

Nature, Nurture and Universal Grammar

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ABSTRACT: In just a few years, children achieve a stable state of linguistic competence, making them effectively adults with respect to: understanding novel sentences, discerning relations of paraphrase and entailment, acceptability judgments, etc. One familiar account of the language acquisition process treats it as an induction problem of the sort that arises in any domain where the knowledge achieved is logically underdetermined by experience. This view highlights the 'cues' that are available in the input to children, as well as children's skills in extracting relevant information and forming generalizations on the basis of the data they receive. Nativists, on the other hand, contend that language-learners project beyond their experience in ways that the input does not even suggest. Instead of viewing language acquisition as a special case of theory induction, nativists posit a Universal Grammar, with innately specified linguistic principles of grammar formation. The "nurture versus nature" debate continues, as various "poverty of stimulus" arguments are challenged or supported by developments in linguistic theory and by findings from psycholinguistic investigations of child language. In light of some recent challenges to nativism, we rehearse old poverty-of-stimulus arguments, and supplement them by drawing on more recent work in linguistic theory and studies of child language.

1. Introduction

In the normal course of events, children acquire remarkable linguistic capacities in just a few years. After an initial period of rapid linguistic development, children stabilize and henceforth manifest linguistic competence that is equivalent to other members of their community. Linguistic competence includes understanding novel sentences; discerning relations of paraphrase, entailment, and ambiguity; judging that certain strings of words are unacceptable; and so forth. The main task of linguistic theory, within the generative tradition, has been to explain this remarkable acquisition scenario. The details turn on, *inter alia*, (i)-(iii):

(i) the stable states children achieve,

(ii) the linguistic input children receive, and

(iii) the nonlinguistic capacities of children, especially the capacities to form and test generalizations based on their experience.

Nativists hold that (i) is underdetermined by (ii) in theoretically important respects, even given optimistic assumptions about (iii). In recent work, some have accused nativists of underestimating (ii) and/or undue pessimism about (iii) in light of recent findings about the abilities of infants to extract regularities from environmental input. Cowie (1999) presents the most recent challenge, but there have been others.¹ We think these responses to nativists underestimate (i), and thus fail to address the

¹ See Bates, Elman, *et.al* (1996) and the subsequent discussion in *Mind and Language* 13: 571-597 (1998).

strongest 'poverty of stimulus' arguments for innate linguistic knowledge. These arguments are supported both by developments in linguistic theory and a growing body of experimental research on child language. Humans, young and old, exhibit mastery of linguistic principles that are not plausibly learned.

As we stress throughout, the *details* of descriptive linguistics matter: First, one tries to find principles that characterize human grammars; *then* one tries to determine which aspects of these grammars could plausibly be learned from experience, and which are more likely to be innately specified (see Chomsky 1969). There is widespread confusion on this point, perhaps because poverty of stimulus problems are superficially similar to "induction problems" that arise in any domain where knowledge is logically underdetermined by data. But there are important differences between the study of language and the more general study of human cognition. So it is worth rehearsing and supplementing some old arguments (*cf.* Chomsky 1965, 1986; Hornstein and Lightfoot 1981) by way of replying to the "enlightened empiricism" that Cowie and others have suggested. We also review some psycholinguistic investigations of children, which provide independent evidence for innate principles that sharply delimit the space of possible human languages.

From the nativist perspective, children acquire an adult language – *i.e.*, they achieve a stable state – by trying out various linguistic options that are available in human languages; but children never entertain options that extend beyond the boundary conditions imposed by Universal Grammar. This is the *continuity hypothesis* (Crain, 1991; Crain and Thornton, 1998; *cf.* Pinker, 1984). Of course, input matters. Children quickly settle on a system of linguistic principles equivalent to those of adults in the local community. Children in monolingual English environments acquire English, and not Italian or Chinese. But nativists should not be surprised if such children exhibit some German or Romance or East Asian constructions, absent any evidence for these constructions in the primary linguistic data. Indeed, theory-driven mismatches between child and adult language may be the strongest argument for a universal grammar, and against models according to which children construct hypotheses based on linguistic experience. A detailed argument of this kind is described in the final section.

2. Background to Nativist Arguments

We assume that language learning involves the acquisition of a grammar, where a grammar is a cognitive resource that allows its possessor to generate and recognize endlessly many expressions of the language. These expressions can be viewed as structures that pair signals with meanings. Signals are represented, at some level, as strings of words. But as speakers know, many strings of English words are not well-formed expressions of English; and some word-strings can be paired with more than one meaning. For example, (1) is unacceptable; and (2) is structurally ambiguous. One can use (2) to report either that theses are useless for stopping philosophers or that philosophers with theses are unstoppable (*cf. Mary saw the man with binoculars*).

- (1) *I wonder who John hoped that left the party.
- (2) You cannot stop a philosopher with a thesis.

In addition, many strings of words are *unambiguous* in interesting ways. Consider (3).

- (3) John said that he thinks Bill should wash himself.
 - (a) John_i said that he_{i/j} thinks Bill_k should wash himself_k
 - (b) *John_i said that he_{e_{i/j}} thinks Bill_k should wash himself_i
 - (c) *John_i said that he_k thinks Bill_k should wash himself_k

Here the reflexive pronoun *himself* must denote Bill, while the nominative pronoun *he* may or may not denote John. Coindexing indicates referential dependence of the pronoun on another noun phrase (see Higginbotham 198x): *himself* cannot depend on *John*, and *he* cannot depend on *Bill*. So while (b) and (c)

express perfectly coherent thoughts, (3) is not a way of expressing these thoughts in English. Only the (a)-reading is possible. By contrast, in (4) the accusative pronoun *him* cannot depend on *Bill*; although it may or may not depend on *John*. Section 4 provides further details and additional examples of related phenomena.²

(4) John said that he thinks Bill should wash him

Such facts are significant. For in the course of language acquisition children are exposed to finitely many strings of words, each of which presumably conveys a single meaning in the conversational context. Yet all speakers judge that strings like (2) are ambiguous. No one produces strings like (1), and no one tells children that such strings are ill-formed; yet even young children somehow know this. Similarly, no one uses strings like (3) or (4) to convey illicit meanings; but neither are children told not to assign such meanings. Yet again, even young children know that (3) does *not* have the illicit (b) and (c) readings. Grammars thus go beyond the finite primary linguistic data *along several dimensions*, as illustrated below. The Language Acquisition Device (LAD) represents the child's contribution to G_L , the grammar of the language spoken by the child in the stable state, on the basis of the primary linguistic data (PLD).

PLD	→	LAD	→	G_L
<string ₁ ,meaning ₁ >				<string ₁ ,meaning ₁ >
<string ₂ ,meaning ₂ >				<string ₂ , (meaning _{2a} , meaning _{2b})>
<string ₃ ,meaning ₃ >				<string ₃ ,meaning _{3a} >
...				...
<string _n ,meaning _n >				<string _n ,meaning _n >
				<string _{n+1} ,meaning _{n+1} >
				<string _{n+2} , (meaning _{n+2a} , meaning _{n+2b})>

² Competent users of a *spoken* language can recognize the meaningful *sounds* of their language as such, modulo constraints (like memory limitations) imposed by speakers' finite natures. Speakers can also tell when a sound does not correspond to a possible word in their language. For example, 'znala' sounds fine in Russian, but not English. Study of phonology reveals poverty of stimulus arguments that we will not discuss. (See, e.g., Halle 1990.) Moreover, compare (1) with:

- (i) Who left (the party)
- (ii) I wonder who left
- (iii) I wonder who John hoped left
- (iv) I wonder who John wanted/asked to leave

While (iii) is stilted, it is better than (1). This illustrates the reliable fine-grained discriminations that language users make about the *relative* acceptability of strings. But we will not stress this aspect of grammatical competence, except to note that it poses further difficulties for non-nativists. See Lightfoot (1991) for discussion of examples like (1) in the context of further poverty of stimulus arguments. Similarly, we will not focus on the fact that speakers can often assign (only certain) meanings to syntactically defective expressions like, 'the child seems sleeping'.

G_L generates endlessly many expressions, which associate word-strings with meanings. A string of words can be ambiguous, even if the child was exposed to it on just one reading. Many strings never encountered are ambiguous as well. Moreover, G_L will exclude endlessly many strings of words and, for each licit string, G_L will preclude the assignment of many meanings to that string; even though the PLD will not always contain explicit information to the effect that certain meaning assignments are illicit.³

The absence of ambiguity may go unnoticed until it is highlighted by theory. Consider the Binding Theory principles (Chomsky 1981) that govern the referential dependencies of pronouns -- and thus constrain the array of meanings that can(not) be assigned to (3): A reflexive pronoun must be bound locally (i.e., the antecedent of a reflexive pronoun must be relevantly 'nearby'); a nominative pronoun cannot be bound locally (its antecedent must not be 'nearby'); and a referring expression like 'Bill' cannot be bound by a pronoun.⁴ One wants to know just *how* these constraints, or refinements of them, are implemented (see Hornstein, forthcoming). But whatever the details, the Binding Theory offers a window into Universal Grammar. Correlatively, it is grist for the nativist mill: If young children adhere to the principles of the Binding Theory, one can ask whether or not these principles are plausibly learned by children.

Or consider the Yes/No question in (5). The declarative counterpart is (5a), and *not* (5b).

- (5) Was the child who lost kept away from the other children?
 (a) The child who lost **was** kept away from the other children.
 (b) The child who **was** lost kept away from the other children.

Similarly the string of words in (6) cannot have the indicated interpretation; the possible answer *Yesterday* refers to when John did the telling, and not when Mary did the eating.

- (6) *When_{*i*} did John tell you what Mary ate t_{*i*}?

These examples illustrate another well-attested linguistic phenomenon. Natural languages permit *displacement* operations, in which an expression "moves" from one sentential position to another; and there are *constraints* on these operations, with the result that many putative movements (and meanings) are illicit. We return to the details. But whatever the explanation, one cannot over-emphasize the importance of *facts that reveal constraints* for the study of language and language learnability.

For the child achieves a stable state that reflects all and only the constraints that govern human languages. So one cannot describe the acquisition problem – the *extent* to which the primary linguistic data underdetermine the stable state – without a description of these constraints. The question of whether children 'learn' language cannot be intelligently asked, much less answered, until one has a sense of *what* children learn. Theoretical linguists have postulated a rich system of constraints that explain many

³ Of course non-ambiguity and ungrammaticality are closely related. Constraints that exclude non-meanings for licit strings of words may also exclude other strings. We ignore lexical ambiguities, as in 'Meg found the bank', due to homophonous words.

⁴ An expression A c-commands another expression B if there is a path that extends above A to the first branching node, and then proceeds down to B. An expression A binds another expression B, iff: A c-commands B, and B is coindexed with A (making A the antecedent of B). The precise notion of locality remains a matter of study. As a first approximation, A is a *local binder* of B, iff: A binds B, and A is in the smallest clause that contains B.

phenomena of ungrammaticality and unambiguity in many languages. Experimental psycholinguists have investigated the emergence of these constraints in child language. These literatures provide compelling support for much older poverty-of-stimulus arguments. Challenges to the innateness hypothesis must fully engage with these literatures. We think Cowie fails to do so.⁵

As we emphasize throughout, the 'logical problem of language acquisition' is *not* just the problem of generating all the viable word strings, each with a correct meaning, from the finite PLD. The child acquires a grammar that generates *all* and *only* the sound/meaning pairs of the local language, modulo limitations on vocabulary. This raises the question of why (and how) the child acquires G_L -- as opposed to weaker, stronger or just different grammars. In so far as answers to this question require claims about the child's own contributions to grammar-formation, beyond recognition of the primary linguistic data and deployment of plausible learning principles, there will be poverty of stimulus arguments for innate aspects of G_L .

