Chapter Five
Considering Linguistic Knowledge

At the end of the previous chapter, we noted that there may be principles underlying the acquisition of the lexicon that are not obvious at first glance. But isn't that always the case in science? Who would have thought at the beginning of this century that general principles of physics govern the very way that all materials, living or otherwise, are constructed, or that principles of neurobiology control the very makeup of all living matter? From that perspective, it is perhaps not surprising that some rather non-obvious principles govern something as ubiquitous as the human's implicit knowledge of language.

This chapter is a pause of sorts from our usual head-long rush into technical detail, for here we cover some basic concepts and a few insights from language acquisition. These are important—indeed, have been important all along (except that we didn't have time to bring them up)—because, as we shall see, for some linguists, they guide the very nature of linguistic theorizing, the general purpose of which is to ascertain what one's implicit knowledge of language looks like in the first place.

1. Basic Concepts

1.1 Implicit knowledge of language

All along in the course, we've been making reference to the native speaker's knowledge, where with "native speaker" we mean *any* native speaker, not just one who happens to have had a lot of schooling, or one who can even read and write. But let's be more precise now. When we speak of the native speaker's *implicit* knowledge of language, we refer in linguistics to knowledge that many speakers may not even be aware of at all (hence implicit). Further, when we speak of this knowledge of *language*, we refer not just to any kind of knowledge that happens to be used in language, but only a part of it. Specifically, we refer here only to the speaker's implicit knowledge of phonology, morphology, and syntax.

It may be useful to consider this last point in more detail. In previous chapters, we have become acquainted with what is believed to comprise the speaker's knowledge of phonology and morphology. (As we proceed into chapter six, we will become familiar with what many linguists consider to constitute the speaker's knowledge of syntax.) But what about this other knowledge, the type that might be used in language, but that we exclude from our discussion.

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Consider, for example, this: When humans speak to one another (i.e., engage in conversation), they intuitively know that they ought to contribute to the conversation only information that is relevant to the conversation. Now, what one person considers relevant to a topic may, of course, differ greatly from what the next person considers to be relevant,[1] but this does not detract from the point that both (or all) parties realize that relevance plays a major role in keeping conversations going. In other words, speakers know, implicitly, that in order to keep a conversation going, they must obey what the philosopher H. P. Grice once called the ***Maxim of Relevance***, which says nothing more than "be relevant."

Would anyone seriously doubt that speakers follow Grice's maxim? Probably not. But note what this maxim explains—or, better, what it *doesn't* explain. Recall, for example, that native speakers of English consider the phoneme /p/ to have the allophones [p^h] and [p] while native speakers of Khmer consider these two sounds to be allophones of different phonemes: [p] is an allophone of /p/, and [p^h] is an allophone of /p^h/. In other words, the English speaker's implicit linguistic (here: phonological) knowledge differs from the Khmer speaker's implicit linguistic (here: phonological) knowledge. So how do we explain this knowledge? How could one take a principle like the Maxim of Relevance—or any other such general principle—and explain such knowledge? In fact, it is extremely difficult even to imagine how such general principles could explain such important linguistic facts. Indeed, in the view of many linguists, it's simply impossible to do so.

The general claim in linguistics, then, is that native speakers possess a level of linguistic knowledge that is entirely implicit and that cannot be explained by reference to general principles of language use. Within linguistics, the widely-accepted label for this special kind of linguistic knowledge is ***competence***.[2]

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[1] I might note in passing, too, that relevance of this sort varies from culture to culture.

[2] Note that the term "competence"—as used in linguistics—certainly differs from the everyday use of "competence." Hence, a potential employer might wind up asking whether an applicant has "competence in word processing"—plainly different at many levels from the linguist's reference to "competence."

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1.2 Autonomy

According to those who work in the dominant paradigms of linguistics, principles like Grice's maxims cannot explain such knowledge. Indeed, their view is that one's knowledge of phonology—and of morphology and syntax—is governed by principles that have no correlates whatsoever in the principles that govern general human behavior. In other words, the principles that govern the speaker's knowledge of phonology, morphology and syntax are specifically linguistic in nature: *These principles govern the speaker's knowledge in only these areas; these principles are not just special cases of the more general principles that govern other aspects of human behavior or knowledge*. This view is known as the ***Autonomy Thesis***.

There is, in fact, good reason to question the autonomy thesis. After all, proposing such a thesis amounts to a violation of the ***principle of parsimony*** (also sometimes known as "Occam's Razor"), which limits scientific thinking to the simplest possible solutions. Think about it: The simplest solution would be to claim that linguistic cognition is no different than any other kind of cognition while the autonomy thesis claims that humans are equipped with (at least) two different kinds of cognition, one of which is specialized only for linguistic knowledge.

How could one ever prove that linguistic knowledge is autonomous in the mind/brain? There are, in fact, several arguments to this effect, but one of the strongest (in the opinion of some) involves dissociation. Consider this: If linguistic knowledge really is separate from the rest of cognition, then selective damage to the brain should yield cases where linguistic cognition is intact while other cognition is kaput, or where linguistic cognition is damaged while the rest of cognition is intact. And, in fact, a good number of such cases have been located. Some examples:[3]

1. A child with a brain injury (congenital left-hemispheric lesion) acquiring Hebrew: The child had difficulties with word choice, the appropriateness of language use, factual knowledge, but few difficulties with syntax or morphology.

2. A child with Down's syndrome: Severe cognitive deficits, but very good control of her native French.

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[3] Evidence of this type has been mounting steadily as linguists have realized what to look for, namely, dissociations between linguistic cognition and other forms of cognition. Those wanting more information of this kind might turn to H. Tager-Flusberg, ed., *Constraints on language acquisition* (Hillsdale, NJ: Lawrence Erlbaum, 1994). Pinker's book also provides a number of other such sources.

3. A child with an IQ of 57: Excellent in all aspects of her language (syntax, morphology, phonology, and so forth).

4. Autistic children: Many severely deficient in social interaction, yet unimpaired in their grammatical development.

What do examples like these indicate? For many analysts, such examples suggest that linguistic cognition can exist in the mind/brain independently of the rest of cognition. And, of course, that's exactly what the autonomy thesis predicts.

It is, though, one thing to say that principles are autonomous, yet quite another to say that they are inborn, that is, that they comprise a part of our ***innate*** endowment. Yet this is exactly what many linguists conclude about the language capacity. Later in this chapter, we'll return to this proposal and provide the kind of logic that leads to it.

1.3 The extent of native-language knowledge

An aspect of our implicit knowledge of some language that is easily overlooked is its extent. On the one hand, our human brains have a finite capacity for storage. We just can't learn and remember everything (a fact that students in linguistics classes keep on reminding their linguistics professors about just around the time before examinations). Yet, even though we do not have infinite memory, we do have the capacity to understand and produce an infinite number of new sentences. That is, all native speakers have the capacity to produce and understand literally millions of sentences that they've never seen or heard before.[4]

So what might one conclude from this capacity? One conclusion is that *we couldn't possibly just memorize every sentence of a language since that would outstrip the finite capacity of our limited brains rather rapidly. Instead, we must be endowed with some special capacity that will generate this infinite number of sentences from a finite set of principles or rules.*

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[4] Those who want proof of this might try this: Go to the library and start picking up books you've never read and turning to pages randomly. Read the first sentence at the top of the page and ask the following question: "Have I seen this sentence before?" (The answer, in all likelihood, is "no.") Repeat this process until such time that you are convinced that you have an infinite capacity to understand sentences you've never seen before.

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2. What native-language acquisition looks like

Between the ages of approximately 18 months and 4 years (there is fluctuation), children exposed to language in some speech community will become recognizable as native speakers of the language of that speech community. In the course of development, it turns out that children display an almost remarkable degree of uniformity in the "errors" they make.

2.1 X-word stages

Let us return first to the age of acquisition. As hinted above, there is a good deal of fluctuation in the ages at which different children will acquire the same aspects of grammar. And because of this fluctuation, researchers searched for some better way, besides age, to be able to compare one child to the next. One very interesting and oft-used means of comparison is known as the ***Mean length of utterance*** or MLU.

