poverty of the Stimulus Argument

The author characterizes the debate over the *a posteriori* POSA as "deeply unsatisfying" because it has resulted in an impasse. When empiricists offer evidence suggesting that a particular grammatical rule can be learned from the primary linguistic data using general-purpose learning strategies, nativists are usually not convinced to thereby reject nativism. Rather, one of their strategies is simply to identify "different grammatical rules and principles, that are again claimed to be unlearnable" from the data provided by the environment (p. 197). Of course, the empiricists then often counter with further evidence suggesting that these newly offered rules and principles are also available in the primary linguistic data and learnable by means of general-purpose learning.

Perhaps in an attempt to avoid a prolonged, potentially endless battle over the *a posteriori* POSA, "nativists have recently shifted their

argumentative emphasis . . . toward another type of poverty of the stimulus argument" (the author, p. 205). Variously labeled as the "Logical Problem of Language Acquisition," the "Projection Problem," and the "Negative Evidence Problem," this argument endeavors to "cut through the miasmic uncertainty surrounding the outcome of the *a posteriori* argument and to establish decisively that nativism about language learning must be true"(p. 205). This "more *a priori* variant of the poverty of the stimulus argument"—hereafter referred to as the *logical problem* POSA—purports to show that the available data "are impoverished not merely with respect to the acquisition of some particular grammatical rule but with respect to the acquisition of *any* grammar powerful enough to generate a natural language." In short, it aspires to demonstrate that "the primary data are impoverished not just in fact but in principle" (p. 205).

Consider the following example. A language learner hears others utter the sentences *It is likely that John will leave, John is likely to leave,* and *It is possible to leave.* Given this data, the learner could easily conclude that *John is possible to leave* is a sentence. However, according to proponents of the logical problem POSA, "competent speakers of English universally judge that construction to be ungrammatical" (the author, p. 206). Why? One empirically based explanation is that when they utter this (or other, similar) putative sentences, they are provided "negative evidence"—information indicating that a particular strings of words are not well-formed sentences in the target language. On the other hand, proponents of the logical problem POSA assert that the language learner encounters almost no "negative evidence" in the available data—and that the little encountered is not beneficial to learning—thus giving rise to the logical problem POSA.

The author enumerates a number of reasons given in support of the assertion that there is no negative evidence available to the learner. First, competent speakers obviously do *not* provide learners with a list of ungrammatical sentences. Second, research evidence (most notably, Brown & Hanlon,1970), allegedly indicates that learners typically do not have their ungrammatical utterances corrected. Third, because there are an infinite number of well-formed sentences which the learner never encounters, "the mere nonoccurrence of a string

in the data cannot by itself constitute negative evidence" (the author, p. 208).

Further, even granting the presence of some negative evidence, there are still problems with asserting that it plays a role in language acquisition. First, consider the individual learner. Given the assumption that negative evidence would occur in a haphazard fashion, "there is no reason to expect that she will chance upon just the negative evidence she needs to discover her mistake" (pp. 212–213). And even if a learner typically makes numerous errors and has each of them corrected, and even if she were only exposed to well-formed sentences, there would remain an infinite number of well- and ill-formed sentences about which she would never obtain information. Finally, consider the totality of learners in a language community. "How can we guarantee that everyone gets exactly the evidence he or she needs to arrive at the same grammatical hypothesis as everyone else?" (p. 212).

Nativists assert domain specificity and innateness as a solution to the logical problem. First, by asserting domain specificity, nativists "may hope to ensure that learners do not project hypotheses requiring negative evidence for their disconfirmation, thus rendering the learnability problem in the individual case tractable" (p. 212). Second, by asserting innateness, nativists thereby presumably explain why virtually the totality of language learners in a specific language community—despite their differing linguistic experiences—nevertheless acquire the grammar of their native language. Thus, by asserting domain specificity and innateness, nativists avoid the empiricists' problem of explaining "how it is that learners manage to err only in ways that turn out to be rectifiable on the basis of the paltry and haphazard data" (p. 213) available to them.