The acquisition process itself presents similar questions. If language learners are 'conservative,' admitting into their grammars only principles that generate expressions encountered in the linguistic environment, learners will *undergenerate*; their grammars will be weaker than adult grammars. Such learners would not achieve the stable states that allow for production and comprehension of sentences never encountered. As Pinker (1990, p. 6) remarks, "... children cannot simply stick with the exact sentences they hear, because they must generalize to the infinite language of their community." But "... if the child entertains a grammar generating a superset of the target language," that is also problematic from the standpoint of learnability. One might think that such *overgeneration* is easily correctable. Won't a child whose grammar is too strong say something that leads to correction by adults? Upon reflection and investigation, this is far from clear.

Crucial here is the distinction between positive and negative evidence. Positive evidence is evidence that certain expressions are licit (in the target grammar); negative evidence is evidence that certain expressions are illicit. The PLD is a source of positive evidence for children. Expressions used by adults should, presumably, be generable. But it is far less clear that the PLD is a source of negative evidence for children. Not only is corrective feedback rare, children are notoriously insensitive to it. Of course, one needs to distinguish explicit correction ('You cannot say that') from subtler facts that a clever child might notice (e.g., that adults never *use* a certain construction). But only negative evidence or some substitute for it can falsify a grammar that overgenerates. In short, the path of language acquisition can take a child through a stage of overgeneration only if some basis for correction is available.

With respect to *some* aspects of grammar, children do indeed go through stages of undergeneration and overgeneration before converging on a stable state. To a limited degree, children 'hone in' on adult grammar. For example, young children overgenerate when they use past tense forms like 'telled' while also using 'told'. In this respect, their grammar generates a superset of the adult expressions. This liberality is presumably overcome *in this case* because there is a substitute for negative evidence, a principle of 'uniqueness' that governs verbal morphology. When children realize that only a single word is used to express any inflected form of a verb, the appearance of 'told' in the PLD expunges 'telled' as the past tense form of 'tell' (Pinker, 1984). But few systematic examples of syntactic or semantic overgeneration have been documented (see Fodor and Crain, 1987). When children overgenerate in the

⁵ Similarly, Elman et al (1996) are content with a dozen general observations about the logical problem of language acquisition. Their main comment on research in linguistics (p. 384) is that eccentricities of form (language) need not be taken as evidence for eccentricities in underlying processes (grammar). They contend that 'connectionist simulations' are the empirical testing ground for adjudicating among competing claims about grammatical knowledge and grammar formation. We briefly review the success of such simulations in section 7.

domain of verbal morphology, non-adult expressions persist in their speech for many months, well into their 3rd or 4th year. Syntactic and semantic overgeneration, if it occurs at all, does not persist in this way.

In any case, in the absence of explicit negative evidence, an appeal to 'uniqueness' is one of only a few possible remedies for overgeneration. For syntactic and semantic overgeneration, however, a principle of 'uniqueness' is an unlikely source of implicit negative evidence (see section 6.1). This imposes a serious constraint on *noninnate* aspects of G_L: Learned aspects of grammars are not acquired via processes that would lead to uncorrectable overgeneration. Children do not employ learning principles that would lead to an overgeneration of expressions or meanings, such that children would not (in the normal course of events) be exposed to evidence that would correct the overgeneration. The parenthetical qualifier is important. It is not enough that *some* children will have correction experiences, or that all children *might*. All normal children rapidly achieve the stable state. So the relevant evidence has to be ubiquitous, such that every normal child is exposed to it. And the process of correction must not rely on any cleverness (attention, memory, *etc.*) that some normal children do not have.

In addition to overgeneration and undergeneration, another difference between child language and adult language involves what we will call *misfiring*. When children misfire, they are essentially ignoring the PLD, in favor of strings of expressions not attested in the target language. Nativists take heart in this. From the perspective of Universal Grammar, child language is expected to deviate from the target language, but only in circumscribed ways. The continuity hypothesis maintains that child language can differ from the language of the linguistic community only in ways that adult languages can differ from each other. On one familiar proposal, adult languages can differ (apart from vocabulary) only in the values assigned to a small set of innately specified 'parameters,' each with only a small number of possible settings. Because children do not know in advance which setting will be attested in the target language, they may adopt settings that prove 'wrong' -- so long as the 'correct' settings can be learned on the basis of positive evidence. We argue that misfiring occurs and that it presents a deep difficulty for nonnativists.

3. The Logic of Nativist Arguments

As the previous paragraphs suggest, it would be absurd to claim that *grammars* are innate, or even that children have access to *no* negative evidence. Although any child could have acquired any human language, children raised in (monolingual) English environments converge on English – not German, or Russian, or dialects that include 'telled.' The environment matters, and not just with respect to choice of vocabulary. Languages differ structurally in some respects. For example, there are parameters that determine whether direct objects come before or after verbs in transitive constructions. English children settle on the latter option, while Korean children settle on the former. So this aspect of the child's stable state must be determined by her experience. However, nativists contend that many aspects of grammars – e.g., universal linguistic principles like the Binding Theory -- are *not* acquired in this fashion. But since the issue is empirical, one should not expect *demonstrative* arguments.

In particular, no reasonable nativist tries to prove that children could never acquire English given any possible data (supplemented by any learning theory). After all, if linguists can discover the principles of human grammar by collecting arcane data and deploying the scientific method, it is at least conceivable that children do likewise. But it is highly implausible. For one thing, children are not generally good scientists. Typical children will not hit on the inverse square law no matter how much data concerning planetary orbits they encounter. Moreover children do not have access to the kinds of adult judgments (of unacceptability and non-ambiguity) that drive linguistic analysis. The *child's* primary linguistic data do not include *this* kind of evidence. Yet in just a few years children achieve a stable state governed by the very principles that trained linguists occasionally discern in their investigations of adult grammars. Nativists often make the point by saying that children could not have learned the adult language given the data available to them. But in this context, 'available' means 'plausibly available to all normal children'. (This leaves room for dispute about what is available; and nativists often present

various arguments, based on various premises about the available data. See, e.g., Lightfoot 1991.)

Some nativist arguments rely only on the minimal assumption that children have no explicit information about non-expressions. Those advancing such arguments can concede, for rhetorical purposes, that the primary linguistic data contain any number of (perhaps rare and complex) expressions. An earlier example is repeated here in (7).

(7) John said that he thinks Bill should wash himself.

Adults know, although they were never told, that 'himself' *cannot* be referentially dependent on 'John' or 'he'. Even if children learn that 'himself' is always coreferential with another term, based on examples like 'John washed himself', this leaves open the possibility that (7) is ambiguous. And note that 'himself' can refer to John in all of the following:

- (8) (a) John said that he thinks he should wash himself.
 (b) John said to Bill that he wants to wash himself.
 (c) John wants to shave Bill and wash himself.

Indeed, in (8c), 'himself' must refer to John.

So why *don't* children acquire a more permissive grammar, according to which 'himself' *may* depend on 'John' in (7)? But if children allow multiple interpretations – e.g., the antecedent of 'himself' can be any prior term – no positive evidence would prove them wrong. This invites the conclusion that humans have *a priori* knowledge of the relevant principle of the Binding Theory: A reflexive pronoun *must* be locally bound.

Perhaps children 'figure out' this principle, in part by noting that adult use of 'himself' always conforms to this generalization. Maybe children find regularities in the primary linguistic data, and then infer (nondemonstratively) that the regularities reflect rules. This requires 'figuring out' a good deal of syntax; see note 3. It is also worth asking how the regularities could be established in the first place, and why they remain stable despite the considerable latitude in experience and cognitive abilities of children. But let this pass for now, and consider (8d-f), where *himself*, in (8a-c) has been replaced by the accusative pronoun, *him*.

- (8) (d) John said that he thinks he should wash him.
 (e) John said to Bill that he wants to wash him.
 (f) John wants to shave Bill and wash him.

Adults know that 'him' in (8d) *cannot* be referentially dependent on the second 'he'; although 'him' can be referentially dependent on the first 'he' (or 'John'). But children are not taught such things. Also note that (8e) is multiply ambiguous, whereas 'him' *cannot* be referentially dependent on 'John' in (8f).

Once again, it is conceivable that children figure out that the pronominal 'him' cannot be bound locally. But adults can and do say things like 'That is him', where the demonstrative and the accusative pronoun have the same referent. Imagine a family looking at pictures: "Uncle Bob is here somewhere. That's him." Semanticists may well describe such cases as examples of coreference without antecedence: The direct object in 'That_i is him_j' is not referentially *dependent* on the subject, although the terms are coreferential ($i = j$). But how does the *child* know this isn't a case where 'him' has a local antecedent? After all, utterances do not come subscripted. (Similarly, adults can say 'That is Bob,' making it hard for learners to exclude 'He_i likes Bob_i'.)

Still, maybe children manage to avoid (or ignore) such examples in the course of acquiring the semantics of pronouns. In any case, no matter how impoverished the PLD are, it is always conceivable that children fill the gap by exploiting some substitute for explicit evidence of ungrammaticality. Cowie

puts a lot of weight on this point; and as a matter of logic, she is correct. Claims about the PLD and G_L can never constitute a *proof* that children do not learn G_L on the basis of the PLD *and something else*. This is especially clear if we allow that children can ignore (mischaracterize, or set aside for special treatment) at least some of what adults say. The issue is whether the proposed supplements to the PLD yield *plausible* alternatives to nativism.

If children *learn* the rule for reflexive pronouns, in part because adult speech contains no reflexives with *only* nonlocal antecedents, then children must be very good at determining adults' intended referents. Mistakes could yield a grammar that (uncorrectably) overgenerates. Moreover, an account is owed of *how* the absence of certain expressions affects language acquisition. If the suggestion is that children keep a record of word strings they encounter, along with the assigned interpretations, this seems to be at odds with independent studies of human memory. Adults can recall (at best) the gist of the immediately previous word string, not its phonological or syntactic details.⁶ Surely children cannot be expected to remember more than adults do.

On the other hand, if children don't keep specific records, it is hard to see how the factual absence of certain expressions could play any role in psychological development. The general moral is worth stressing: one must distinguish between the mere *availability* of data and the *utility* of that data for children. It is not enough that there *are* facts that would help children learn language. Children must be able to recognize and make use of these facts. And independently motivated claims -- say, about memory or other limited cognitive resources -- can be relevant in assessing the overall plausibility of various claims about language acquisition (*cf.* Elman et al. 1996).