In order to get an MLU measurement on a given child, we count the number of morphemes (including bound morphemes!) that that child produces per "sentence" for about 100 (or so) sentences running, uttered on a single day. Then we divide that total number of morphemes by the total number of "sentences" (100 if it's 100 "sentences"). The figure we wind up with—it might be something like "2.3" or "1.9"—is the MLU of that child at that point in time. And, as it turns out, MLUs are a relatively stable means of comparing one child's development to the next child's development without having to refer to the children's ages. (To be sure, there *are* problems with MLUs, one of which we'll turn to in just a few moments ...)

With this means of comparing development across children, it becomes possible to observe stages of development. In particular, the following, MLU-based stages appear across the board in language acquisition:

2.1.1. ***Babbling stage***. The child babbles. These babbling sounds are human-language sounds: They comprise recognizable vowels and consonants. Interestingly, the set of human-language sounds that children produce during this stage is a superset of the set of sounds present in the child's yet-to-be-acquired native language.

2.1.2. *One-word stage*. During this period, the child's linguistic production averages at around a single word (i.e., MLU is at 1 or a little above).

2.1.3. ***Two-word stage***. During this stage, production comes close to two words (i.e., MLU is around 2). This is sometimes referred to as the ***telegraphic stage*** because it might resemble the kind of very stripped-down language that an adult might produce if she had to pay a small fortune for every word (as when sending a telegraph). Most linguists who work on acquisition argue that the two-word stage is, in fact, the first stage of syntactic development (even though there are some psychologists who dispute this claim).

2.1.4. "The all-hell-breaks-loose stage." There is no official name for this stage of development. It isn't a "three-word stage" because what we find here is huge variation, with some children jumping from the two-word stages to much, much longer utterances. (Pinker is the one who came up with the quaint label we employ here.)

As noted above, there are problems with MLUs as a way to compare children. One of the more interesting problems with MLU came about when linguists began comparing the acquisition of different languages, in particular, the acquisition of morphology-poor languages like English to the acquisition of morphology-rich languages like German. What they found was that MLUs don't work in comparing the linguistic development of children acquiring different languages (primarily because, surprisingly enough, the inflectional morphology of richly-inflected languages is acquired a good deal *earlier* than the inflectional morphology of poorly-inflected languages).

2.2 Morpheme orders

If we were to stop with the X-word stages of acquisition, though, we'd be missing some of the most interesting parts. Early on in acquisitional research (the 1960s), linguists began to examine the acquisition of various grammatical function words (the closed-class morphemes). And what they found was that these morphemes appear to be acquired in largely the same order, regardless of the child and regardless of the learning environment (including parental education, socioeconomic class and so forth). Perhaps the most well-known of these ***morpheme-order*** studies is Roger Brown's *A First Language* (Cambridge, MA: Harvard University Press, 1973), in which he documented the following order on 14 closed-class morphemes for three unrelated children (Adam, Eve and Sarah) acquiring English:

1. Present progressive (-*ing*)
2.-3. *in, on*
4. Plural (-*s*)

5. Past irregular (e.g., *went*)
6. Possessive (e.g., *dog's tail*)
7. Uncontractible copula (*is, am, are*)
8. Articles (*the, a*(*n*))
9. Past regular (*-d*)
10. Third-person regular (*-s*)
11. Third-person irregular (*does, has*)
12. Uncontractible auxiliary (*have, has, is*)
13. Contractible copula (e.g., *she's here*)
14. Contractible auxiliary (e.g., *she's running*)

Clearly, something interesting seems to be going on here. Alas, early attempts to explain the morpheme order above were not terribly successful. For instance, Brown himself examined whether the frequency of parental use of these morphemes could explain their order of acquisition. (The idea would follow the intuitive principle, "most used by parents = first acquired by child.") It didn't work at all. Contrary to what common sense would seem to dictate, there was hardly any relationship between frequency of use by caretakers and order of acquisition.

In fact, when we wrote above that early attempts to explain the morpheme order were not terribly so successful, one might be left with the impression that later, more recent research has been more successful. The answer is "no"—but for an entirely different reason. More recently, the general attitude is that the early studies examined what turns out to be a pretty motley collection of morphemes that don't necessarily have much to do with one another, anyway. Consider, for instance, *in* and *on* and the regular past morpheme (*-d*): Why should we even bother to compare these? They're completely different linguistic phenomena, *in* and *on* being prepositions and the other involving verbal morphology. As a result, what has been studied in more recent research involves only those morphemes that one would to be related anyway. One expects, for example, the past regular (*-d*) and the past irregular (e.g., *went*) might in some way be related, especially when we observe children acquiring English coming up with forms like *goed*. And, indeed, that relationship is what motivated the large amount of research that Steve Pinker and his colleagues have directed toward just these two.

So the early morpheme-order studies were both a hit and a flop. A hit because they certainly are surprising. And a flop because they wound up examining a heterogeneous set of morphemes that left little reason to look for internal relationships.

2.3 Developmental sequence

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When seen in isolation, then, the development of closed-class morphology (the morpheme orders discussed above) do not, in the end, appear to be that revealing. It is when we observe the development of such morphology in an appropriate context that matters become more interesting. Consider, for example, the English closed-class morphemes *do* and *no(t)*, used, often in combination with the agreement morpheme *-s*, to negate sentences (e.g., *She doesn't like broccoli*.). As it turns out, observation of children acquiring English yields that all of them pass through common stages of development as they acquire negation. The three stages that comprise such development are as follows:

Stage 1: "External" negation with *no*

No the sun shining.
No sit here.
No dog bite you.
No money
No Fraser drink all tea.

Stage 2: "Internal" negation without auxiliaries

I no singing song
The sun no shining.
Don't sit here.[5]
We can't talk.
I no want envelope.
I no taste them.

Stage 3: "Internal" negation with auxiliaries (including *do*)

I'm not singing.
The sun isn't shining.
It isn't cold.

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[5] This example appears to include the auxiliary-negation combination *don't*. In the adult language, of course, *don't* would be a contraction of *do* + *not*. In child language, however, there is at least some evidence to suggest that *don't* is initially picked up not as a contraction that includes the auxiliary *do*, but rather as a whole word. In other words, at this stage, uses of *don't* are not indication that children have picked up auxiliaries yet. Rather, for the children at this stage, *don't* would comprise a (whole) word with the meaning, 'negative.' To refer to such uses, developmental linguists employ the technical label ***unanalyzed form***.

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I don't have a book.

What makes such ***developmental sequences*** interesting, then, is that we are able to observe the development of grammar-related, closed-class forms like auxiliaries and negation in their appropriate context. Again, it turns out that such sequences appear to be a kind of universal: All children acquiring English as their native language pass through what appears to be the same sequence.[6]

Before leaving the developmental sequence, it is perhaps worthwhile to note what it might suggest about terms like "***error"*** (or "mistake"). When caretakers observe, say, an English-learning child saying things like *The sun no shining*, they typically think the child is making an "error." But is it? In fact, it appears to be a normal developmental stage, universal to all children learning English. In other words, from this perspective, the appearance of forms like *The sun no shining* is not an "error" at all, but rather just an indication of an absolutely normal developing system. (We turn below to the weighty question of whether children need to be corrected when they produce things like *The sun no shining*.)

3. What's necessary for acquisition?

That may seem like a simple enough thing to say, but a little logic and a few observations about the general situation reveal that such a simple thing starts looking more and more amazing.

3.1 Success rate near 100%

Consider success rates first. When we observe humans (children, adults) attempting to learn, we find a good deal of variation. So, when we observe humans trying hard to learn, say, computer programming (or math or history or garden care or automobile repair ...), a few of them wind up learning the material very well (they become "experts" at it); most wind up learning the material at some intermediate level (they're "OK" at it); and a few of them wind up hardly learning the material at all (they are "lousy" at it). Indeed, the infamous "bell curve" is based on just such normal performance, where some few succeed very well, most do just OK, and a few fail.

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[6] It is important to understand that such sequences are universal in the sense that all children, learning any language, will pass through them. The particulars of such sequences vary somewhat, however, from language to language, though not from child to child learning a particular language.