The author asserts that the foregoing nativist argument, though "compelling," is nonetheless "flawed" (p. 215). The logical problem, far from characterizing only a limited number of specific knowledge domains like language, is instead "a completely general problem arising for all learning involving projection beyond our experience. There is . . . a dearth of negative evidence not just in the domain of language but in every domain in which people learn" (p. 215). So, if we assume that a lack of negative evidence is sufficient grounds for adopting domain specificity and innateness, then "we

should 'go nativist' about everything. But this is untenable. The nativist's argument, therefore, must be invalid" (p. 215).

To make her case, the author asks us to consider how we come to learn about food. Regardless of our differing, often limited, culinary experiences, almost all still achieve "culinary competence"; namely, "the ability to recognize and distinguish a variety of foods from each other and from nonfoods" (p. 215). Consider the specific task of learning about curries. "No one ever systematically informs us that Irish stews, tacos, and quiches—let alone boats and babies and bisons—are not curries" (p. 215). Yet "despite the dearth of negative evidence, we all manage to converge on the view that a curry is itself and not another thing" (p. 215).

For the author, this argument provides grounds for embracing enlightened empiricism—i.e., embracing domain specificity while rejecting innateness. If our knowledge of food is guided only by general learning principles, we might overgeneralize that "All the world's a curry." Given the apparent unavailability of sufficient corrective feedback to reign in this overgeneralization, we must come to possess domain specific knowledge "constraining our choices of curry hypotheses" (p. 215).

However, from the fact that there exists such domain specific knowledge—e.g., that we know "that curries are a kind of food, or that they have a characteristically spicy taste" (p. 215)—it does not follow that such knowledge is innate. With respect to language learning, the nativist argues that despite individual differences in linguistic experiences, virtually all language learners in a given community converge on the same, correct, language hypothesis. For the nativist, this supports their positing of innateness. The author asks, with respect to culinary competence, "Are we then to accept the same inference here?" and answers "Surely not!" As the author expresses it, "all normal people exposed to a curry or two arrive at more or less the correct view about what curries are." And yet, "It is just absurd to suppose that the domain-specific principles required for learning about curries are innate, biologically encoded in a special 'culinary faculty'!"(p. 215).

In making this point, the author is at pains to emphasize that her "aim here is emphatically not to suggest that nativism about language is

implausible in the way that nativism about curries is" (p. 216). The two domains differ considerably in terms of when and how they are acquired (among other differences), so we would also expect the implausibility of innateness in each case would be argued on different grounds. Rather, her purpose is to argue that while it is "miraculous and mysterious" that "humans learn an awful lot, about a bewildering variety of topics, from sketchy and largely positive data," this is not "a reason to accept a nativist explanation of the miracle or solution to the mystery" (p. 216). Indeed, since the logical problem with respect to culinary competence "is so clearly a sham suggests that the Logical Problem of Language Acquisition may be something of a pseudo-problem too" (p. 216). Thus, if we are disinclined to assert the existence of an innately culinary faculty to explain culinary competence, we should be dubious about positing an innate faculty to explain the acquisition of language.

Further, a closer examination of the curry example makes clear that "there is much more negative evidence around than a proponent of the Logical Problem would allow." Indeed, for the student of curries, there is a "vast quantity of indirect or implicit negative evidence about curries available. For example, the fact that we call hamburgers 'hamburgers,' not 'curries' is surely evidence" (p. 216). The presence of such negative evidence for learning about curries suggests that we take a hard look at the nativist's claim that negative evidence is lacking in language learning. "Just as there are many sources of negative evidence in the data concerning curries, so there must be substantial sources of negative evidence in the data concerning language" (p. 222). To this end, the author offers three sources of negative evidence available to the learner.