At this point empiricists might retreat from specific alternatives to nativism, yet claim to have earned a Scotch Verdict: It is hard to assess the prior probability of nativism, and also hard to assess the relative probability of nativism compared with the claim that children exploit some (unspecified) substitute for negative evidence. In our view, this amounts to little more than the logical point that alternatives to nativism are conceivable. In the absence of a proposal about *how* children 'figure out' the Binding Theory and other linguistic principles, there is no empiricist alternative to assess. But the case for nativism is even stronger than this traditional line of argument suggests. For we have been reviewing a line of thought based on (i) minimal assumptions about the primary linguistic data, and (ii) adult judgments concerning some rather complex sentences. While such arguments are important, and pervasive in linguistics, the nativist quiver contains other arrows.

For example, nativists support their cause by arguing that children respect certain grammatical constraints at an early age. If adults can judge certain strings to be (un)acceptable or (un)ambiguous, then children must acquire such capacities as they grow into adulthood. But that leaves a lot of time for exposure to a lot of data. On the other hand, if young children demonstrate knowledge of the very principles that characterize adult grammars, that would compress the acquisition problem considerably. Of course, one can never prove that 3-year-olds have not already utilized a vast range of (positive and negative) data. But the case for nativism is strengthened in so far as children respect grammatical constraints before they are *plausibly* exposed to the data needed by learning accounts. The argument is even better if the alternatives to nativism rely on young children coming across (and attending to) complex and/or unusual expressions -- or if it independently implausible that *all* normal children will come across (and utilize) such data. This argument is developed more fully in sections 6 and 7.

⁶ According to Foss and Hakes (1978, p. 111), "Typically we cannot exactly recall an utterance by the time we have finished processing the next one. ... This limitation on our memory capacity strongly suggests that at the same time we listen to an utterance we are recoding its string of words into some other structural representation, and that this recoding occurs at a very rapid rate." (See also Sachs 1967; Wanner 1974).

We conclude this section by rehearsing the basic structure of the best nativist arguments. Initially, linguists consider an array of phenomena, including both positive and negative judgments of acceptability for strings of words, along with the permissible and prohibited meanings assigned to acceptable word strings. Setting aside considerations of the primary linguistic data available to learners, linguists propose analyses of adult grammars, to explain the range of linguistic phenomena under consideration. This is followed by cross-linguistic research, in search of putative linguistic universals -- candidates for inclusion in Universal Grammar. Given a possible linguistic universal, call it principle U, one asks whether or not there is sufficient evidence for U in the PLD. If every child acquires a grammar with U, but *couldn't* have encountered expressions that would have informed them that the target language has U, this is taken to be convergent evidence that U is part of Universal Grammar. The alternative is to suppose that the learning process was driven by something -- such as a substitute for negative evidence -- in addition to the PLD. Once candidates are considered, we claim, the proposal that linguistic universals are learned becomes more implausible. Even if children *could have* learned that the adult grammar has U (given the right data), it does not follow -- and it may not be at all plausible -- that they acquired a grammar with U by learning. The requisite data may be unavailable to children (or anyone but trained linguists); available data might not be utilizable (by children); or the data might not be sufficiently ubiquitous to account for the knowledge of U by *all* normal children, especially with respect to aspects of grammar where very young children exhibit adult linguistic competence.

Poverty of stimulus arguments often exhibit this multi-pronged structure; a *prima facie* case for nativism, based on minimal assumptions about the primary linguistic data, is bolstered by observations about what children know at an early age. After all, these are the hallmarks of innateness in other domains: early emergence, throughout the species, of traits that seem to be (dramatically) underdetermined by the environment. Cowie sometimes speaks as if she objects to the practice of advancing *various* poverty of stimulus arguments, with different kinds of premises. She draws analogies to a "many-headed hydra" (p.201): Just when one argument is shown not to be decisive, another springs up to take its place. But one can hardly object to the strategy of providing several converging (non-demonstrative) arguments for an empirical thesis.

4. Linguistic Constraints

There are two ways of describing the possibilities for referential (in)dependence with pronouns. One way is to list the various grammatical possibilities: accusative pronouns, like *him*, can be referentially independent; accusatives can also have antecedents (coindexed c-commanders) that are local; reflexives, like *himself*, must have local antecedents. The alternative is to formulate negative constraints, linguistic principles that exclude certain possibilities, leaving everything else open: Accusatives cannot appear with local antecedents; reflexives must appear with (cannot appear without) a local antecedent. Lasnik (1976) showed that the second strategy is more parsimonious. He proposed a single generalization that explained (mandatory) independence: Unless the conditions of the generalization are met, referential dependence is possible. Lasnik thus proposed a constraint on referential dependence.

The second strategy is also attractive on conceptual grounds. One might have expected referential dependence to *always* be possible. Why *can't* a pronoun just depend on anything else in the sentence? But in terms of acquisition, suppose a child conjectured that *Chris said Pat should help herself* can mean: $Chris_i$ said Pat_j should help her_i ? Or that *Chris said Pat should help her* can mean: $Chris_i$ said Pat_j should help $herself_j$? Such overgeneration would present a learnability problem, absent negative evidence or some substitute for it. And it seems unlikely that such overgeneration would even be noticed by adults, given the relative frequency of the sentences/contexts that would manifest the error. It seems even less likely that children would notice that adults use these relatively complex sentences *only* on certain interpretations. Moreover, the question is not whether *some children might* come across the needed evidence, but whether *all children (who overgenerate) would*. These observations suggest that children do not *add* the Binding Theory to a more permissive system of generating sound/meaning pairs. Rather, the

In many but not all linguistic contexts, the verbal elements *want* and *to* may be contracted to form *wanna*. Examples (10)-(13) illustrate permissible contractions. Example (14a) illustrates an impermissible contraction.

- (10) (a) Who does Arnold **wanna** make breakfast for?
 (b) Who does Arnold **want to** make breakfast for?
- (11) (a) Does Arnold **wanna** make breakfast for Maria?
 (b) Does Arnold **want to** make breakfast for Maria?
- (12) (a) Why does Arnold **wanna** make breakfast?
 (b) Why does Arnold **want to** make breakfast?
- (13) (a) I don't **wanna** make breakfast for Arnold or Maria.
 (b) I don't **want to** make breakfast for Arnold or Maria.
- (14) (a) *Who does Arnold **wanna** make breakfast?
 (b) Who does Arnold **want to** make breakfast?

On one standard account of *wanna*-contraction, Wh-questions are formed by movement of a Wh-phrase from one position at an underlying level of representation to another position, where it is pronounced. A further assumption of the account is that an empty category, which we abbreviate as *t* (for the 'trace' of Wh-movement), is left behind as a record of Wh-movement. In object extraction Wh-questions like (15), the Wh-phrase is extracted from object position of an embedded infinitival clause. The trace does not intervene between *want* and *to*, so *wanna*-contraction is permitted.

- (15) (a) Who do you want to kiss *t*? Object Extraction
 (b) Who do you wanna kiss *t*?

However, when the Wh-phrase is extracted from subject position, as in (16), the trace blocks contraction of *want* and *to* to form *wanna*.

- (16) (a) Who do you want *t* to kiss Bill? Subject Extraction
 (b) *Who do you wanna kiss Bill?

These data invite the following generalization: Contraction of the two verbal elements *want* and *to* is blocked if the trace of Wh-movement intervenes between them.⁸ In declaratives, the constraint on contraction is irrelevant, so contraction is tolerated.⁹

As examples (10)-(13) indicate, much of the evidence available to children learning English runs counter to the constraint. Contraction of *want* and *to* is licensed in general -- (14) is an exception to the rule. Therefore, if learners were to adopt standard principles of induction, they would be tempted to violate the constraint. If the grammars of English-speaking children lacked the constraint on contraction

⁸ More specifically, contraction is prohibited across a Wh-trace that is Case-marked (Chomsky 1980; Jaeggli 1980). An alternative explanation of the facts, offered by Snyder and Rothstein (1992), is that a null, Case-assigning complementizer is responsible for blocking the contraction. The details of the analysis are not important for this paper.

⁹ In English, the constraint on contraction across a Wh-trace also prohibits *supposed to* from being contracted to *sposta*, and *have to* from becoming *hafta*, and so on. In French, the constraint affects the environments in which liaison is possible (see Selkirk 1972).

of *want* and *to*, then child English would include more sentences than adult English does. Without the constraint, children would overgenerate.

Children who lack the constraint on contraction across a Wh-trace should permit contraction to a similar extent in both subject and object extraction questions. To test children's adherence to the constraint, then, an experiment was designed to elicit relevant questions from children (Thornton 1990, 1996). This permitted a comparison of the proportion of contraction by children in questions like (14) with contraction in questions like (10). The finding was that the 21 children interviewed (mean age = 4;3) contracted more than half the time (57%) in questions like (10), but these same children contracted less than 10% of the time in questions like (14), where contraction is outlawed by the constraint.

4.2 An unexpected generalization

The linguistic constraint that prohibits *wanna*-contraction also applies to a variety of other constructions, but not in ways that can easily be determined on the basis of the primary linguistic data. For example, the constraint prohibiting contraction across a Wh-trace governs a linguistic phenomenon known as *is*-contraction. As example (17) illustrates, the verbal element *is* appears to contract with the word to its left in Wh-questions. Notice, however, that the trace of Wh-movement is positioned between *think* and *'s*, as indicated in (18). Something is amiss. Either (17) represents a counter-example to the constraint on contraction across a Wh-trace, or the orthographic representation in (17) is misleading, and the verbal element *is* actually contracts with the word to its right. If so, (19) would be a linguistically motivated orthographic representation of the string of words in (17).

(17) What do you think's in the box?

(18) What do you think **t** is in the box

(19) What do you think s'in the box?

Once more complex data are considered, it becomes evident that the orthographic representation in (17) is misleading; (19) is more accurate. That is, a good case can be made that *is* contracts to its right. The paradigm in (20) provides some relevant evidence. The paradigm shows that *is* can contract when there is a Wh-trace to its left, as in (20b), but not when there is a Wh-trace to its right, as in (20c-d).¹⁰

(20) (a) Do you know what that is doing **t** up there?

(b) Do you know what that's doing **t** up there?

(c) Do you know what that is **t** up there?

(d) *Do you know what that's **t** up there?

Therefore, (17) is not a counter-example to the constraint that prohibits contraction across a Wh-trace; the constraint simply does not apply in (17). It applies in (20c-d), however, blocking contraction.

Having witnessed two applications of the constraint on contraction, it is important to ask how a learning-theoretic account could explain the generalization that relates *wanna*-contraction and *is*-contraction. The constraint applies to linguistic phenomena that bear little superficial resemblance. In the *wanna*-contraction paradigm, the constraint prevents contraction across the subject position of an embedded infinitival clause, whereas in the *is*-contraction paradigm, the same constraint prevents contraction across the object position in a tensed clause. Until a wide range of linguistic phenomena was considered, including both positive and negative data, linguists failed to see that the two phenomena were related. Assuming that language-learners do not have access to such complex arrays of positive and

¹⁰ *Is*-contraction is also blocked if there is *no* linguistic material to the right of *is*, e.g., when it is in sentence final position.

negative data, nativists conclude that language-learners must have an advantage over linguists, in knowing the linguistic constraint on Wh-trace in advance of encountering the limited primary linguistic data to which they have access.