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But is all human learning governed by the "bell curve"? Consider walking. When we (Westerners) talk about things that toddlers must learn, we include walking in the list ("Johanna is just learning to walk!" "Janine learned to take her first steps today!"). So what makes walking so interesting? Consider the success rate. Do we find that some toddlers succeed very well ("Suzanne became an expert walker!"); some learn to walk at some intermediate level ("Robert only learned to walk at a sort of so-so level."); and some fail to learn to walk ("Poor Harry! He never learned to walk!")? Obviously not. The success rate for walking is evidently not at all like the success rate for, say, learning automobile repair.

Now consider becoming a native speaker of the language of one's speech community. Will a child raised entirely in the Bronx (an area of New York) become a native speaker of the Bronx dialect? Will a child raised entirely in Brownwood, Texas become a native speaker of that West Texas dialect? Will a child raised in South Central Los Angeles become a native speaker of the dialect common to the area? Or will these children (or any other children, for their respective speech communities) exhibit the "bell-curve" effect: Some succeed spectacularly in becoming native speakers of the language they're exposed to in childhood; others succeed only at some intermediate level; and some unfortunates just don't become native speakers at all.

If the point isn't clear enough, here it is: To the extent that children are exposed to language, they'll become native speakers of that language. And the success rate is nearly 100%.[7] As Steve Pinker once quipped, "The only way you can *stop* children from acquiring language is to grow them in a barrel!"

3.2 Data of exposure

So humans are remarkably successful at becoming native speakers, just as they are remarkably successful at becoming walkers. But what information do toddlers use to acquire this implicit knowledge? Obviously, children born into, say, a Portuguese-speaking environment isn't born already knowing Portuguese; rather, every scrap of evidence indicates that every child is fully capable of acquiring implicit knowledge of any human language that she is exposed to. As a result, it must be the case that these children get some sort of information from their

[7] We have to say only "nearly" 100% because some children are born with deficits like hearing impairments that make it impossible for them to hear or with severe brain abnormalities that makes it impossible for them to learn. We don't normally see many of these children, however; they're often either at clinics for specialized help (e.g., clinics for the hearing impaired) or in long-term care institutions, totally out of sight.

environment.

Information from the environment comprises an awful lot of ground, of course, and different kinds of information from the environment may be useful for different types of learning tasks. Here, of course, our interest is in the acquisition of a grammar, that is, the core areas of linguistic knowledge (syntax, morphology and phonology). We are not particularly interested in, for example, how the child learns that it is inappropriate to use "cuss words" in "polite company" (e.g., in front of parents). Suppose, then, for the acquisition of syntax, morphology and phonology, we attempt to divide up into types the information that children might make use of—and then examine each of these types with a bit of observation and logic.

3.2.1. ***Positive data*****.** This is the simplest imaginable type of information that a child might have from the learning environment: Just exposure to phrases and clauses in some context. It's not hard to imagine that nearly every child is exposed to positive data. What we do know, though, is that those who are not exposed to positive data during childhood do not acquire their native language.[8,9]

3.2.2. ***Negative data***. Unlike positive data, which tell the child what is possible in a language, negative data provide direct information on what is impossible in a language. Such data can take many forms, though a correction by a caretaker is the most usual case. (For the linguist, negative data are the real-world counterpart of her use of the asterisk, which tells other linguists what is impossible.)

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[8] Here and below we employ the terms positive *data*, negative *data*, indirect *data* and so forth. In fact, other technical discussions of these matters may use the terms positive *evidence*, negative *evidence*, indirect *evidence*, and so forth. For a discussion of the meanings and appropriateness of such terminology, see the following:

M. Beck, B. D. Schwartz & L. Eubank. 1995. Data, evidence and rules. In *The Current State of Interlanguage* ed. by L. Eubank, L. Selinker & M. Sharwood Smith, 175-193. Amsterdam: John Benjamins.

[9] There are, of course, the tragic cases like Genie, who—from very early childhood—was shut into a room, usually tied to a potty, and never spoken to by her sadistic parents and sibling. (Her father and brother did bark at her, as if she were a dog.) After her rescue from this horror at age 13, Genie made great progress in acquiring vocabulary, but her grammar has never approached anything like that of a native speaker. Pinker outlines a number of such cases in *The Language Instinct*; see his references for more information.

3.2.3. *Indirect data*. Note with positive data that the child can actually observe what is possible: What the caretaker says is positive data. Likewise, negative data are also observable: When a caretaker corrects a child's language, the child would be able to observe that the caretaker is making some attempt at change. By contrast, indirect data refer to knowledge that is gained by what the child does *not* observe in the environment. Suppose, for example, that the child observes caretakers saying something like *Where did you say who went?*. This observation would be information from positive data. However, the child does *not* observe caretakers saying things like *Who did you say where went?* Presumably, the child could conclude that *Where did you say who went*? is possible in the language because she saw the caretaker saying it. And the child could presumably conclude that *Who did you say where went?* is impossible in the language because the child does *not* observe anybody saying it.

3.3. Evaluating the three types of data

Common sense would suggest that children might use all three types of data in acquiring the grammar of their native language. Alas, common sense isn't always right. Here we will not evaluate children's use of positive data; it seems incontrovertible that they do (and cases like those of Genie demonstrate that this type of data had better be around, too). But what about negative data and indirect data?

3.3.1. Negative data evaluated. Is this kind of exposure (e.g., parental correction of phonological, morphological or syntactic errors) really necessary for acquisition to take place? In *Learnability and Cognition* (Boston: MIT Press, 1989), Steve Pinker looks at this question and notes the following: For negative data to play a necessary role in acquisition, these data would have to be available to all children; children would have to pay attention to these data (i.e., not ignore them); and these data would have to be usable by all children. (We speak of *all* children here because *all* children successfully become native speakers.)

Are negative data available to all children? Certainly it is true that some parents, especially in certain sociolinguistic strata, correct their children's errors in "pronunciation" and "grammar" (that is, what we would roughly call phonology, morphology, and syntax). It is, however, also true that some parents only rarely correct their children's errors, and there are even some parents who never correct their children's errors of this kind. And it doesn't seem to make a difference: All children, regardless of correction, nonetheless successfully become native speakers. Simple logic thus dictates: *Negative data on phonology, morphology and syntax are not a necessary*

prerequisite for acquisition to take place.

But even if such data are available, they would also have to be used, that is, not ignored, as Pinker points out. After all, it wouldn't matter if a caretaker gave negative data all day long, if the child ignored every bit of it. Yet there is fairly good evidence that children really do ignore these attempts at correction. Consider the following three passages.[10]

1. Child: Nobody don't like me.
   Mother: No, say "Nobody likes me."
   Child: Nobody don't like me.
   [dialogue repeated eight times]
   Mother: Now listen carefully, say "NOBODY LIKES ME."
   Child: Oh! Nobody don't likeS me.

2. Child: Want other one spoon, Daddy.
   Father: You mean, you want THE OTHER SPOON.
   Child: yes, I want other one spoon, please, Daddy.
   Father: Can you say "the other spoon"?
   Child: Other ... one ... spoon.
   Father: Say ... "other.".
   Child: Other.
   Father: "Spoon."
   Child: Spoon.
   Father: "Other ... spoon."
   Child: Other ... spoon. Now give me other one spoon?

3. Child: I turned the raining off.
   Parent: You mean you turned the sprinkler off?

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[10] These three passages are quoted from the following sources:

McNeill, D. 1966. Developmental psycholinguistics. In F. Smith & G. Miller, eds., *The genesis of language*. Cambridge, MA: MIT Press.

Braine, M. 1971. On two types of models of the internalization of grammars. In D. Slobin, ed., *The ontogenesis of grammar*. New York: Academic Press.

MacWhinney, B. & C. Snow: 1985. The Child Language Data Exchange System. *Journal of Child Language* 12:271-296.

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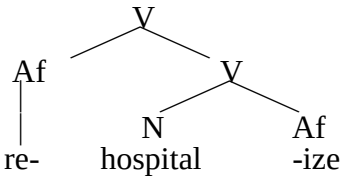
Child: I turned the raining off of the sprinkler.