Before discussing these sources of negative evidence, the author notes that thus far she has been using the terms *data* and *evidence* as equivalent in meaning. However, she now elects to use *data* to mean "the facts as they are presented to experience," and *evidence* to mean "those facts as they bear on the (dis)confirmation of some theory" (p. 222). The purpose of making this "admittedly vague" distinction is to emphasize that negative *data* can serve as *both* positive *and* negative *evidence* (as can positive *data* as well). To clarify, consider the case of negative *data*. To reiter-

ate, negative *data* are facts "presented to experience" (p. 222)—e.g., in the form of explicit disapproval—"to the effect that such and such a string of words is not a sentence of the language he is learning" (pp. 222–223). If the language learner's hypothesis regarding her language predicts that this string of words *is* a sentence, then the negative *data* serves as negative *evidence* for the hypothesis. On the other hand, if the learner's hypothesis correctly predicts that this string is *not* a sentence, then the aforementioned negative data serves as positive evidence for the hypothesis.

Using this data/evidence distinction, the author proffers three sources of negative evidence in the primary linguistic data: 1) negative *data;* 2) positive *data;* and 3) nonoccurrence. Consider each in order.

Negative data as negative evidence. The claim that negative data are virtually unavailable has been "the mainstay of the argument from the Logical Problem" (p. 227). To support this claim, nativists almost always cite one particular study: Brown and Hanlon (1970). Based on their study of the verbal interactions of three mother/child dyads, Brown and Hanlon concluded that "parents' explicit expressions of approval and disapproval did not correlate with the syntactic well-formedness of what their children said" (the author, p. 228). Most members of the linguistics community took Brown and Hanlon's findings "to demonstrate the nonexistence and irrelevance of negative data *tout court*" (p. 228).

The author notes that despite the widespread acceptance of the "no negative evidence" thesis within these communities, a few raised serious, research-based objections. For example, Hirsh-Pasek, Treiman, and Schneidermann (1984) found that two year olds had their ill-formed utterances repeated by their mothers with significantly more frequency than their well-formed sentences and, further, that these repetitions also corrected the child's error. Hirsh-Pasek et al. (p. 81) concluded that "the language learning environment does present subtle cues that distinguish between well-formed and ill-formed sentences."

The author also briefly summarizes the research of Demetras, Post, and Snow (1986) and Bohannon and Stanowicz (1988) which also provides evidence that feedback to children is provided differentially for their ill-formed vs. well-formed utterances. Further, the author

cites Moerk (1991) whose reexamination of Brown and Hanlon's original data she views as "significant in that it demonstrates that even Brown's own transcripts reveal an abundance of corrective feedback in the primary linguistic data" (p. 230).

After reviewing this research demonstrating the existence of negative data as negative evidence, the author observes that the Brown and Hanlon (1970) study appears to be "an outdated shibboleth" and that the subsequent, aforementioned research "confirms what any parent not personally involved in debates about learnability will tell you, namely that it is simply false that parents do not correct their children's ungrammatical utterances" (p. 230). However, before finishing her discussion of this topic, the author presents, and then responds to, some of criticisms leveled at the aforementioned research.

The first criticism she examines is that provided by Morgan and Travis (1989). They examined Brown's original data, generating findings in general agreement with the aforementioned researchers. For example, Morgan and Travis found that, for all the children in the study, expansions followed ungrammatical utterances with greater frequency than they followed grammatical utterances. However, though their findings were largely in accord with the above-noted researchers, Morgan and Travis reached a quite different conclusion, stating that while "some parental responses may sometimes supply the perspicacious child with correction, we fail to see sufficient evidence to warrant the conclusion that language input generally incorporates negative information" (p. 551). According to them, for negative evidence to be effective, it must be supplied with much greater frequency and over a longer time. Specifically, negative evidence can benefit the child only when "for each type of overgeneralization error the child makes, the child is systematically provided with some recognizable form of correction" which "continues until the child succeeds in remedying that error" (Morgan & Travis, 1989, p. 535).