Returning to child language, the nativist is compelled to predict that children will adhere to the constraint on contraction across a Wh-trace in both constructions. We have already reviewed the results of an investigation of children knowledge of the constraint as it applies to *wanna*-contraction. Another twelve 2- to 4-year-old children participated in an elicited production experiment designed to assess their knowledge of the constraint that prohibits contraction across a Wh-trace (Thornton, 1990). The finding was the complete absence of illicit productions. Illicit contraction is apparently prevented by the constraint. These mutually supporting findings suggest that the same constraint rules out (14a) and (20d). Nonnativists owe a learning account of how children could plausibly *add* such a constraint to a grammar that does not already incorporate it.

4.3. A constraint on referential dependence

In (21) and (22), the pronoun *he* may or may not be dependent on the referential expression (r-expression) *the Ninja Turtle*. Again, coindexing indicates referential dependence.

(21) The Ninja Turtle_i danced while he_{i/j} ate pizza.

(22) While he_i ate pizza, the Ninja Turtle_{i/j} danced

In (23), however, referential dependence is impermissible; (23) is unambiguous, having only the (a)-reading.

- (23) He danced while the Ninja Turtle ate pizza.
 (a) He_i danced while the Ninja Turtle_j ate pizza
 (b) *He_i danced while the Ninja Turtle_i ate pizza

The relevant constraint, Principle C of the Binding Theory, prohibits "backwards anaphora" in (23). That is, the constraint prohibits coindexing a pronoun and a referring expression when the former c-commands the latter. If child grammars lacked Principle C, (23) would be ambiguous; the (23b)-reading ought to be available for children. But as Crain and McKee (1985) demonstrated, children (aged 2-5; N = 62) were effectively adults in this respect.

Taken by itself the ungrammaticality of (23b) is not terribly surprising, since *He* binds *the Ninja Turtle*. For plausibly, (i) a referentially dependent term must be c-commanded by its antecedent; and (ii) descriptions, like names, are *basic* referential devices -- i.e., *not* expressions whose reference is ever anaphoric. So neither of the coindexed expressions in (23b) can be referentially dependent on the other. But the question is whether young children *figure out* (i) and (ii), based on evidence; see the discussion of (8) above. We think the better hypothesis is that innate aspects of grammar ensure (i) and (ii), making it unnecessary --and psychologically impossible -- for children to consider grammars that do not respect these constraints.

4.4 Another unexpected generalization

Principle C provides another example of how language-learners project beyond the primary linguistic data in ways that classical models of induction would not anticipate. The relevant observation is that Principle C governs other constructions besides backward anaphora, including strong crossover questions. An example is given in (24). The relevant observation is that the pronoun, *he*, must be construed deictically, as picking out a single male individual. On one standard account, the strong crossover question in (24) is derived from the underlying representation in (25). Wh-movement applies to (25). As it moves, the Wh-phrase 'crosses over'

the pronoun on its way to its surface position, leaving a Wh-trace behind at the site of extraction. The result is (26).

(24) Who did he say has the best smile?

(25) He said who has the best smile

(26) Who did he say t has the best smile

Notice that in (26) the pronoun c-commands the Wh-trace. In the backwards anaphora construction, illustrated in (27), the pronoun c-commands the r-expression, *Yogi Bear*.

(27) He said that Yogi Bear has the best smile.

Chomsky (1981) proposed that Principle C governs the anaphoric relations in both constructions, prohibiting coindexation of the pronoun and Wh-trace in (26) -- thus prohibiting coindexation of the pronoun and 'Who'. This explains why the pronoun cannot be referentially dependent on the Wh-phrase in a strong crossover question. Principle C dictates that the only way to interpret the pronoun is deictically. The pronoun in (26) picks out a single male individual.

(28) *He_j said Yogi Bear_j has the best smile

(29) *Who_j did he_j say t_j has the best smile

If Principle C were not operative, (26) would be ambiguous. In addition to the deictic reading of the pronoun, the pronoun could be referentially dependent on the Wh-phrase. This would result in an interpretation, indicated in (29), in which the pronoun is treated as a bound variable. Although the bound variable interpretation is not possible for strong crossover questions, this interpretation arises in questions like (30), which can be analyzed as in (31). Principle C does not apply in (30), since the Wh-trace is not bound by the pronoun.

(30) Who said he had the best smile?

(31) which x is such that x said x has the best smile

An experiment was conducted to find out if children distinguish questions like (30), which have a bound variable reading for adults, from strong crossover questions like (24). If child grammars respect Principle C, children should reject the bound variable interpretation of (24); only the deictic interpretation should be available to them. By contrast, if child grammars lack Principle C, then both the deictic interpretation and the bound variable interpretation ought to be available for both (24) and (30); these questions should be ambiguous for children. The findings of the experiment were clear-cut. The twelve children tested (3-5 years) rejected the illicit reading of strong crossover questions like (24) over 90% of the time, whereas they accepted the corresponding readings for questions like (30) about half the time (Crain, 1991; Thornton, 1990, Crain and Thornton 1998).

It is time to take stock. Languages contain constructions that go well beyond children's primary linguistic data, in ways that are surprising given the absence of negative evidence. In order to fill this gap, linguists postulate a rich complex system of principles, and appeal to theoretical constructs like traces -- unpronounced records of expressions that have moved (perhaps quite far) from their original positions. But there are substantive *constraints* on such displacement; and such constraints provide the *starting points* for poverty of stimulus arguments. If the specific constraints hold, as descriptive (psycho)linguistics suggests, are they plausibly learned? Similar remarks apply to referential dependence of pronouns,

wanna-contraction, and a plethora of other linguistic phenomena not discussed here (see Crain and Thornton 1998 for an extensive review). One cannot separate arguments for nativism from arguments that grammars are constrained in idiosyncratic ways. Replies must either challenge these descriptive claims about human grammars, by providing alternative explanations of the relevant judgments, or address the learning problems (including those concerning uncorrectable overgeneration) associated with *specific constraints*. We have reviewed several experimental studies that examined constructions for which the linguistic input potentially encourages children to form hypotheses that are *not* consistent with the grammar of the target language. In the absence of innate linguistic knowledge, children who use linguistic experience as a guide would be expected to overgeneralize where adults do not, producing illicit constructions and assigning illicit interpretations to sentences. But children evidently do not consider hypotheses at odds with the (independently hypothesized) principles of Universal Grammar. This suggests an acquisition scenario according to which children are guided by innate knowledge.

5. Language as Second Nature

With some illustrative poverty-of-stimulus arguments in place, we turn to Cowie's criticism of nativist arguments. Unsurprisingly, Cowie contends that learners may have better data, and may be able to make more of it, than nativists suppose. The proposal is that (i) the primary linguistic data are less impoverished than initially appears, and (ii) children are capable of extracting more from their experience than one might think. These would be interesting suggestions, were they accompanied by a proposal about how children extract the various kinds of constraints discussed above (and below) from the data available to them. But Cowie does not, in this sense, offer an alternative to positing a Universal Grammar.

Cowie suggests, however, say that "the apparent plausibility of the Chomskian position derives from the nativist's helping himself to a variety of more or less implausible assumption about learners and their experience (p. ix)." Arguments from the poverty of the stimulus are "based on empirical assumptions that are at worst outright false, and at best highly dubious (p. 177)." Cowie contends that the poverty-of-the-stimulus-argument "is completely unable to support *any* form of nativism about language-learning; and "obsession with such arguments has therefore been a mistake." She concludes (p. 276) that there is "no reason to accept" the proposal that language acquisition is constrained by innate principles of Universal Grammar; there is "no 'back door' route to nativism opened up by consideration of whether the Chomskian constraints on grammars are learnable from experience;" and "the poverty of the stimulus, that trusty innatist stalwart, likewise does nothing to brace the nativist position on language acquisition (p. 276)" These are strong claims. But since they are not backed up by detailed responses to the kinds of arguments reviewed above, they are hard to take seriously. If poverty-of-stimulus arguments don't confirm the Universal Grammar hypothesis *at all*, one wonders what the standards for confirmation are.

Rhetoric aside, Cowie's own position is rather agnostic. She is neither a behaviorist nor a classical empiricist; she tentatively endorses a "weak" form of nativism, but one that does not posit a Universal Grammar. Cowie takes the logical problem of language acquisition to be "a completely general problem arising for all learning involving projection beyond our experience (p.215)." The deep issue is whether this is so. We think Chomsky and others have provided good arguments to the contrary. Critics need to *engage* with those arguments and provide an *alternative* to the principles of Universal Grammar.¹¹

¹¹ Cowie draws a relatively sharp distinction between (what she regards as) *a priori* and *a posteriori* poverty of stimulus arguments. We regard all the arguments above as *a posteriori*. But like most arguments in science, they involve both premises (typically confirmed by empirical methods) and reasoning (demonstrative and nondemonstrative); compare arguments in physics that draw on Bell's Theorem. We think Cowie distorts the literature by suggesting that many nativists still try to establish nativism *a priori*. In any case, *Chomsky* does not deserve Cowie's suggestion (p.174, p.248) that he only

Cowie suggests that we consider knowledge of curries. Exposed to just a few examples, humans come to know what curries are; we can reliably classify novel dishes as curries or not. How does our knowledge extend so far beyond the primary curry data? Cowie's suggestion is that such learning is possible because of "the vast quantity of indirect or implicit negative evidence about curries available (p. 216)." For example, burgers are called 'burgers', not 'curries'; which suggests that burgers are not curries. And "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)."

While Cowie does not claim that language-nativism is as implausible as curry-nativism, she does think the case for language-nativism is somehow weakened by the fact that we project beyond the primary curry data. Of course, if anyone were to hold that nativism is established by the mere fact that children generalize, Cowie is right to lampoon them. As we have emphasized, however, projecting beyond experience is just one aspect of language acquisition. Children also fail to project beyond their experience in characteristic ways. It is *this* fact that most impresses nativists. The theoretical problem posed by human language learning is to explain why children project beyond their experience just so far and no further; the specific "angle" of projection seems arbitrary (and idiosyncratic to *linguistic* projection). We see no parallel to this problem in the curry example, if only because there is no known analog of the Binding Theory, or constraints on displacement, for curries.

Still, it is worth considering the suggestion that learners may achieve the target grammar by exploiting available negative evidence. We have seen that by around age 3, children consistently adhere to linguistic constraints -- and thus refrain from generalizing in ways that experience might tempt them. In the absence of evidence of overgeneration in experimental studies with 3-5-year-olds, we infer that children do not overgenerate. Beyond a few anecdotes, there is no evidence that children commit systematic errors in forming Yes/No questions; in knowing when (not) to contract; and in assigning the correctly limited range of meanings to pronouns. This does not mean that children never make errors. Even adult language users occasionally behave in ways that diverge from their remarkably stable competence judgments. Researchers in child language thus commonly allow for a margin of error, due to experimental 'noise', generally set at 10% of children's responses. Assuming that such a margin for errors is reasonable, it is safe to say that experimental investigations have not revealed violations of putatively universal principles of language. (See Bogen and Woodward [1988] on the general issue of 'noise'.)

Cowie conjectures, however, that children may initially overshoot the target language before age 3. She holds that such children may hypothesize grammatical principles that are not in the linguistic system of adult language users. Having made this conjecture about the course of language development, she then suggests that children may nevertheless recover from their nonadult hypotheses on the basis of direct or indirect negative evidence falsifying their erroneous grammatical hypotheses. Of course, if her conjecture is incorrect, and children do not make grammatical errors before age 3, then the issue is moot; even if negative evidence exists, children would never need to avail themselves of it.