What makes these passages interesting is that they show children either ignoring corrections (first two passages) or, when the corrections are not ignored, then creating such chaos that it seems clear that the correction should not have been attempted in the first place. The general point is, then, that children quite often ignore caretaker's attempts at correcting the errors in their developing grammars. *More generally, it thus appears that even if negative data are supplied (a questionable assumption), they appear to be ignored quite often.*

But what if negative data are both available and not ignored by the child? Again, Pinker points out that such data would have to be usable. In fact, usability is a fairly complex criterion, so it's perhaps best to exemplify it. Recall from our study of morphology that word structure may be represented as a series of rules. An example from the morphology chapter is repeated here for convenience:

-ize attachment: N + -ize → V
re- attachment: *re-* + V → V

With these examples applied to the word $hospital_N$, we considered the following hierarchical structure for *rehospitalize*:



With that background, now imagine a child acquiring English who comes up with the word *rehappize* (apparently meaning to "make happy again"). Where did this child go wrong? Suppose that the child has one of the rules from above wrong. So rather than having the adult-like rule for *re-* attachment (i.e., *re-* + V → V), the child has come up with a bizarre version:

child's bizarre *re-* attachment rule: *re-* + Adj → V

Obviously, with this rule, the child would have a grammar that would produce *rehappize*, and that grammar would have to be changed in order for the child to become like the native speakers surrounding her.

Now imagine the caretaker's response upon hearing the child's word, *rehappize*. One can grant full use of one's imagination on what kinds of corrections that the caretaker might come up with: "You can't say that." Or maybe "That's impossible." Or maybe "Well, how about if you say 'I made them happy again'." Or whatever! The much more crucial point is this: Given the information from the caretaker provides, how is the child going to zero in on the fact that it is the *re-* attachment rule that needs to be changed? After all, the child also has an *-ize* attachment rule operating here (as well as several dozen other rules that we've left aside for purposes of presentation!). The simple fact is that for the vast majority of parental corrections, the child is left with no particular way to make sure that she can go into the grammar and change just the right rule among the plethora of rules operating at any one time. *In other words, even if negative data were available (questionable) and not ignored (also questionable), it is still not particularly conceivable that negative data would even be usable.*

3.3.2. Indirect data evaluated. So children don't appear to require negative data in order to acquire the grammar of their negative language. What about indirect data? Recall from above that indirect data refer to information that the child does *not* observe in the environment. Are indirect data necessary for acquisition to take place? Again, it's not very plausible. There are, in effect, to two problems with this view, both involving memory (though in different ways).

Consider, then, the child who does not observe a sentence like *Who did you say where went?* in the learning environment. A fundamental problem here is the infinite capacity of native speakers discussed earlier: They all have the capacity to produce and understand an infinite number of new sentences, ones they've never seen before. But note that we now have a contradiction: How is the child to know that the absence of a particular sentence from the environment wasn't just an accident? That if she had just waited long enough, then the caretaker would have gotten around to uttering such a sentence (among the infinite number of other sentences that the caretaker might have uttered). And if so, when should the child stop waiting, given this infinite capacity? Only a few minutes? Surely not! A few hours? Probably not very wise. Days? Weeks? Months? Years?

There is, then, a fundamental problem with indirect data, namely, how (and when) to know that these data are missing by accident or by design. Related to it is a matter of the child's memory for grammar *per se*. In fact, we know from experimental studies of adults that even they don't have memory for the grammar of particular sentences. Once they hear a sentence, they forget its grammar and only remember its meaning.[11] And yet if adults have such poor memory for the

[11] For example, if you give adults something like *The woman was hired by Xerox*, they'll easily remember that "Xerox hired her" (i.e., what the sentence means), but they'll be harder pressed to

grammar of particular sentences, now can we expect a 3-year old to be much better? After all, we just noted above that the child might have to keep in mind that *Who did you say where went?* wasn't being uttered, and that the child might even have to keep this in mind for a good, long time! *The bottom line: It seems rather implausible to think that the use of indirect data might play a necessary role in the acquisition of the native-language phonology, morphology, or syntax.*

In summary, it appears that the only information from the learning environment that is really necessary for acquisition to take place is plain, simple positive data. That's not much, but that's precisely what modern theories of language acquisition assume to be necessary. It also provides us with an answer to the posed for ourselves above, namely, whether children need to be corrected when they produce things like *The sun no shining*. The answer, as far as anyone can tell, is "no."[12,13]

4. Innateness

So what about innateness? The claim in the dominant paradigms of linguistics is that the capacity to acquire human-language competence (i.e., implicit knowledge of phonology, morphology, syntax) is part of our innate endowment, our genetic make-up. Again, though, if this seems hard to buy into, it is because this claim, like the autonomy thesis, is a straightforward violation of the principle of parsimony (preferring simpler explanations over more complex ones). In other words, one certainly should *not* accept the innateness claim without evaluating the evidence.

4.1 Neurobiological evidence?

And so what is the evidence? One would think, of course, that one would require direct,

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remember later on that the sentence was in what linguists call the "passive."

[12] Just a little warning here: We assume that negative and indirect data are not necessary for the acquisition of phonological, morphological, and syntactic knowledge in the native language. For other areas of linguistic activity—what counts as "relevant" information in terms of Grice's Maxim of Relevance, for instance—it may be a whole different ball game. We just don't know.

[13] In fact, there's one further type of data, namely, ***explicit data***. This type of data would involve caretakers' teaching actual language rules to their children. In fact, I don't know anyone who might imagine doing this sort of thing. Even so, it is important to mention explicit data because of their presumed importance to second-language learning by adults.

neurobiological evidence for this kind of claim. Alas, getting at direct, neurobiological evidence is not only extremely difficult—ask any neurobiologist whether her job is a tough one—but also fraught with ethical problems. After all, we may not even think twice about slicing up cockroaches to understand something about their capacities. We do, however, think twice about butchering "higher" animals like cats and monkeys (even though this is, in fact, done rather often). And, of course, nobody even dreams about mutilating human babies just to find out if their brains include "grammar genes." Luckily, though, there are other, non-invasive means that can be employed to provide evidence for or against such claims.

4.2 Ethological evidence

The most non-invasive evidence for the innateness claim on linguistic competence has its roots in a more general area of science, namely, ***ethology***, that area of science that observes animal or human behaviors in detail and draws conclusions from those observations. And before turning to the actual evidence on linguistic competence, it is important to understand something about the *modus operandi* (way of working) of ethologists examining innateness claims.

The best way of learning about the ethologists' way of operating is to consider an example. Here we consider the work of Eberhart Gwinner and his colleagues, who examined migratory behaviors among European garden warblers.[14] In particular, these warblers spend their summers in north-central Europe and their winters in central Africa. At two particular times of the year, they begin migration, flying either southward along a very well-defined path to Africa, or northward on a well-defined path to Europe. Now, the essential questions that Gwinner and his colleagues asked were these: How do these birds "know" when to take off for migration (the flight timing)? How do they "know" which direction to fly (the flight heading)? How do they "know" how long to fly (the flight duration)?

There are, of course, very plausible answers to such questions, ones that do not include any sort of claims about innateness. For instance, one can imagine that young European garden warblers know when to take off on migration because they observe their parents' behavior—and that their parents learned the same behavior in the same way. In effect, then, the information is passed from one generation to the next, not in some genetically-determined way, but in a way that would not

[14]A summary report on the research of Gwinner and colleagues on bird migration is the following:

E. Gwinner. 1986. Internal rhythms in bird migration. *Scientific American* 254:84-92.

be unlike, say, instructions on how to cook some special kind of cookies, passed from one generation to the next among human families. (My mother and her sister, for example, learned how to prepare and bake a special kind of pastry from their mother; presumably, the recipe, which was never written down, goes all the way back to my great-great-grandmother in Switzerland.)