The author counters Morgan and Travis by opining that the conditions they posit as necessary for the effectiveness of negative evidence appear "much too strong." Specifically, in response to the requirement that each type of overgeneralization error must be systematically corrected if negative evidence is to be deemed effective, the author responds: "That one type of error goes unremarked is surely no reason to think that feedback is not involved in the correction of other types of error" (p. 232). In supporting her point, the author quotes Demetras, Post, and Snow (1986, p. 287), who argue that "the failure of occurrence of negative feedback to some [ill-formed] utterances presents a serious problem to the child only if it is assumed that the child is working on acquiring the entire grammatical system at once." And to Morgan and Travis's insistence that correction, to be effective, must persist until the error vanishes, the author responds: "The fact . . . that the information provided by feedback *alone* could not and does not suffice to falsify a rule, does not show that feedback plays no disconfirmatory role at all" (p. 232).

In addition to the critique of Morgan and Travis, the author summarizes that of Marcus (1993). The author begins by reporting Marcus' observation that children with differing familial and cultural backgrounds receive diverse types and amounts of feedback. Yet, despite these differences, virtually all children master their native tongue. Thus, for Marcus, there appears to be no evidentiary support for claiming that feedback is required for language mastery. Second, for Marcus, the feedback that is provided the learner does not constitute definitive evidence of a string's grammaticality unless "extremely unrealistic assumptions are made about the learner's linguistic behavior and learning methods" (the author, p. 231). For example, based on the type of feedback reported in Bohannon and Stanowicz (1988), Marcus contends that a learner would need to repeat any given string 85 times (and receive feedback as described by Bohannon and Stanowicz) to reach 99 percent certainty regarding its grammatically. However, since children become reasonably adept at grammatical well-formedness without such repetitions, Marcus concludes that the role played by feedback is not crucial.

The author responds to Marcus by first noting that just because some children receive a paucity of feedback "does not indicate those who . . . receive it do not exploit it in their acquisition of language" (p. 232). Indeed, "there is no reason to insist that all children must make use of the same sorts of evidence to the same extent" (p. 232). The author finds support for her view from others. For example,

MacWhinney (1989, p. 99) states that "the presence or absence of any particular support for language learning is not critical" and Nelson, Denninger, Bonvillian, Kaplan, and Baker (1984, p. 47) that "there are *many* components of input for most children that are not necessary for their syntactic growth."

The author also takes issue with Marcus' second criticism. While agreeing that children are unlikely to gauge a given *string's* grammaticality by uttering it 85 times, the author does find it plausible "to think that she might repeatedly use a given *structure* or *rule,* and use the feedback to judge the appropriateness of that structure or rule" (p. 233). In other words, language learners "may achieve a high degree of certainty as to the appropriateness of a given string not by repeating the string itself, but by . . . using the structure it instantiates" (p. 233).

In concluding her retort to Morgan and Travis (1989) and Marcus (1993), the author reiterates that the research indeed shows that "*negative data*—explicit information as to what sentences are not—*do exist* in the child's environment" (p. 234). Thus, the logical problem is resolved without invoking nativism. What remains, then, is the "empirical question" of showing in greater detail "*how* children manage to make use of the negative data that feedback provides" (p. 234).

Positive data as negative evidence. Based on Pinker (1986), the author offers what the former calls *constraint sampling* as a method for utilizing positive data to correct grammatical errors. In constraint sampling, the learner is viewed as randomly picking one feature of a sentence appearing in the primary linguistic data and then using that feature as a constraint. Consider the following example of how a language learner might come to learn to attach the suffix s to a verb stem when forming a declarative sentence. Assume that the learner hears the sentences *The boy wants a curry* and *Dad wants a beer,* yielding the general rule that a verb stem should always have an *s* attached to it. According to the author's account of constraint sampling, the learner would immediately hypothesize a constraint of this rule—perhaps, that one adds *s* when the subject of the declarative sentence is animate. This rule would be falsified by exposure to a sentence such as *The curry tastes good,* at which point the learner might hypothesize that *s* is used for the present tense.

Here the learner used positive data—exposure to the sentence *The curry tastes good*—as negative evidence to correct her earlier overgeneralization. In this manner a learner ultimately forms a rule with the appropriate constraints (the author, p. 226). Of course, unlike Pinker (who is a nativist), the author sees constraint sampling and constraint setting as a product of prior experience.