Suppose that children do make grammatical errors before age 3. Then negative evidence must be available *in sufficient quantities* during the early stages of language development. Abundant evidence is needed to ensure that *any* child who makes an error can recover from it by age 3 (or thereabouts). In

occasionally defends his claims about Universal Grammar as empirical hypotheses. Like many authors, Chomsky often sketches arguments (especially in opening chapters and material intended for non-specialists) developed in more detail elsewhere. But unsurprisingly, his work is relevant to his more general views. Rightly or wrongly, Chomsky takes the detailed constraints that emerge from linguistic investigation to support his brand of nativism, the argument for which has always *a posteriori*; although Chomsky (1966) has also stressed its connection with older philosophical arguments. We return to this point in the final section.

considering the availability of negative evidence, Cowie focuses on the structure-dependence of rules. She construes nativists as assuming that "no evidence exists that would enable a three-year-old to unlearn" mistaken (structure-independent) rules. But no reasonable nativist would endorse such a strong claim about all possible evidence. Cowie quotes the following passage from Chomsky (1975, p.31) as the basis of her (p. 184) interpretation: "A person may go through a considerable part of his life without ever facing relevant experience, but he will have no hesitation in using the structure-dependent rule, even if all of experience is consistent with [the structure-independent rule]." But as this passage makes clear, Chomsky is not claiming that nobody *ever* has relevant experience; the issue concerns the robustness of evidence, not its existence. As Lasnik and Crain (1985) note, if relevant data are not robust, then at least some (and perhaps many) children won't come by them, and these children will not converge on the grammar of the linguistic community. But this does not happen; all (normal) children converge on the steady state. Therefore, if convergence depends on there being relevant (positive or negative) evidence, then the evidence must be available in abundance.

Suppose, to the contrary, that evidence falsifying the structure-independent hypothesis for forming Yes/No questions is not available to children, in abundance, before they reach their third birthday. Then many children should be observed to make structure-independent errors. But every 3-year-old that has been studied experimentally has been found to obey structure-dependence (Crain and Nakayama 1984). So either children never form structure-independent hypotheses (before or after age 3), or there is abundant negative evidence *available to and used by* very young children. Is there any guarantee that there *is* abundant evidence that lets all children learn that all linguistic principles are structure dependent? According to Cowie, "... something like the requisite guarantee can be provided when one reflects on the sheer size of the data sample to which a learner has access." (p. 219). Of course, even if this turns out to be so for Yes/No questions, the same question could be raised for every grammatical constraint that children obey. But let's consider the facts, including the *specific* constraint in question, in a little detail.

The issue of Yes/No questions is interesting, as Chomsky (1971; 1975) observed, because the actual rule for forming such questions appears to be more complex than an alternative structure-independent process for question formation. As Chomsky also observed, both structure-dependent and structure-independent hypotheses are compatible with much of the input that learners receive, i.e., sentences without embedding. For example, a structure-independent hypothesis like 'move the *first* auxiliary verb' yields the right results for questions like the following:

- (32) (a) Bill can play the sax. ⇒ Can Bill play the sax?
 (b) The sky is blue. ⇒ Is the sky blue?

But the structure-independent hypothesis won't always yield the right results. In (33), the subject NP is *the man who is beating a donkey*. In the corresponding Yes/No question, the main auxiliary verb *is* moves past the subject NP, yielding (34). Fronting the first auxiliary verb, which is embedded in the relative clause, is impossible; (35) is ungrammatical.

(33) The man who is beating a donkey is mean

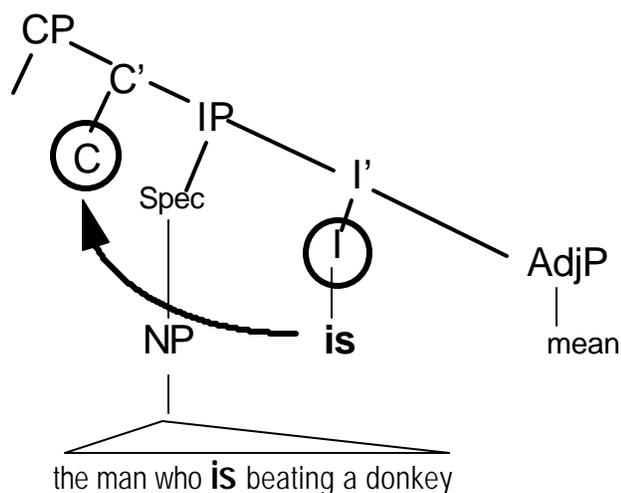
(34) Is the man who is beating a donkey mean?

(35) *Is the man who beating a donkey is mean?

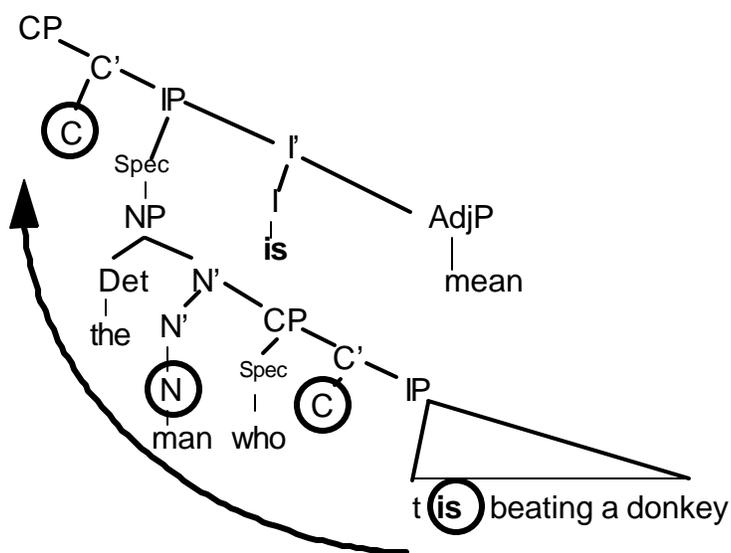
Since Chomsky himself draws attention to sentences like (33-34), his view is presumably *not* that such word strings are not part of the primary linguistic data, and therefore could not be used to expunge sentences like (35), if children produced them. But exegesis aside, Cowie is right to note that such sentences are part of children's input. So why do nativists bother talking about Yes/No questions?

For one thing, one might wonder whether *all* children who form Yes/No questions in an adult fashion have been exposed to (enough) evidence of the relevant sort. But more importantly, the point is not merely that the simple-minded word-order rule is wrong. As always, the Chomskian nativist argument stresses the specific constraints governing grammatical operations. In this case, the best linguistic analysis to date is that Yes/No questions are formed by ‘local’ movement. The auxiliary verb that moves is the inflectional head, I, of the projection IP. The movement takes the auxiliary verb to the next head position, C, which is the head of the Complementizer Phrase, CP. The process is depicted in (36). As (36) indicates, such movement is subject to the Head Movement Constraint (Travis 1984). According to this constraint, heads of phrases can only move locally. Movement of the auxiliary verb in the relative clause, as in (37), would violate the constraint because such movement would cross the heads of *two* other phrasal projections (circled in the diagram in 37).

(36) I to C movement



(37) The Head Movement Constraint (I to C Movement is blocked by intervening heads).



The same principle explains the nonambiguity of example (5), repeated here as (38).

(38) Was the child who lost kept away from the other children?

Given the Head Movement Constraint, the auxiliary *was* cannot have originated in the relative clause. So (38) cannot have the following (perfectly coherent) meaning: Was the child who was lost kept away from the other children?

Suppose that young children initially form a structure-independent hypothesis, instead of the I-to-C movement analysis, and then encounter complex examples like (33) or (38). At this point, children are charged with two tasks. First, they must modify their grammar to allow for I-to-C movement, as in (36). When this is accomplished the learner will produce adult Yes/No questions. But children face a second task. They must *also* learn the Head Movement Constraint in (37). That is, children must also learn the *ban* on non-local movement of heads. Adding the rule for I-to-C movement does not entail purging the incorrect structure-independent process. For children who begin with a structure-independent hypothesis, the existence of experience revealing the possibility of I-to-C movement does not suffice to outlaw non-local movement. The non-local movement option could co-exist alongside the local movement option in children's grammars. Until children come to know (37), *both* styles of question-formation would be tolerated, and (38) would be ambiguous. The learnability problem facing children is how to learn from experience that non-local movement is prohibited. This is why nativists bother talking about Yes/No questions.

We are not denying that children could learn that the hypothesis 'move the first auxiliary verb' is incorrect. One way would be to maintain a case-by-case record of the constructions attested in the primary linguistic data. Another possibility is that children could exploit negative evidence; *perhaps* children retreat from overgeneration by 'figuring out' that the grammar contains no structure-independent rules. For reasons we discuss below, these solutions are unlikely on computational and more general psychological grounds. But the question of eliminating errors misses the mark. We want an account of fact that every language, and therefore every learner, enforces a ban on non-local movement of the heads of phrases; that, in English, contraction of *want* and *to* is prohibited in questions where the Wh-phrase is interpreted in the subject position of an infinitival clause, and *is*-contraction is prohibited when the wh-phrase is interpreted in the position following it; and that referential dependencies are restricted in both declaratives and in strong crossover questions.

To sum up the present discussion, nativist arguments concerning the structure-dependence of rules are not the howlers that Cowie makes them out to be. The point is not that children *couldn't* falsify a grammar that is governed *solely* by linguistic operations that appeal solely to the linear order of words in strings of words. The point is that the falsifying evidence is not obviously robust in the required way, and more importantly, all normal children know (and know early) that their grammar does not include *any* strictly linear word-order rules. Again, facts about ungrammaticality and unambiguity matter, and we are owed an account of how children could learn such facts.¹²

6. Direct and Indirect Negative Evidence

¹² Historically, focus on structure-dependent rules was associated with a theoretical shift from phrase-structure theories to transformational theories governed by constraints. But it is also worth remembering that acoustic strings do not have intrinsic syntactic structures. Such structure is imposed (or "projected") by speakers. Consider the familiar analogy of necker cubes, which normal humans "see" as three-dimensional. This kind of projection contrasts with traditional induction, where mind-independent phenomena (e.g., gravitation) give rise to data (e.g., observed motions of planets) that provide a basis for nondemonstrative inferences about the phenomena in question (e.g., that it is governed by an inverse-square law).

Only negative evidence, or some substitute for it, can inform learners that they have overshot the target language. If learners overgeneralize on the syntactic paradigms illustrated in section 4, then they will have grammars that generate supersets of the word strings of adult-English. So, if there is insufficient evidence that such grammars are too strong (in relevant respects), children do not overshoot in the first place. (Or at least their eventual retreat to the adult grammar is not driven by learning.) As we have already mentioned, there is only anecdotal evidence of *any* overgenerating in the literature on child syntax or semantics, although there is evidence of overgeneration in the acquisition of morphological processes such as forming the past tense of a verb. Still, it is worth examining the idea that children exploit negative evidence, if only to emphasize that language acquisition is not a general induction problem. After briefly considering the availability of direct negative evidence (ungrammatical expressions labeled as such), we address some claims about more subtle (implicit) forms of negative evidence.