To get at answers to these questions, Gwinner and colleagues performed a series of experiments involving hand-raised chicks, that is, young birds that were, for their whole lifetimes, never exposed to any source of information from the environment from which they could have learned the relevant information on timing, heading and duration of migratory flight. And the results of these experiments were remarkable, even mind-blowing: These birds "knew" exactly when to take off (timing), exactly which direction to fly (heading), and exactly how long to fly (duration). Now, think about it: If these young birds could not have learned this information from, for example, observing their parents, what other explanation is possible? In fact, the only possible explanation is that they "knew" this information from internal sources, that is, that the information was innate.

The experiments performed by the ethologist Gwinner and his colleagues comprise a simple and straightforward way of showing that knowledge must be innate. Further, these experiments illustrate the concept of knowledge determination. If some specified bit of knowledge can be tracked in full to information available from the organism's environment, we say that that knowledge is determined by the information available in the experience of the organism. However, when we find that the information available to the organism is less than what the organism knows—as in the case of Gwinner's hand-raised European garden warblers—then we say that that knowledge is underdetermined by experience. And *it is* ***underdetermination*** *of this kind that constitutes evidence that that knowledge derives from innate sources.* In simple terms, then, the garden warblers knew more than they were exposed to.

Fortunately, ethological experimentation does not always involve, for example, hand raising. There are other ways of finding out whether the learning environment plays a role or not. (And, as we will discover below, we do not need to hand-raise human babies in total isolation from linguistic exposure in order to determine whether the brain machinery underlying linguistic competence is innate or not.) The central lesson to be learned from the garden-warbler example above is, simply, that *underdetermination provides evidence for innateness*.

Now we can turn to our linguistic evidence. As promised, this evidence does not involve hand-

raised, isolated babies. In fact, what it does involve is the intuitions of any native speaker. By intuitions, we simply mean whether a given string ("sentence") is possible or impossible in a language.

Consider the "sentences" in the list below:

1. Mary caught the ball.
2. Who caught the ball?
3. What did Mary catch?
4. Mary caught what?
5. Who caught what?
6. What did who catch?
7. John said that Mary caught the ball.
8. John said that who caught the ball?
9. John said that Mary caught what?
10. John said who caught what?
11. John asked who caught the ball.
12. John asked what Mary caught.
13. Who did John say caught the ball?
14. Who did John say caught what?
15. What did John say that Mary caught?
16. What did John say Mary caught?
17. What did John say who caught?
18. Who did John say what caught?

In the 18 sentences above, we notice that some of them are fairly commonplace, every-day sorts of utterances. Nothing special, in other words. Others of them seem a little more esoteric. It's not every day that one hears a sentence like *John said who caught what?* On the other hand, sentences like this one, while a little unusual, certainly don't seem impossible, either. One can easily imagine a context in which in might even be said. But then we get down to the last sentence: *Who did John say what caught?* And most native speakers' intuitions immediately go berserk at this point, almost screaming out that something is very, very wrong, disastrously wrong with this poor, unfortunate sentence. In other words, our intuitions tell us quite clearly that the last sentence is not just a little unusual in the language; it's flat impossible.

Let's step away for a moment and consider the native speaker who looks at these sentences and

provides these intuitions. What we can say is that the person knows that the sentences in (1) through (17) above are possible in English, but that the sentence in (18) is impossible. In other words, we've now simply established what the person knows—just as Gwinner and his colleagues had to establish, prior to their work, what the garden warblers know.

Now, where did this person get the knowledge that (1) though (17) are ok, while (18) is impossible? Consider the sources from the learning environment for this knowledge. Maybe the person knows that (18) in particular is bad because she never heard her parents uttering such a catastrophe. Alas, this explanation doesn't work out because, as we've already seen, the particular combination of our capacity to produce and understand an infinite number of novel sentences does not square at all with children learning from what they don't observe. In other words, this potential explanation for the speaker's knowledge that (18) is bad doesn't work because we rule out indirect data as a source of information in language acquisition.

So if indirect data (our first attempt at explaining this knowledge) doesn't work, how about another source: Perhaps the person knows that (18) is bad because her parents corrected her for making this awful sentence when she was young. Once again, though, we wind up with a non-explanation: We also already observed that such negative data do not explain acquisition. Indeed, in this case, there's even more reason to think that such an explanation doesn't work. Consider this: If native speakers of English in general know that the sentence in (18) is bad because of corrections from their parents, then they must have all uttered such a sentence during their childhood and been corrected for it. There is, however, absolutely no evidence whatsoever in all of the reams and reams of child language data collected to date that children ever do produce such things. And, obviously, they can't be corrected for what they don't produce.[15]

So what are our options at this point? We've eliminated two different potential explanations for the speaker's knowledge that (1) to (17) are ok, while (18) is bad. A final way to go about trying

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[15] Indeed, as the research reported by, for example, Steven Crain and colleagues has demonstrated, children can't even be goaded by experimental manipulation into producing monstrosities of the kind shown in (18). For an example of this kind of research, try the following article:

Crain, S. & M. Nakayama. 1987. Structure dependence in grammar formation. *Language* 63(3):522-543.

to explain this speaker's knowledge would be to appeal, again, to the learning environment, this time, though, to our final source of information from the environment: positive data. Recall that positive data includes just simple exposure to sentences (that is, no corrections, no attempt to learn from what doesn't appear, and so forth). Could exposure to simple positive data explain the speakers' knowledge? A moment's reflection shows that this is certainly not very plausible!

After exhausting all of the ways to explain where the speaker could have gotten her knowledge from external, environmental sources, we wind up in the end in much the same place that Gwinner and colleagues arrived: If this knowledge cannot be traced to the environment, then the only place it could have come from is some internal source. In other words, it's innate.

What we have, then, is evidence that such knowledge derives from internal, innate sources. Note, then, that *this evidence is also a straightforward case of underdetermination: The speaker knows more than she was exposed to.*

5. What level of analysis?

Finally, we turn to what would be the appropriate way to analyze the native speaker's linguistic knowledge. To understand what this means, consider a passive sentence like the one below:

The lyrics were written by a master composer.

If we were to ask, say, 10 different native speakers, all non-linguists, to describe the structure of the example sentence above, what we would almost undoubtedly find is that the nature of their descriptions would not all be the same. One might find, for example, both of the following attempts to describe this structure:

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Analyst #1: The passive sentence is formed by putting into first position the thing or person that receives the action of the main verb (*the lyrics*), followed by a form of *be* (e.g., *were*), then the main verb as past participle (*written*), then the preposition *by*, and then the person or thing that performs the action of the verb (*a master composer*).

Analyst #2: The passive is formed from its active counterpart (*A master composer wrote the lyrics*). The original object (*the lyrics*) is moved to first position; the original subject (*the lyrics*) is moved to object position; the main verb is changed to a past participle (*wrote->written*); the verb *be* (*were*) is added before the verb; and the preposition *by* is added after the main verb.

As the two example descriptions illustrate, all of our analysts might succeed at producing an accurate description of the passive, their descriptions would not all be in the same format. More generally, then, it would turn out that the level of grammatical description may differ from one analyst to the next. The question we would thus want to ask is whether any particular level of description might be more highly valued in some sense.

In fact, Chomsky has proposed a way to discriminate among different levels of descriptions with three so-called ***levels of adequacy:*** Observational adequacy, descriptive adequacy, and explanatory adequacy. Below we will go through these three levels one at a time.

5.1 Observational adequacy

A grammatical analysis that attains ***observational adequacy*** is one that simply generates the sentences at hand. Put simply, this level would mean, for the example passive sentence above, that nearly any description would suffice, so long as the description successfully produces the sentence. In effect, then, both of the analysts' descriptions of the passive sentence above would attain this level of adequacy. So would the grammatical descriptions that one finds in typical grammar-reference books and foreign-language teaching textbooks.

5.2 Descriptive adequacy

A grammatical analysis that attains ***descriptive adequacy*** is one that (i) generates the sentences at hand and that (ii) reflects native-speaker intuitions of grammatical form. In this regard, consider again the example passive sentence from above, repeated here for convenience:

The lyrics were written by a master composer.

In fact, most native speakers know intuitively that this sentence is in some sense related to its active counterpart:

Active: A master composer wrote the lyrics.
Passive: The lyrics were written by a master composer.