Nonoccurrence as negative evidence. According to the author, some (e.g., Baker, 1979; Lightfoot, 1982; Pinker, 1986), in deliberating on the logical problem POSA, argue that since most sentences in any natural language have not been (and never will be) spoken, a language learner cannot use the nonoccurrence of a particular string of words as evidence that it is not a sentence. On this view, a learner has no way of discerning whether the nonoccurrence of a particular sequence of words is "due to its not being a sentence or whether . . . no one has had occasion to utter it yet." Taking a contrary position, the author argues that in many instances "the nonappearance of a string in the primary data can legitimately be taken as constituting negative evidence" (p. 223).

Consider a language learner who embraces the view, reportedly common among preschoolers (Pinker, 1986, 1994), "that all intransitive verbs can be used as causatives" (the author, p. 223). For example, in addition to uttering grammatically correct sentences such as *I melted it,* this learner says, incorrectly, *I giggled her* when she means that she caused her to giggle. Now suppose this learner sees her father knock his coffee off the table. If her father speaks on the matter (e.g., when asked by his wife what happened), the learner expects to hear *I falled the cup off the table* (or some stylistic variation thereof). Instead, she hears her father say *I caused the cup to fall from the table.* The nonoccurrence of *I falled the cup off the table* provides negative evidence for her faulty rule that all intransitive verbs can serve as causatives (the author also notes that hearing *I caused the cup to fall from the table* constitutes positive data serving as negative evidence for the faulty rule).

In addition, the author also reports what is another, "more interesting" (p. 224) example of nonoccurrence serving as negative evidence. Consider these two strings: (1) *Steve enjoyed the curry* and (2) *Enjoyed curry Steve the.* According to the author, the nonoccurrence of the

first string does not constitute negative evidence that it is ill-formed, while the nonoccurrence of the second string *does* constitute evidence of its ill-formedness.

In explaining this distinction, the author first differentiates between a string *qua string* and a string *qua instance of a particular syntactic structure*. For any given syntactic structure, there are many strings to which a learner has been exposed. So, although *Steve enjoyed the curry* may not have been uttered in the learner's presence, many other strings exhibiting its structure (e.g., *Bruce loved the movie*) have so occurred. Hence, the nonoccurrence of any particular string is not evidence that it is ill-formed, since many other strings with its structure are part of the learner's primary linguistic data. This is *not* the case, the author argues, for the nonoccurrence of *Enjoy the curry Steve the.* For the author, "what makes it negative evidence is not merely its nonoccurrence qua string: what makes it negative evidence is its nonoccurrence qua instance of a particular syntactic structure" (p. 225).

The "Iterated" Poverty of the Stimulus Argument

By acknowledging domain-specific skills, but denying that they are innately known, the enlightened empiricist appears to be committed to the proposition that the Universal Grammar (UG) is "learned from experience." In particular, she appears to hold the view that the UG is learned "prior to children's learning any particular grammar for their language." How is this possible? For the nativist, it is not possible. As the author expresses the nativist position, a child who has yet to acquire a language "is in no position to formulate, let alone test" the principles that regulate "languages in general." Rather, the acquisition of a specific language presupposes knowledge of these general principles. However, this information is not available in the environment in a form accessible to a pre-linguistic child. Therefore, the UG must be innately given. So goes the nativist argument that the author dubs the "'iterated' argument from the poverty of the stimulus" (the author, p. 239).

For example, within Chomsky's principles and parameters approach, the UG provides knowledge of the what is called the *pro-drop parameter.* Specifically, prior to learning a language, the child somehow already knows that some languages allow sentences lacking apparent subjects (*null-subject* languages), others (*non-null-subject* languages) do not. English is an example of the former, Italian the latter. Thus, the Beatles' line "I am the walrus" is, in Italian, "Sono il tricheco" (translation: am the walrus) (Cook & Newson, 1996). According to Chomskyan theory, after limited exposure to English, the language learner sets the pro-drop parameter "switch" one way, the Italian language learner sets it the other way. *How* the parameter is set then governs a learner's future grammatical constructions. "When these switches are set, the child has command of a particular language" (Chomsky, 1988, p.63).