Over the past 30 years, many researchers have examined whether children receive various forms of negative reinforcement for ill-formed sentences. Among the earliest studies was that of Brown and Hanlon (1970), who analyzed hours of tapes of children interacting with their parents. Children were rewarded, with attention and approval from their parents, when what they said was *true*; and approval was forthcoming, even if the children's utterances were ungrammatical. If a two-year-old child said, "Doggie sit chair" and the dog was sitting on the chair, the parent might have said, "Yes, that's right", as opposed to "No, don't say it that way, say: *The doggie is sitting on the chair.*" But suppose the child said, "Johnny got a cookie," which is perfectly grammatical, when in fact Johnny didn't get a cookie. Then the parent would reject the utterance and reply "No, he didn't." Adults may reward children selectively for what they say; but the pattern of rewards does not explain how children might learn that about *grammatical errors*.

Other researchers have reached the same conclusions (e.g., Bowerman 1988; Brown and Hanlon, 1970; Morgan and Travis, 1989; Marcus, 1993). Bowerman (1988) argues that what little negative evidence has been uncovered to date is irrelevant for the specific cases of overgeneration that are attested in the literature on child language. Slobin (1972) concludes that children are not corrected for ungrammatical utterances in many of the societies studied by his research group. Pinker (1990) concludes that

"When parents are sensitive to the grammaticality of children's speech at all, the contingencies between their behavior and that of their children are noisy, indiscriminate, and inconsistent from child to child and age to age." (p. 217)

Moreover, even if negative evidence were available, children may not avail themselves of it. There are no evidences showing that children exposed to negative evidence use it to purge their grammars of incorrect hypotheses (see Newport, Gleitman and Gleitman 1977).

Cowie cites studies showing that mothers of 2-year-olds repeated (and usually corrected) their children's ill-formed utterances 21% of the time, but mothers repeated their children's well-formed utterances only 12% of the time, a significant difference. Similar findings were reported in a study by Demetras, Post and Snow (1986). A study by Bohannon and Stanowicz (1988) found that over 70% of parents' recasts and expansions follow children's ill-formed sentences, and 90% of their exact repetitions follow well-formed expressions. However, only 34% of children's syntactic errors were followed by any form of feedback. As Cowie remarks, "the different kinds of response distinguished by Bohannon and Stanowicz do not correlate anywhere near perfectly with the well-formedness of children's utterances." Nevertheless, Cowie says that such studies are "of critical importance to our understanding of language acquisition." For they "show that *negative data*—explicit information as to what sentences are not—*do exist* in the child's environment, albeit in a noisy form. As such, they do much to resolve, at least in principle, the Logical Problem of Language Acquisition (p. 234) "

We are not similarly enamored with the existence of 'noisy' negative evidence for some children, at some ages. It needs showing that such data are available to all children (at the right ages) and that the

data correlate with children's recovery from errors.¹³ Moreover, one needs to specify just *which* informational content children assess by using statistical methods. It may be statistically significant that 70% of parental recasts follow ill-formed expressions, while 30% follow grammatical expressions. But are children sensitive to this difference? Are the thresholds for significance for a child the same as for a statistician? Moreover, for children to use relative frequencies of the sort cited as a means of ruling out grammatical hypotheses, they must somehow represent their previous utterances. Cowie suggests that children keep a record of the "rules" or "structures" that they have used. We turn next to the plausibility of this assumption.

Even if noisy negative evidence can falsify erroneous grammatical hypotheses, the existence of such evidence (for some children at some ages) doesn't begin to explain how *every* child converges on a rich and complex target grammar before they attend grammar school. Indeed, while Cowie stresses the existence of negative evidence, she eventually admits that not all children experience it:

"... the fact that some children may be denied useful feedback does not indicate that those who are lucky enough to receive it do not exploit it in their acquisition of language. ... For there is no reason to insist that all children must make use of the same sorts of evidence to the same extent. ... A child who lacks feedback will place greater reliance on other sources of negative evidence... p.232"

But this admission makes a mystery of the fact that *all* children acquire the target grammar, unless the "other" sources of negative evidence are ubiquitous and would lead any normal child to the target grammar. This is important, since Cowie never says *how* negative evidence can aid children in learning specific linguistic constraints. We see no reason to suppose that adult responses to ill-formed utterances could help children recover from errors of overgeneration, if children initially overgenerate in the course of learning the relevant constraints.

6.1 The Uniqueness Principle

This brings us to a consideration of putative substitutes for direct negative evidence. To illustrate how errors might be corrected using indirect negative evidence, Cowie offers an example of a girl, Edna, who sees her father drop a cup off a table. Edna's father then remarks on what happened:

(39) Father: I caused the cup to fall from the table.

Upon hearing her father's description of the event, Edna engages her current linguistic system and

¹³ In this regard, studies by Cazden (1972) and by Nelson, Carskadden and Bonvillian (1973) found that children who received expanded parental input fared no better through the course of language development than children who did not. Cowie responds by pointing out that if grammatical change takes place in small steps, with recovery from different errors taking place at different times, then one would not expect to find an overall correlation between the availability of negative evidence and children's grammatical advances. But small steps in the course of acquisition makes the paucity of negative evidence *more* problematic, not less problematic. To the extent that negative evidence (expansions, misunderstandings, recasts) is not consistently available throughout the course of development, it is less likely to be the source of error correction, precisely because the relevant evidence for correcting any particular error is less likely to be available at that stage of development at which it would be most useful. (Similarly, Cowie misconstrues the common idealization to "instantaneous" learning: nativists are not helping themselves to an illicit premise, but rather letting empiricists assume that any evidence the child encounters is available for computation at any stage of learning.)

determines that she would describe the same event as in (40).

(40) Edna: I falled the cup off the table.

Cowie suggests that the mismatch between the string generated by Edna's father and the one generated (silently) by Edna constitutes evidence that some property of her grammar must be jettisoned and replaced with something else. Somehow, Edna identifies the faulty rule or structure in her system, and replaces it with a rule or structure that generates (39).

But this assumes that Edna won't modify her grammar to allow for *both* (39) and (40); for the example to work, (39) must dislodge (40). Hearing (39) might simply lead Edna to a grammar with two ways of expressing the same message, as in (41).

(41) (a) I caused the cup to fall from the table.
 (b) I falled the cup off the table.

In short, for Cowie's scenario for 'unlearning' to work, children need to assume some kind a principle like the following (Wexler, 1979; see Clark 1987 for an extensive review):

The Uniqueness Principle: Different linguistic forms express different messages.

Given this assumption, perhaps encountering (39) would indeed lead Edna to a grammar that excludes (40).¹⁴ Perhaps some mechanism purges grammars of "redundant" lexical items that express the same meaning. As noted earlier, a variant of the uniqueness principle has been invoked to explain how children purge their lexicons of non-adult forms like "goed" and "foots" (see Pinker 1984). As Fodor and Crain (1987) point out, however, the same solution is highly implausible as an explanation of how learners abandon erroneous syntactic principles. For example, recall the *wanna*-contraction paradigm. We cannot detect even a subtle difference in meaning between the (a) and (b) expressions in (42) and (43).

(42) (a) Why does Arnold want to make breakfast?
 (b) Why does Arnold wanna make breakfast?

(43) (a) Who does Arnold want to make breakfast for?
 (b) Who does Arnold wanna make breakfast for?

(44) (a) Who does Arnold want to make breakfast?
 (b) *Who does Arnold wanna make breakfast?

Adults may or may not contract in certain linguistic contexts, in apparent conflict with the uniqueness principle; so while contraction is impossible in (44), this is *not* because the meaning of (44a) somehow 'blocks' (44b) via uniqueness. Hence, if Edna had incorrectly overgenerated, hypothesizing that *wanna*-contraction is always possible, it can't be the uniqueness principle that would help her retreat from the error. Other examples of contraction make the same point.

Nor would the uniqueness principle help a child acquire principle C of the Binding Theory. Recall that (45) is ambiguous, while (46) is not:

(45) The Ninja Turtle danced while he ate pizza.
 (a) The Ninja Turtle_i danced while he_j ate pizza
 (b) The Ninja Turtle_i danced while he_i ate pizza

¹⁴ The issue is unclear, since 'x broke y' and 'x caused y to break' seem to differ slightly in meaning; and in any case, adult English allows for the latter construction, which is no more stilted than (39).

- (46) He danced while the Ninja Turtle ate pizza.
 (a) He_i danced while the Ninja Turtle_j ate pizza
 (b) *He_i danced while the Ninja Turtle_i ate pizza

How is the child to learn this constraint (and others) on possible interpretations of pronouns. Whatever utility the uniqueness principle may have in the domain of verbal morphology, it does not have the same utility as an account of how learners come to know that (44b) and (46b) are ungrammatical; but to correct overgeneration, this is just what is needed. Moreover, at the level of sentences, semantic uniqueness assumptions are independently implausible. Consider apparently synonymous pairs like *There are three men in the garden/ Three men are in the garden* or *The store doesn't carry meat or potatoes/ The store doesn't carry meat and the store doesn't carry potatoes*. Thus, our conclusion continues to be that of Fodor and Crain (1987, p. 49):

"The Uniqueness Principle may very well be an important factor in the acquisition of phonology, and of lexical rules, and perhaps even of some 'pure' syntactic rules. But it fails for other important syntactic phenomena...the Uniqueness Principle will not substitute for direct negative data in all the cases in which it would be needed if learners did have a systematic tendency to overgeneralize rules."

6.2 A Record of Attested Structures

Cowie proposes another substitute for (direct) negative evidence: The non-occurrence of predicted strings. She suggests that "the non-appearance of a string in the primary data can legitimately be taken as constituting negative evidence" (p.223). For example, people don't produce utterances like (47b):

- (47) (a) The table is tough to dance on.
 (b) *On the table is tough to dance.

To exploit this fact about adult usage, children would need to (keep accurate records of and) combine the rules or structures previously used to generate/interpret expressions with a sensitivity to the *absence* of strings like (47b). This requires representing absences; therefore, it presents a resource problem: Unless children know in advance which absences to look for, they would have to maintain records for all kinds of information that can be extracted from the input, including *much* information that will prove irrelevant for grammar formation. Since the grammatically relevant information is often subtle, children would have to be constantly recording potential cues -- and determining their relevance.

For example, consider how children would represent the absence of *wanna*-contraction in the kinds of Wh-questions discussed in section 4 (i.e., where contraction is prohibited when extraction is from the subject of an infinitival complement clause). Children would need to distinguish between word strings that differ in the nature of the Wh-phrase; as we saw, *wanna*-contraction is permitted in 'why' (adjunct) questions, but not in 'who' or 'what' (argument) questions. In 'who' and 'what' questions, moreover, *wanna*-contraction is permitted if the verb following *wanna* is transitive, but only if the site of extraction of the Wh-phrase follows the verb, rather than precedes it. Therefore, children must encode the distinction between subject and object position, as well as between transitive and intransitive verbs. In the simplest case, children would require a learning algorithm that operates on labeled strings of words of length 6 (Wh-phrase, auxiliary verb, subject NP, *want*, *to*, verb).

To take another example, adjectives and verbs have different 'control' properties, as illustrated in (48).