This intuition of grammatical relatedness is exactly what descriptive adequacy requires of a grammatical analysis. Now consider again the two analysts' descriptions from above, again repeated here for convenience:

Analyst #1: The passive sentence is formed by putting into first position the thing or person that receives the action of the main verb (*the lyrics*), followed by a form of *be* (e.g., *were*), then the main verb as past participle (*written*), then the preposition *by*, and then the person or thing that performs the action of the verb (*a master composer*).

Analyst #2: The passive is formed from its active counterpart (*A master composer wrote the lyrics*). The original object (*the lyrics*) is moved to first position; the original subject (*the lyrics*) is moved to object position; the main verb is changed to a past participle (*wrote->written*); the verb *be* (*were*) is added before the verb; and the preposition *by* is added after the main verb.

Note that the description proposed by analyst #1 does not make reference to the active counterpart of *The lyrics were written by a master composer*. By contrast, analyst #2 makes explicit reference to this relationship ("The passive is formed from its active counterpart"). In this sense, then, the first analysis above does not attain descriptive adequacy; only the second analysis attains this level of adequacy.

5.3 Explanatory adequacy

A grammatical analysis that attains ***explanatory adequacy*** is one that (i) generates the sentences at hand, that (ii) reflects native-speaker intuitions of grammatical form, and that (iii) provides an explanation for how the grammar is acquired by all children. Note that this level of adequacy is essentially the same as descriptive adequacy except that we now require an explanation for acquisition to be built into the grammar itself.

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It seems obvious that an analysis that does not attain descriptive adequacy will also not attain explanatory adequacy. As a result, we do not consider the example proposal of analyst #1 again here. But does the second analyst's proposal attain this level of adequacy? Consider this proposal again:

Analyst #2: The passive is formed from its active counterpart (*A master composer wrote the lyrics*). The original object (*the lyrics*) is moved to first position; the original subject (*the lyrics*) is moved to object position; the main verb is changed to a past participle (*wrote->written*); the verb *be* (*were*) is added before the verb; and the preposition *by* is added after the main verb.

Note that even though this proposal does relate the passive to the active (hence dealing with the requirement to reflect native-speaker intuitions), there is absolutely nothing in this proposal that could explain how the passive is invariably acquired by all children. In other words, this proposal attains descriptive adequacy, but fails to attain explanatory adequacy.

We have thus seen that different analysts may come up with different descriptions of the same grammatical phenomenon (e.g., the passive sentence); however, that different analysts may come up with different descriptions does not mean that all of these descriptions are necessarily equal. Rather, the adequacy of an analysis depends on what one expects of it: We expect more of an analysis that attains descriptive adequacy than of an analysis that attains observational adequacy; and we expect a good deal more of an analysis that attains explanatory adequacy than of one that attains only observational or descriptive adequacy.

In fact, it is not clear that anyone has ever managed to produce an analysis that really does attain explanatory adequacy, nor is it clear that anyone will be able to, at least in this century. Attaining this level is nonetheless the goal of many linguists' work. In the following chapter, we sketch the outlines of a linguistic analysis that makes a serious attempt to attain just this level of adequacy, this time not for phonology or for morphology, but for syntax.

Chapter Five

Glossary

"All-hell-breaks-loose stage": After children pass through the two-word stage, their grammatical development literally explodes, making it impossible to say that there is a "three-word" stage. For lack of a better term, Pinker terms this stage of explosive grammatical development the "all-hell-breaks-loose" stage. See also Babbling stage, One-word stage, Two-word stage, Telegraphic stage.

Autonomy thesis: In the dominant paradigm of linguistics, the claim is that one's linguistic competence is governed by cognitive principles that are entirely independent of all other cognition and that these principles are not reducible to more general cognitive principles.

Babbling stage: In the early period of linguistic development, children produce recognizable human-language speech sounds (vowels and consonants). These sounds form a superset of the sounds that make up the inventory of the language the child is acquiring.

Competence: The native speaker's implicit (i.e., subconscious) knowledge of phonology, morphology and syntax of a particular language is that speaker's competence. All children (short of pathology) acquire full competence of the language of their speech community.

Descriptive adequacy: A grammatical analysis that attains descriptive adequacy is one that (i) generates the sentences at hand and that (ii) reflects native-speaker intuitions of grammatical form. See also Observational adequacy, Explanatory adequacy, Levels of adequacy.

Developmental sequence: When children acquire the morphosyntax of a particular language, they pass through well-defined developmental stages. All children, regardless of their upbringing, pass through these stages.

Error: In folk terminology, an error refers to a child's presumed misunderstanding of a linguistic structure. Given developmental sequences, however, it appears that children's "errors" may not be errors in the folk sense at all, but rather unavoidable milestones that all children must pass through in acquisition.

Ethology: The science in which one carefully observes the behaviors of animals (including humans) and draws conclusions from these observations.

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Explanatory adequacy: A grammatical analysis that attains explanatory adequacy is one that (i) generates the sentences at hand, that (ii) reflects native-speaker intuitions of grammatical form, and that (iii) provides an explanation for how the grammar is acquired by all children. See also Observational adequacy, Descriptive adequacy, Levels of adequacy.

Explicit data: The teaching of actual grammatical rules to learners. One might imagine teaching, for example, the so-called "passive" rule to adult learners of a second language. It is not conceivable to think that explicit data play any useful role in native-language acquisition since few, if any, caretakers provide this kind of data to toddlers. The role of explicit data in second-language acquisition has also been questioned. See also Positive data, Negative data, Indirect data.

Indirect data: If a learner were to attempt to draw grammatical conclusions based on what she does *not* observe in the input, that learner would be attempting to employ indirect data. Considerations of the finiteness of human memory and of native-speakers' capacity to produce and understand an infinite number of novel sentences suggests that indirect data do not play a useful role in acquisition.

Innateness: In the dominant paradigm of linguistics, the claim is that the principles underlying native-language competence are given as part of one's genetic endowment, or innate.

Maxim of Relevance: The philosopher Grice proposed the Maxim of Relevance as a general principle that governs how speakers manage conversations.

Levels of adequacy: One's expectations of a grammatical analysis are encoded by the three levels of adequacy: observational, descriptive and explanatory. See Observational adequacy, Descriptive adequacy, Explanatory adequacy.

Morpheme-order studies: In early studies of child language acquisition, researchers examined the order in which a series of closed-class morphemes would be acquired. There do appear to be very stable orders in which closed-class morphemes appear, but, given the heterogeneity of the morphemes themselves, it is also apparent that one cannot provide a single, all-encompassing explanation for these orders.

One-word stage: In early language acquisition, children pass through a stage in which their mean length of utterance is near one word. See also Two-word stage, Telegraphic stage, Babbling

stage, “All-hell-breaks-loose” stage.

Mean length of utterance (MLU): A means by which one can compare the development of one child to the development of another child (both learning the same language).

Negative data: If a child were to attempt to change her grammatical representation on the basis of information from the environment indicating that a given phrase or clause is impossible, one would refer to the data from the environment as negative data. The most usual example of negative data would be a correction by a caretaker. However, considerations of the availability of negative data, the use of negative data, and the usability of negative data conspire to suggest that this source may not be required for language acquisition. See also Positive data, Indirect data, Explicit data.

Observational adequacy: A grammatical analysis that attains observational adequacy is one that simply generates the sentences at hand. See also Descriptive adequacy, Explanatory adequacy, Levels of adequacy.

Positive data: The data of experience comprising only sentences in some context. This form of exposure is generally thought to be the simplest, most basic kind; it also appears to be the only kind of data from which all children acquire native-language competence. See also Negative data, Indirect data, Explicit data.

Principle of parsimony: A general principle governing scientific endeavor. The principle limits theorizing to the simplest of solutions. Also known as Occam’s Razor.

Telegraphic stage: See Two-word stage.

Two-word stage: During early language acquisition, children pass through a stage during which the mean length of utterance is around two morphemes. Utterances at this stage often lack grammatical morphemes (closed-class morphemes), giving them the appearance of short telegraph messages. This characteristic gave rise to the term *telegraphic stage* for children at the two-word stage. See Mean Length of Utterance.