According to the author (p. 241), Chomsky takes the position that his assertion of the UG "can be made true by fiat"—that a description of the UG "just is"(p. 240) a factual description of the initial state of a human being's language faculty. The author, however, raises serious objections to Chomsky's assertion that the UG exists, and thereby objects to the iterated poverty of the stimulus argument. Consider again the pro-drop parameter. The author cites Hyams (1983) as observing that virtually all children initially speak as if their language is a null-set language. For example, early on an English speaking child will say, "Want cookie" instead of "I want a cookie." If they never receive negative evidence for such constructions—as assumed in Chomskyan and other nativist theories—then how do they come to set the correct parameter? Is the correct setting triggered by the positive data available in adult speech? If that is the claim, then UG proponents have to account for the fact that much of the positive data in English contains null-set sentences (Berman, 1990). The author offers the following examples: *Couldn't give a damn, Wouldn't believe a word he said,* and *Must have been the mailman* (p. 255). If the existence of the UG is questionable, then, for the author, an empiricist is not burdened with having to explain how it is acquired.

CONCLUDING COMMENTS

In the title of this review, I refer to Cowie's "war" on poverty of the stimulus arguments. Hyperbole aside, my purpose was to emphasize the no-holds-barred nature of the disagree-

ment between the author and some linguistic nativists. This conflict is particularly well represented in the polemical style exhibited by both sides. For example, the author asserts that the POSA "does *nothing* [italics added] to brace the nativist position on language acquisition" (p. 276) and is "polemically impotent" (p. 203). In addition, in a paper appearing online, the author reports that, in response to her criticism of POSA, its proponents "have done nothing beyond thumping the table and saying it again" (Cowie, 1999/2000, ¶ 6).

Of course, the other side has been equally combative in its rhetoric. For example, Fodor's (1999/2000) response to one of Cowie's arguments was a terse "So what?" (¶ 2.2.2) Elaborating, he opined that the arguments she offers "either misconceive the issues or are, in crucial respects, unsound" (Fodor, 1999/2000, Introduction section). Further, he described her version of enlightened empiricism as "empty" (¶ 2.2.3). Similarly, Collins (2003) has characterized Cowie's reposte as "somewhat myopic" (p. 160) and has described her as "the latest in a long wearisome line of philosophers who have sought to challenge the assumptions of the generative program" (p. 187).

In this review I have not added to these criticisms by providing a behavior-analytic critique of her book. Rather, my intent has been to furnish behavior analysts with an exposition of one philosopher's often-elaborate critique of three versions of the POSA. This critique (after a suitable behavior-analytic translation) is largely congenial with a behavior-analytic perspective on language acquisition and thus provides behavior analysis with additions to its armamentarium of arguments. Finding support in other disciplines appears particularly important, given the view—held by many behavior analysts—that behavior analysis is "somewhat beleaguered" (Staddon, 2004, p. 117).

Given this concern, behavior analysis can take its lead from philosopher Richard Rorty (e.g., 1999) who has developed neo-pragmatism by borrowing arguments from others, many of whom (e.g. Davidson, 1984) do not consider themselves pragmatists. The upshot of this approach for Rorty has been, in addition to successfully promoting neo-pragmatism, the encouragement of a productive dialogue with those from whom he has borrowed.

Perhaps the same can happen between Cowie and behavior analysts. Indeed, Cowie's (1999)

statements regarding behaviorism—e.g., "contrary to the behaviorist's contention, training by caregivers is not necessary for children's acquisition of adult syntax" (p. 162)—suggest that she holds some misconceptions about behavior analysis that could change as a result of that dialogue. The product of such exchanges between behavior analysts and Cowie, as well as exchanges with others outside behavior analysis who also offer critiques of nativism (e.g., Elman, Bates, Johnson, Karmiloff-Smith, Parisi, & Plunkett, 1996), could be a less beleaguered behavior analysis—in particular, a less beleaguered behavior analysis of language acquisition.