- (48) (a) John is easy to please. (*John is easy __ to please Mary)
 (b) John is eager to please. (John is eager __ to please Mary)

In (48a), 'John' is semantically associated with the object of 'please' (cf. *It is easy for us to please John*). In (48b), 'John' is semantically associated with the subject of 'please'. What statistical information lets children abstract this fact? And how *many* regularities would children have to consider to find the relevant ones? Similarly, auxiliary verbs are raised in English, but main verbs are not. Does this mean that young children keep records of which verbs raise?

Learners would also need to keep records of 'higher-level' categories, to form the correct generalizations about displacement operations. For example, the learner cannot take extraction from a position in main clause as evidence that extraction is also possible from that position in a subordinate clause, because some languages only permit extraction from main clauses. Similarly, auxiliary verbs are raised in main clauses, but not in subordinate clauses. And one can topicalize a prepositional phrase, but not a tensed verb phrase: *I said John would run into the room, and into the room he ran*; but cf. *I said John ate beans and eat (*ate) beans he did*. As these examples indicate, in the absence of an account of how linguistic absences could affect the child's grammar, Cowie's suggestion seems to imply that children record all possible grammatical distinctions, and then figure out which one's adults use. But not even trained linguists proceed in anything like this exhaustive fashion (see Fodor and Crain [1987, p. 51]).

Of course, we cannot *prove* that this substitute for negative evidence is not utilized by children. But we think it is a highly implausible solution to the logical problem of language acquisition. It *might* be that children are gifted linguists (but not gifted physicists or musicians) who *all* construct the *same* terrific theory, despite their different histories and intellectual powers. But we see no reason to believe this apart from an *a priori* commitment to empiricism.

6.3 The Primary Linguistic Data

One might think that we are being overly pessimistic about the data available to children. Empiricists often point to caretaker speech (sometimes called "Motherese") as special sources of evidence for learners. Cowie cites literature showing that caretakers simplify their speech to children. There are almost no grammatical errors, and caretakers provide cues that make structures transparent, including rising intonation. But as Newport, Gleitman and Gleitman 1977, p. 112) remark:

"the finding that Motherese exists cannot by itself show that it influences language growth, or even that this special style is necessary to acquisition—despite frequent interpretations to this effect that have appeared in the literature. After all, Motherese is as likely an effect on the mother by the child as an effect on the child by the mother."

However, while Cowie stresses the characteristics of parental input that have been called "intelligent text presentation," (Levelt, 1975, p 15), she denies that parents avoid 'complex' structures. She contends that parental speech to children contains abundant evidence of sentential embedding, including examples of the sort needed to confirm (say) the adult rule for forming Yes/No questions. In support of this claim, Cowie appeals to a study by Pullum (1996) based largely on the Wall Street Journal. While acknowledging that the Wall Street Journal (WSJ) is not an ideal source of evidence about the constructions young children encounter, Cowie maintains that such sources "may nevertheless be considered representative" of the primary linguistic data (p. 186). Cowie cites the following quote from Pullum:

"[we] have no reason to assume that we will get an unrepresentative sample of the syntactic types of questions that would come up in natural contexts in front of children if we simply look for question marks in the WSJ corpus. Speakers of English simply do not have enough conscious control over the syntactic properties of the questions they ask to make such a source unrepresentative." (Pullum, 1996, p. 507).

The conclusion that the Wall Street Journal is representative of children's input hinges on the assumption

that caretakers do not simplify their speech to children. However, it is pertinent to note that empiricists have argued against innate linguistic knowledge, on the grounds that caretakers do simplify their speech to children. For example, Brown (1977, p. 20) remarks that

"... it has turned out that parental speech is well formed and finely tuned to the child's psycholinguistic capacity. The corollary would seem to be that there is less need for an elaborate innate component than there at first seemed to be."

A similar claim is made by Horning (1969, p. 15-16):

"[t]he child is not initially presented the full adult language he is ultimately expected to learn. Rather, he is confronted with a very limited subset, both in syntax and vocabulary, which is gradually expanded as his competence grows. We should not expect our inference procedures to perform well when confronted directly with complex languages."

Another example, from Levelt (1975, p. 20), summarizes studies by Sachs et al. (1977), and Snow (1977):

"From these studies it appears that adults in addressing children use short, simple sentences with little embedding... It should ... be obvious that from the purely syntactic point of view the urge for strongly nativist assumptions has been diminished by these findings."

Cowie and Pullum reach just the opposite conclusion, claiming that caretaker speech to children is *complex*, thereby reducing the need for innate principles of grammar. Skeptics might begin to wonder just how empirical the arguments for empiricism are. Of course, Cowie is not responsible for past empiricists; and we cannot prove that parents do not effectively read the WSJ to children, and that children, in turn, do not effectively scour the text for any potentially relevant information. But we would like to see some reason for thinking that any of this is *true*. More importantly, we would like to see how the WSJ would help 2-year-olds acquire grammars with the *specific* constraints that are respected by 3-year-olds.

7. Experience-dependent learning algorithms

Nativists are often accused of unfairly assuming that their opponents do not endow learners with mechanisms for determining the deep regularities (and unobservable syntax) of language. Cowie claims "that empiricist approaches to learning possess resources that have hitherto not been properly appreciated..." (p. ix). So we briefly consider some of these resources, the general character of which, we think, has been properly acknowledged by many nativists.

The empiricist approach to learning assumes a corpus-based, inductive approach to learning. This approach to language-learning maintains that children's grammatical hypotheses are securely tied to their primary linguistic data. Let us refer to this class of models as the Input Matching model. The Input Matching model places little, if any, emphasis on innately specified linguistic knowledge as a source of children's grammatical hypotheses. Instead, general-purpose learning algorithms are assumed to underlie language learning, as well as other cognitive processes. One example of the Input Matching model is the Competition Model of MacWhinney and Bates (1989); the general idea is relatively clear from Hume (1739).

According to the Competition Model, a learner relies on 'cues' from the input to form simple recurrent networks, which are designed to attend to transitional probabilities in the input (how likely one item is to follow another); these networks are the hypothesized 'grammars.' The networks are sensitive to statistical 'information' or 'cues' inherent in speech. Examples of cues include word order, morphological agreement between linguistic items, and semantic plausibility. The learner comes to place more or less weight on different cues according to their availability in the linguistic environment. These differences allegedly account for cross-linguistic variation and for variation among speakers of the same language.

Recent research findings demonstrate children's sensitivity to at least some statistical and distributional properties of the linguistic input. Cowie cites a study by Read and Schreiber (1982), which showed that 7-year-olds are sensitive to structural notions like subject noun phrase, as long as the phrases contains more than one word. Cowie also cites Saffran, Aslin and Newport (1996), who showed that infants can learn word boundaries by attending to statistical properties of the input. This is of genuine interest. But Cowie's subsequent argument appears to be as follows: (i) there is evidence that children are sensitive to "statistical or distributional properties of linguistic inputs during language learning (p.192);" (ii) children are sensitive to structural notions like subject noun phrase; so (iii) there is "good evidence that they are perfectly well able to acquire the 'abstract' syntactic concepts that they need to form [structure-dependent] hypotheses through statistical analysis of the speech they hear around them (p. 193)." This is a non-sequitur, without the question-begging assumption that the statistical sensitivities children possess *are relevantly like* the statistical sensitivities they would need to possess in order to learn the *specific* grammatical constraints in question. (Perhaps this is not the argument Cowie intends. She also suggests agnosticism in various places; and we agree that the falsity of empiricism has not been demonstrated. But we don't think the absence of *proof* is reason for agnosticism.)

In any case, no one doubts that children use experience-dependent mechanisms to learn some aspects of language. Children who grow up in an English-speaking community learn to speak English; those who grow up in a Basque-speaking community learn to speak Basque. Saffran, Aslin and Newport showed that 8-month-old children could exploit statistical learning mechanisms to extract information about transitional probabilities from the input. Infants inferred the existence of word boundaries between three-syllable nonsense "words," by using some such experience-dependent mechanism. Sequences of syllables that crossed a word boundary were not treated as a "word" during the post-test phase, because there was a lower probability for a sequence of syllables to be repeated if it crossed a word boundary than if that sequence was part of a "word." According to Cowie, it is reasonable to infer that syntax can be acquired using the same kinds of statistical learning mechanisms.

In evaluating such claims, it is important to know what experience-dependent mechanisms cannot do. Connectionist or parallel distributed processing networks rely on local regularities -- i.e., changes in the "connection between one unit and another on the basis of information that is locally available to the connection" (McClelland and Rumelhart 1986, p 214). According to McClelland and Rumelhart (1986, p 214), such models "provide very simple mechanisms for extracting information from an ensemble of inputs without the aid of sophisticated generalizations or rule-formulating mechanisms." But in a recent series of papers, Gary Marcus (1998; 1999; also see Smith, 1996) has shown that while such mechanisms are capable of extracting information about transitional probabilities, they are ill-suited to learning many other properties of languages.¹⁵

There is a second inadequacy with experience-dependent learning mechanisms that rely on

¹⁵ For example, at the earliest stage at which children can be tested, they are found to assign an exclusive-or interpretation to sentences with disjunction in appropriate linguistic contexts (Chierchia, Crain, Guasti and Thornton 1999). Marcus (1998) demonstrates that the simple recurrent network included with the handbook to *Rethinking Innateness* (Plunkett and Elman, 1997) cannot model the parity function, which is equivalent to the exclusive-or interpretation. (The function to be learned yields the answer "true" just in case there is an odd number of 1s in the input; e.g., 10 is "true" but 11 is "false"). Marcus trained the network on 15 of the 16 possible inputs in a four-bit version of the problem. When the model was tested on the 16th pattern, the parity function (= exclusive-or) was not generalized to the novel input in most instances. More likely than not, the network responded incorrectly. For example, suppose the novel input was 1111. The correct response is "false" as anyone can see. However, the network responds "true" because this response was given to all of the inputs that were most similar to the novel input during the learning phase (e.g., 1110, 1101, 1011, 0111).

localist error-correction algorithms, such as the back-propagation algorithm. In extracting information based on local connections, these mechanisms do not generalize beyond the training set. Consider the formation of past tense using *-ed*. The Rumelhart and McClelland (1986) model applied generalizations, in much the way children do, to many novel items that resembled words that had appeared in the training set. For example, suppose that *sing*, *sang* and *walk*, *walked* were in the training set. The outputs for the novel inputs *malk* and *spling* were *malked* and *splang*. Marcus notes, however, that when the novel inputs did not resemble items in the training set, the outputs were nothing like those that humans would produce: the past tense of the novel word *frilg* was *frezled*, and the past tense of *ploanth* was *bro*. This last example is particularly insightful. Presumably, the novel input form *ploanth* is quite unlike anything in the training set. Consequently the experience-dependent learning algorithm has difficulty associating a past tense form to it. Its best guess is that the past tense form is quite unlike anything in the training set; the result is *bro*.