Unanalyzed form: A child exposed to contracted forms in input like *wanna* (*want* + *to*) may not initially realize that such forms are, in fact, contractions of two morphemes and therefore treat them as if they were single “words.”

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Underdetermination: The condition observed when an organism “knows” more than it has been exposed to from the environment. The general conclusion from underdetermination is that this “knowledge” must come from internal sources, that is, innate.

Chapter Five

Appendix

Geoffrey Sampson wrote *Educating Eve*, in which he takes issue with the view that an innate component underlies our linguistic competence. Geoffrey's own description of the book is as follows (see http://www.cogs.susx.ac.uk/users/geoffs/REmpNat.html):

> I recently wrote a book, *Educating Eve*, published in September 1997 by Cassell (ISBN 0-304-33908-3), which examines the full spectrum of linguistic arguments for nativism - from Chomsky's works of thirty years ago to the new wave of the 1990s - and shows that they are all built on sand.
>
> The message of Educating Eve is that there is no `language instinct'.
>
> Educating Eve shows that anyone who is willing to look at the evidence in good faith must agree that language, and indeed human intellectual achievements more generally, are in all the ways that matter purely cultural developments. Only to a trivial extent are they predetermined by our biology.

The following is a review of Educating Eve by Feargal Murphy (http://www.ucd.ie/~linguist/gogs2.html), distributed over the Linguist List:

Date: Mon, 18 May 1998 11:56:11 +0100
From: LINGUIST Network <linguist@linguistlist.org>
To: LINGUIST@LINGUIST.LDC.UPENN.EDU
Subject: 9.734, Review: Sampson: Educating Eve.

LINGUIST List: Vol-9-734. Mon May 18 1998. ISSN: 1068-4875.
Subject: 9.734, Review: Sampson: educating Eve.

Home Page: http://linguistlist.org/

Editor for this issue: Andrew Carnie <carnie@linguistlist.org>
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What follows is another discussion note contributed to our Book Discussion Forum. We expect these discussions to be informal and interactive; and the author of the book discussed is cordially invited to join in.

Considering Linguistic Knowledge

If you are interested in leading a book discussion, look for books announced on LINGUIST as "available for discussion." (This means that the publisher has sent us a review copy.) Then contact Andrew Carnie at carnie@linguistlist.org

==========Directory==========
1)
Date: Sat, 16 May 1998 13:22:00 +0000 (GMT)
From: feargal murphy <MURPC88@MACOLLAMH.UCD.IE>
Subject: Sampson: Educating Eve

G. Sampson, (1997) Educating Eve: the language instinct debate, Cassell, London, 184pp.

Reviewed by Feargal Murphy, University College Dublin.

The rise of Generative Linguistics in 60s gave a new lease of life to the old Nature vs. Nurture debate. The behaviourist model of language was judged a failure at explaining the process of language acquisition and a new theory of language acquisition emerged. It was rooted in the philosophy of Descartes in the 17th Century as well as Humboldt and, ultimately, Plato. It came to be known as Linguistic Nativism or Rationalist Linguistics and is most associated with Noam Chomsky. The central tenet of this theory is that children are born with some kind of Language Acquisition Device already in place in their brains which allows them to acquire the language of their environment. Within linguistic nativism there has been some debate as to whether this device provides merely general rules that allow for the acquisition of language or whether it is a rich innate system that provides a complete basis for language acquisition so that a child can acquire the complexities of a language at a time when its general cognitive development could not possibly cope with such complexity. The rationalist tradition behind Linguistic Nativism has always been in contrast with Empiricism which states that all ideas and knowledge (including knowledge of language) come from experience obtained through the senses and that there can be no innate knowledge. A view that goes back to the 17th century and the writings of Locke, Berkeley and Hume and in the modern era has also been associated with the philosophers of the Vienna Circle and Karl Popper.

Educating Eve will certainly not determine the outcome of the nature vs. nurture debate, nor will it, in my opinion, lead to any productive insight into the debate. The book fails to understand the scope and import of the nativist arguments and consequently falls short of presenting a

genuine criticism of it. As it stands the book will be applauded by those who are already favourably disposed towards its conclusions and derided by those who are not.

Educating Eve (henceforth _EE_) argues against the writings of Chomsky, as well as Pinker's 'The Language Instinct', Bickerton's 'Language and Species' and Jackendoff's 'Patterns in the Mind'. These three books are essentially aimed at the general rather than the specialist reader. The result of targeting these three books is to make the arguments against nativism appear weak and superficial.

The desire to rescue public opinion from the wrong path is a recurring theme in the author's work. In his _Language and Liberty_, which also argued for a non-nativist view of language, the main aim was to discourage people from following left wing politics. The author's politics, unfortunately, emerge as a strong force in this book. Given the importance of the nativism debate I shall focus primarily on the more important linguistic issues, leaving aside politics except when they cloud the issues surrounding nativism The alternative to nativism that this book argues for is essentially a version of Karl Popper's empiricism. According to this view the ability to learn may be innate but there are no domain specific innate mechanisms involved in learning. People learn the language of the community they belong to in the same way that they learn anything else, by trial and error. They 'make original though fallible conjectures and test them against objective reality' (p.137 - all quotes are from _EE_ unless otherwise indicated). Eventually, using this method, each individual comes to learn a language. What they learn are 'wholly cultural constructs' (p.137), an E-language rather than an I-language, to use Chomsky's terminology. Note that the conception of language in _EE_ is totally different from those of Chomsky, Jackendoff, Bickerton or Pinker. For these nativists, language cannot belong to a society rather than to the individual. This issue is not actually addressed in the book. _EE_ merely states that language is a cultural artifact and not biological, but does not furnish any evidence for this claim. The book also states Popper's belief that the mind is not amenable to scientific theorising (p.138). I am unable to find any evidence in the book to support this claim. However, if one assumes that the mind is outside the domain of scientific inquiry then it simply becomes impossible to engage in a debate about the merits of the specifics of any nativist theory.

The first task this book undertakes is to demolish Chomsky's arguments for nativism (only sourcing material up to 1991) and then to demolish the 'second wave' nativists - with Pinker foremost in the second wave. The book utilises many arguments, some more noteworthy than others, and it would be beyond the scope of this review to examine them all. I will look at a few of the arguments, more or less at random.

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The arguments against Chomsky centre on what Sampson perceives as the basis of nativism:

1: Speed of acquisition
2: Age-dependence
3: Poverty of data
4: Convergence among grammars
5: Language Universals
6: Species-specificity

_EE_ claims the first argument (Speed of acquisition) is hollow as there is no way of determining what exactly 'speed' could be in this area. However, it seems uncontroversial enough that children acquire language faster than adults and that they do so without the problems that adults encounter. There is no attempt in the book to address the notion of whether different languages are acquired at different speeds or whether different children acquiring the same language do so in radically different sequences. This would seem to be a distinct possibility if we do not have an innate language specific faculty but rather rely on some generalised learning mechanism. A look at the evidence regarding language acquisition across languages would have helped make the case conclusive. The book should have dealt more with data available in order to show that only a non-nativist account of language acquisition could capture the facts. Instead _EE_ relies on the notion that the Popperian account can cope just as well as a nativist account and is closer to the 'common sense' view of language. But this does not prove that a Popperian account is right.

Chomsky's second argument (Age-dependence) is dismissed because, the book states, humans can learn language at any age "if they see the task as worthwhile" (p.36). However, the author does not describe any procedure for assessing the worthwhileness of the task. Why a child would find it worthwhile to learn a language when its needs are being met by compliant adults responding to non-linguistic communication is not addressed. If the early vocalisations of children are evidence of the beginnings of language acquisition, then the child is deciding at a very early age that language is 'worthwhile', at a stage when it seems difficult to believe the child is weighing up any options to see what may or may not be worthwhile. Following this logic we could extend the criterion of "worthwhileness" to other 'learning' such as 'learning to walk' and eliminate the innate component from that as well.