REFERENCES

Baker, C. L. (1979). Syntactic theory and the projection problem. *Linguistic Inquiry, 10,* 533–581.

Berman, R. A. (1991). In defense of development. *Behavioral and Brain Sciences, 14,* 612–613.

Bohannon, J. N., & Stanowicz, L. (1988). The issue of negative evidence: Adult responses to children's language errors. *Developmental Psychology, 24,* 684–689.

Braine, M. D. S. (1971). On two types of models of the internalization of grammars. In D. I. Slobin (Ed.), *The ontogenesis of grammar: A theoretical symposium* (pp. 153–186), New York: Academic Press.

Brown, R. & Hanlon, C. (1970). Derivational complexity and order of acquisition in child speech. In J. R. Hayes (Ed.), *Cognition and the development of language* (pp. 11–53). New York: Wiley.

Chomsky, N. (1965). *Aspects of the theory of syntax.* Cambridge, MA: MIT Press.

Chomsky, N. (1975). *Reflections on language.* London: Fontana.

Chomsky, N. (1986). *Knowledge of language: Its nature, origin, and use*. New York: Praeger.

Chomsky, N, (1988). *Language and problems of knowledge: The Managua lectures*. Cambridge, MA: MIT Press.

Chomsky, N. (1991). Linguistics and cognitive science: Problems and mysteries. In A. Kasher (Ed.), *The Chomskyan turn* (pp. 26-53). Oxford: Blackwell.

Chomsky, N. (1995). Bare phrase structure. In G. Webelhuth (Ed.), *Government and bind-*

ing theory and the minimalist programme (pp. 383–440). Oxford: Blackwell.

Chomsky, N. (1997). Language and mind: Current thoughts on ancient problems (part 1). Retrieved October 10, 2004, from http://www.kcn.ru/tat_en/science/fccl/papers/chomsky1.htm.

Collins, J. (2003). Cowie on the poverty of stimulus. Synthese, 136, 159–190.

Cook, V. J. & Newson, M. (1996). Chomsky's universal grammar: An introduction (2nd ed.). Cambridge, MA: Blackwell.

Cowie, F. (1999). What's within: Nativism reconsidered. New York: Oxford University Press.

Cowie, F. (1999/2000). Whistling "Dixie": Response to Fodor. Retrieved October 10, 2004, from http://host.uniroma3.it/progetti/kant/field/cowiesymp.htm

Crain, S. (1991). Language acquisition in the absence of experience. Behavioral and Brain Sciences, 14, 597–615.

Crain, S., & Thornton, R. (1991). Recharting the course of language acquisition: Studies in elicited production. In N. Krasnegor, D. Rumbaugh, R. Schiefelbusch, & M. Studdert-Kennedy (Eds.), Biobehavioral foundations of language development (pp. 321–337). Hillsdale, NJ: Erlbaum.

Davidson, D. (1984). Inquiries into truth and interpretation. Oxford: Clarendon.

Demetras, M. J., Post, K. N., & Snow, C. E. (1986). Feedback to first language learners: The role of repetitions and clarification questions. Journal of Child Language, 13, 275–292.

Elman, J. L., Bates, E. L., Johnson, M. H., Karmiloff-Smith, A., Parisi, D. & Plunkett, K. (1996). Rethinking innateness: A connectionist perspective on development. Cambridge, MA: MIT Press.

Fodor, J. (1999/2000). Doing without what's within; Fiona Cowie's critique of nativism. Retrieved October 10, 2004, from http://host.uniroma3.it/progetti/kant/field/cowiesymp.htm.

Godfrey-Smith, P. (1996). Complexity and the function of mind in nature. Cambridge: Cambridge University Press.

Hirsh-Pasek, K., Treiman, R., & Schneidermann, M. (1984). Brown and Hanlon revisited: Mothers' sensitivity to ungrammatical forms. Journal of Child Language, 11, 81–88.