It is also worth noting that experience-dependent learning mechanisms can form generalizations that humans cannot form. Consider again the Read and Schreiber study. As Cowie points out, 7-year-old children are sensitive to structure-dependent aspects of language. Read and Schreiber also showed that 7-year-olds children cannot learn structure independent rules, like 'drop the first four words of a sentence'. Similarly, Smith and Tsimpli (1995) showed that adults are unable to learn a structure-independent rule of emphasis formation. To the extent that experience-dependent learning mechanisms can form structure-independent generalizations, they are quite unlike humans. If children and adults cannot (in any natural way) form structure-independent generalizations, this also casts serious doubt on Cowie's suggestion that children initially form structure-independent *and* structure-dependent hypotheses, later expunging structure-independent hypotheses on grounds of empirical coverage. There is no reason to believe that children form structure-independent hypotheses at *any* stage of language development. As Smith (1996, p. 7) concludes, "structure dependence is the prerequisite to, not the outcome of, language acquisition."

As we have stressed, we don't see how statistical probabilities of occurrence provide a basis for learning constraints. Experience-dependent learning algorithms fail to explain several other linguistic phenomena. They also fail to explain how children set linguistic parameters to values that generate linguistic structures that are not attested in the input. (We provide an illustration shortly.) In addition, such algorithms are sensitive only to 'local' statistical regularities, whereas human languages exhibit various 'non-local' dependencies. Another example of a 'long distance' dependency is the displacement of a Wh-phrase from a verb phrase with which it must agree in number:

- (49) (a) Which leader_i did Clinton say Milosovic thinks is friendly to Serbia.
 (b) Which leaders_i did Clinton say Milosovic thinks are friendly to Serbia.

In short, experience-dependent models can learn some things that children learn, but this is as far as the analogy goes. Such models are evidently incapable of learning some things that children do learn, and they are evidently capable of learning things that children cannot learn.

8. The continuity hypothesis

We have spoken throughout of children achieving a stable state, G_L , which is an adult grammar. But what of the less stable states children pass through in the course of acquiring the target grammar, G_L ? Initially, one might not think of these states of transition as grammars. For it can seem that children do not display full competence in any human language; at best, they have an imperfect grasp of the local language. From the perspective we have been urging, however, many aspects of adult grammar are innate and in place at a very early age. These innate linguistic principles define a space of possible human languages -- a space the child explores, influenced by her environment, until she stabilizes on a grammar equivalent to that of adults in her linguistic community

If this is correct, then at least as an idealization, language acquisition is a process of language

change. At any given time children are speaking a possible human language, just not the language spoken around them. (Cf. Chomsky's 1986 rejection of E-languages.) Even if the known adult grammars constitute only some of the possible human grammars, one expects to find that many "childish linguistic errors" arise because children are trying out grammars with features found in adult languages elsewhere on the globe. If this expectation is confirmed, it provides dramatic support for nativists. Learning theorists will be hard pressed to explain why English children should produce constructions exhibited in (say) German but not English--especially if children do not produce constructions of the sort that would violate (say) the Head Movement Constraint. On the other hand, nativists can make sense of children in Kansas trying out languages that are not English, yet never trying out languages that violate principles of Universal Grammar; similarities between child-English and adult-German are as unsurprising as similarities between cousins who have never met. So while differences between child language and adult language might initially seem to tell against nativism, faith in nativism leads to the discovery of surprising facts that learning theories would never expect.¹⁶

On this view, children's "errors" are not simply failures to match adult input. Indeed, talk of "errors" here may have outlived its usefulness, except as a way of noting that a child's course of language acquisition--achieving the stable adult state--has not yet ended. In a more interesting sense, children are not merely speaking adult English badly; like monolingual speakers of Japanese, they are speaking a foreign language. An example of children's non-adult (but UG-compatible) productions is the "medial-Wh" phenomenon. Using an elicited production task, Thornton (1990) found that about one-third of the 3-4 year-old children (of English-speaking parents) she interviewed consistently inserted an 'extra' Wh-word in their long-distance questions, as illustrated in (50) and (51) (Crain and Thornton, 1998; Thornton 1996).

(50) What do you think what pigs eat?

(51) Who did he say who is in the box?

This "error" by English-speaking children is presumably not a response to the children's environment, since medial-Wh constructions are not part of the primary linguistic data for children in English-speaking environments. However, structures like (50) and (51) are attested in many languages, such as Irish and Chamorro (Chung 1994). An example from German is given in (52).

(52) Wer_i glaubst du wer_i nach Hause geht?
'Who do you think who goes home?'

The medial-Wh phenomenon accords with the continuity hypothesis. And further investigation shows that this similarity to a foreign language runs deep. For both adult Germans and American children, lexical Wh-phrases cannot be repeated in the medial position. German-speaking adults judge (53) to be unacceptable, and English-speaking children never produce strings like (54), as indicated by the '#.'

(53) *Wessen Buch_i glaubst du wessen Buch_i Hans liest?
'Whose book do you think whose book Hans is reading?'

(54) #Which Smurf do you think which Smurf is wearing roller skates

¹⁶ When Newtonians used the apparently anomalous orbit of Uranus to correctly predict the existence and location of Neptune, that was a serious confirmation of the theory. Similarly, it tells favor of nativism if children go through stages of similarity to adults in other countries.

Instead, children shorten the Wh-phrase or omit it altogether, as in (55).

(55) Which Smurf do you think (who) is wearing roller skates.

Similarly, children never used a medial-Wh when extracting from infinitival clauses. Nor is this permissible in languages that permit the medial-Wh.

(56) Who do you want who to win?

Indeed, insertion of medial-Wh in infinitival complements is universally ungrammatical. So in German, a long-distance structure would be used:

(57) Wen versucht Hans anzurufen
'Whom is Hans trying to call?'

This complex pattern of linguistic behavior suggests that many children of English-speakers go through a stage at which they speak a language that is like (adult) English in many respects, but one that is like German (Irish, Chamorro, etc.) in allowing for the medial-Wh. There is nothing wrong with such a language; it just so happens that adults in New York and London do not to speak it. But it is quite striking that the children of such adults *don't* emulate their parents in this respect. Instead, the children speak like foreigners for a little while. Yet these children don't diverge from their parents with respect to the Binding Theory, the Head Movement Constraint, and many other linguistic constraints. Once again, empiricists owe an account of why children project beyond the data *in certain ways but not others*.

A systematic development of the evidence for the continuity hypothesis is beyond the scope of this paper. Our aim here has been to note a rich source of data, ignored by Cowie and others, that nativists can draw on to provide independent support for their postulation of innate linguistic principles that define the space of possible human languages. Absent plausible alternatives for why children produce medial-Wh constructions, it seems that children project UG-compatible hypotheses, rather than formulating hypotheses on the basis of linguistic experience. Far from disconfirming nativism, the non-adult linguistic behavior of children suggests that children do not attempt to match their hypotheses to the input (as advocates of empiricist approaches to learning contend). Rather, linguistic input seems to guide children through a space of hypotheses made available by Universal Grammar, subject to the (severe) constraints of Universal Grammar. In the course of language development, children are free to project unattested hypotheses, so long as incorrect hypotheses can later be retracted on the basis of the positive evidence. Again, experience matters, but not because children induce the adult grammar from the primary linguistic data. Rather, these data are part of what determines the (normal) child's path through an innately specified space of languages, until the child hits on a grammar sufficiently like the grammars around her, with the result that further data no longer prompts further language change.

9. Conclusion

According to nativists, children acquire stable states (of linguistic competence) that are significantly underdetermined by experience, even given optimistic assumptions about children's (nonlinguistic) capacities to formulate and test hypotheses. If this is correct, language presents theorists with an instance of Plato's Problem: How can humans know so much, in some domains, given so little evidence? Chomsky (1986) explicitly draws a parallel between children acquiring language and Meno's servant, who quickly demonstrates mastery of certain geometric theorems, given suitable prompting. No human has ever seen a (perfect) triangle; yet any normal thinker can come to see that the Pythagorean Theorem is true. Similarly, lack of experience with large numbers does not keep one from knowing that there are infinitely many primes. Such knowledge seems to come "from within;" it seems to be rooted in cognitive resources, common to all human thinkers, as opposed to environmental experience. Nativists argue, citing poverty-

of-stimulus considerations, that many aspects of natural language grammars belong in this category.

There is, however, a recurrent temptation to think that language presents theorists with an instance of Hume's (or Goodman's) Problem: How can thinkers ever (justifiably) settle on a generalization that goes beyond the data, even given a lot of evidence? Given a thousand green emeralds, and no nongreen emeralds, it seems reasonable to infer that all emeralds are green. But as Hume (1739) stressed, any such inference will be nondemonstrative. Future experience might, without being 'contrary to reason', reveal any number of blue emeralds. Nor can one simply declare that inferences of the following form are reasonable: Many Es have been observed, and all the Es observed to date are G, so all Es are Q. Replacing 'E' with 'emerald' and 'G' with Goodman's (1965) predicate 'grue' yields a manifestly unreasonable inference. (An object x is grue, iff x is green and examined prior to 2001 or x is blue. Hence, all emeralds observed to date are grue; yet many emeralds--i.e., those that will not be examined before 2001--are not grue.)

Cowie points out, in various ways, that generalizing typically involves going beyond the data in nondemonstrative ways; hence, the mere fact that children go beyond the data in nondemonstrative ways does not show that children are not acquiring language by generalizing on the data they receive. This is true, but irrelevant. Induction problems are *not* good analogies for acquiring grammars with the features that nativists emphasize. For the problem does not concern an inference from a large body of data that intuitively confirms a generalization G to acceptance of G. The problem concerns acceptance of a generalization whose epistemic relation to the data is tenuous at best. In this regard, analogies to geometry are apt: Experience can play a triggering role, without providing any basis for induction. Of course, language differs from mathematics, in that some aspects of grammars are learned; and if one focuses on those aspects of grammar that are plausibly extrapolations of the primary linguistic data, it is tempting to think that children induce their grammar from the input. But even if many aspects of grammar are induced, other aspects appear not to be.

In short, the existence of Humean/Goodmanian extrapolation does not establish nativism, but nativists do *not* merely point to such extrapolation.¹⁷ The nativist argument concerns particular features of grammar -- e.g., conformity to the principles of Binding Theory, or the Head Movement Constraint -- such that the PLD does not provide any plausible basis for extrapolation to grammars with these features. (Thus, the arguments for linguistic nativism are *much* better than the arguments for curry nativism.) One can challenge the nativists' premises about human linguistic competence, and/or the PLD utilizable by normal children. But given these premises, motivated by the daily work of descriptive linguistics, language acquisition is not merely a matter of Humean/Goodmanian extrapolation. So one cannot reply to Chomskian nativists by noting that empiricists allow for induction. As we have seen, children apparently present linguists with an instance of *Plato's* problem; grammars project beyond the PLD in ways that the PLD do not even suggest. Until empiricists show how specific principles -- like the Head-Movement Constraint and the Binding Theory -- can be learned on the basis of the primary linguistic data, innateness hypotheses will continue to be the best available explanation for the gap between normal human experience and the linguistic knowledge we all attain.

¹⁷ Nor is the nativists' argument that certain principles are respected in all known languages, and that this implies innateness. There are any number of reasons why a feature might be exhibited in all languages. But if all *learners* respect the same substantive *constraints* not evidenced in their PLD, that is an argument for innateness. Some empiricists may insist that all knowledge is a posteriori. But given (*inter alia*) logic and mathematics, we see no good reason for adopting this view. While Quine (1953) justly criticized a certain conception of the analytic, he provided no good argument for the nonexistence of a priori knowledge; see Rey (1998).

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