Hyams, N. (1983). The pro-drop parameter in child grammars. Proceedings of the West Coast Conference in Formal Linguistics, vol. 2. Stanford, CA: Stanford Linguistics Association.

Lightfoot, D. (1982). The language lottery: Toward a biology of grammars. Cambridge, MA: MIT Press.

MacWhinney, B. (1989). Competition and teachability. In M. L. Rice & R. L. Schiefelbusch (Eds.), The teachability of language (pp. 63–104). Baltimore: Paul H. Brookes.

Maratos, M. P. (1982). The child's construction of grammatical categories. In Wanner, E., and Gleitman, L. R. (1982). Language acquisition: The state of the art (pp. 240–266). Cambridge: Cambridge University Press.

Marcus, G. (1993). Negative evidence in language acquisition. Cognition, 46, 53–85.

Moerk, E. (1991). Positive evidence for negative evidence. First Language, 11, 219–251.

Morgan, J. L., & Travis, L. L. (1989). Limits on negative information in language input. Journal of Child Language, 16, 531–552.

Nelson, K. E., Denninger, M. S., Bonvillian, J. D., Kaplan, B. J., & Baker, N. (1984). Maternal input adjustments and non-adjustments as related to children's linguistic advances and to language acquisition theories. In A. D. Pellegrini & T. D. Yawkey (Eds.), The development of oral and written language in social contexts (pp. 31–56). Norwood, NJ: Ablex.

Newport, E., Gleitman, H., & Gleitman, L. (1977). "Mother, please, I'd rather do it myself": Some effects and non-effects of maternal speech style. In C. E. Snow and C. A. Ferguson (Eds.), Talking to children: Language input and acquisition (pp.109–149). Cambridge: Cambridge University Press.

Pinker, S. (1986). Productivity and conservatism in language acquisition. In W. Demopoulos and A. Marras (Eds.), Language learning and concept acquisition: Foundational issues (pp. 54–79). Norwood, NJ: Ablex.

Pinker, S. (1994). The language instinct: How the mind creates language. NY: Harper-Collins.

Powers, D. M. W. (1991). Language acquisition in the absence of proof of absence of

experience. *Behavioral and Brain Sciences, 14,* 629–630.

Pullum, G. K. (1996). Learnability, hyper-learning, and the poverty of the stimulus. In J. Johnson, M. L. Luge, & J. L. Moxley (Eds.), *Proceedings of the 22nd annual meeting: General session and parasession on the role of learnability in grammatical theory* (pp. 498–513). Berkeley, CA: Berkeley Linguistics Society.

Putnam, H. (1971). The "innateness hypothesis" and explanatory models in linguistics. In J. Searle (Ed.), *The philosophy of language* (pp. 130–139). London: Oxford University Press.

Read, C., & Schreiber, P. (1982). Why short subjects are harder to find. In E. Wanner & L. R. Gleitman, *Language acquisition: The state of the art* (pp. 78–101). Cambridge: Cambridge University Press.

Rorty, R. (1999). *Philosophy and social hope.* New York: Penguin.

Saffran, J. R., Aslin, R. N., & Newport, E. L. (1996). Statistical learning by 8-month-old infants. *Science, 274,* 1926–1928.

Sampson, G. (1989). Language acquisition: Growth or learning? *Philosophical papers, 18,* 203–240.

Schoneberger, T. (2000). A Departure from cognitivism: Implications of Chomsky's second revolution in linguistics. *The Analysis of Verbal Behavior, 17,* 57–73.

Snow, C. E. & Ferguson, C. A. (Eds.). (1977). *Talking to children: Language input and acquisition.* New York: Cambridge University Press.

Sokolov, J. L., & Snow, C. E. (1991). The premature retreat to nativism. *Behavioral and Brain Sciences, 14,* 635–636.

Staddon, J. E. R. (2004). A call to arms. *The Behavior Analyst, 27,* 117–118.

Wanner, E., & Gleitman, L. R. (1982). *Language acquisition: The state of the art.* Cambridge: Cambridge University Press.