The book states that there are "plenty of cases on record of adults mastering a second language to native-speaker standard apart from accent" (p.36); I am not quite sure what it might mean to

achieve native-speaker standard apart from accent. What the book suggests - without providing any crucial evidence -- is that “learning as a general process is for biological reasons far more rapid before puberty than after” (p.37). This is supposed to show that age dependence is not proof of a critical period for language acquisition using some innate language acquisition device -- a critical period being one of the features that is characteristic of innate behaviour. The age dependence, according to the book, falls out from the slowing down of the learning process in general after puberty. This, however, seems to contradict the other argument against age dependence; that an adult can master a second language to native speaker standard.The author also states that “Susan Curtiss herself regarded Genie as refuting the strong version of Lenneberg’s claim, that natural language acquisition cannot occur after puberty” (p.37). Susan Curtiss might want to respond to that one herself. My understanding of her work on Genie is that Genie did have language (that is, rule-governed creative linguistic behaviour) but was not able to achieve anything close to native speaker competence while under observation (Curtiss, p.204) even though it would indeed have been ‘worthwhile’ to do so. Furthermore her linguistic development did not follow the same course as children acquiring language normally. This suggests that Genie’s ability to learn language was diminished due to her age but the important point is that her cognitive development in other areas seemed not to be so affected. It would also have been worthwhile for Chelsea to have learned language, yet Chelsea did not achieve native speaker standard (see Jackendoff’s _Patterns in the Mind_).

The third argument presented by Chomsky for nativism, according to the book, is poverty of data during acquisition. The arguments against Chomsky presented in the book do not bear close scrutiny. According to the book, motherese provides a “graded series of language lessons” (p.39) and is not at all degenerate, as the poverty of data argument states. This is not particularly new (the references cited by the author are from the early 70s). The fact is, however, that we really do not yet know what role - if any - motherese plays in learning/acquiring a language. But one thing worth noting is that there is no language teaching theory that proposes that adults could be most effectively taught a second language through being taught in motherese. It is also true that there do not yet exist conclusive studies on the universality of motherese (although the wide variety of language groups studied show a high degree of similarity). In some cultures motherese seems to play less of a role and is different to the motherese we see in the English speaking world. In fact motherese or child directed speech seems not to make the task of learning/acquiring language all that much easier but what it does is allow for a high degree of socialisation. This can be seen in the fact that the simplification of structure decreased greatly when the parent and child were no longer in the presence of the observer but merely being taped. The motherese wa s not for the benefit of the child only.

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Of course, if language is not in any way innate we are left with the problem of explaining how child directed speech is so perfectly a “graded series of language lessons” for the infant, despite the fact that adults are not taught how to give language lessons, perhaps empiricist would have to say that the ability to give “a graded series of lessons” in language is innate. The author states that a Popperian account of language learning would allow for a child to determine the general principles from the input (as it would for an adult who wants to learn a language). This is important as the nativist account holds that the basic principles of language are innate and thus are available for use in determining what is going on in the language input. Individuals learning a language via a Popperian method would use their learning abilities to work out the general principles from the input without any of these principles being already available in the head. But what are we to make of cases of *Specific Language Impairment* where the rate of learning/acquisition a language seems different from other areas of learning? The whole basis of a Popperian analysis is that language acquisition is not different from any other domain where learning occurs. Consequently, it predicts that there could be no difference between the acquisition of language and learning in other cognitive domains. This prediction runs counter to reported facts as clearly presented in Yamada’s 1990 book ‘Laura: A Case Study for the Modularity of Language’.

In arguing against Chomsky, the author takes Chomsky’s much used example of English speaking children determining how to ask yes/no questions. He is seeking to show that Chomsky is being dishonest in his analysis by not verifying the data.

A child working out how to make a yes/no question in English has to work out that if any verb is to be moved it has to be the verb in the main clause and not just the first verb encountered in the sentence. To work this out, a child has to distinguish between verbs and other words and also between main verbs and auxiliary verbs as well as knowing the structure of the phrases in the sentence. Then the child has to work out that auxiliary verbs in the main clause can move to the beginning of the sentence but that main verbs appear in the infinitive while a tensed ‘do’ occurs at the beginning of the sentence. Chomsky’s point is that children learn to make these distinctions and move the appropriate verb from the appropriate position to form a yes/no question with great ease. Both the author and Chomsky agree on the analysis of the rule involved in forming a yes/no question but where they differ is in their belief about the *exposure* of children to yes/no questions where the main clause is not the first clause in the sentence but rather a subordinate clause appears first, as in:

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Is the man who is talking to Bill wearing gloves?

Chomsky's claim is that a child may not encounter such a question in the language s/he hears before determining the rule of yes/no question formation. _EE_ argues that such sentences are indeed present in the language that children hear. What the book actually has to prove is that all children hear this crucial form of yes/no question before they determine the yes/no question formation rule. This is not shown but instead it is argued that such structures are available to children who read; but children who don't read learn language as well. Indeed it is quite possible that children form the rule about yes/no question formation before the ever learn to read. The author claims that he shows that Chomsky is 'wildly mistaken' (p.42), he paraphrases the situation as follows:

".. the man who is the world's leading living intellectual, according the Cambridge University a second Plato is basing his radical reassessment of human nature largely on the claim that a certain thing never happens; he tells us that it strains his credulity to think that it might happen, but he has never looked, and people who have looked find that it happens a lot." (p.42-43)

The people who have looked are Sampson himself and Geoff Pullum (p.42) but they have only checked sources of written language but as I have said this ignores the fact that illiterate children can also speak. In fact, so enamoured is the author with the written word that he argues that, historically, hypotaxis emerges as literacy develops. Somehow the organisation involved in writing leads to great leaps forward in language. He does finally admit that there is no great research on this topic but blames linguistics departments for this ("this is not a popular topic of research in linguistics departments", p.75). There is nothing to prevent the taping of the speakers of modern pre-literate languages -if one can find any cultures that could be classed as pre-literate without any qualifications - in order to test for the occurrence of hypotaxis but the author doesn't seem to have u ndertaken this task. The book doesn't opt fully for the idea that there was little or no hypotaxis in the languages of ancient pre-literate cultures as the author states, hedging his bets: "If there was little or no hypotaxis in these languages" [the Hebrew of the old testament etc.] "that is a real difference between them and more recent languages".(p.75)

For obvious reasons we can never answer the question of whether or not there was hypotaxis in Hebrew before writing or indeed while writing was still a fairly recent innovation. It is true that serious investigation into parataxis and subordination in various languages has been carried out and goes back over one hundred years. The book claims that "it was a cliche of late nineteenth century linguistics that the early stages of languages with a long recorded history showed a

development from what was called parataxis to hypotaxis" (p.74). It cites great linguists like Karl Brugmann, Hermann Paul and Eduard Hermann as support for the notion that languages go from a paratactic stage to a hypotactic stage. But there are two problems with this. Firstly, not all nineteenth century linguists would have been happy with the notion that hypotaxis emerged as writing developed. Hermann Jacobi's 1897 work 'Composition und Nebensatz' concluded that "Maori, like early stages of PIE may not have had contained a relative particle" in subordination (Lehmann, p.60). So Jacobi is claiming that PIE did have subordination which is the basis of hypotaxis for Brugmann, Paul and Hermann. Secondly, there is really no reason to believe that nineteenth century linguists got everything right in comparative and historical linguistics. The idea that hypotaxis emerged from a previous stage must be seen alongside other ideas in nineteenth century linguistics about the development of languages from primitive systems to more complex systems.

The fourth argument the book looks at is Convergence among grammars - the notion that people of varying levels of intelligence and exposure to different language inputs, converge on the same grammar. The book claims (p.46) that Chomsky has admitted that educated people may know their mother tongue better. This does not fit in with an I-Language approach and so is obviously not representative of Chomsky's theory of language. An I-Language approach excludes the possibility of a mother tongue (an E-Language) that people have mastered to varying degrees. For Chomsky such things as English or Hungarian or Swahili do not exist in any meaningful way. This is controversial, but the point is that for Chomsky there is no mother tongue divorced from an individual's knowledge of it.

The second line of argument identified by Sampson is that people do not actually converge on the same grammar as they give different grammatical judgments in response to the same data. The book refers to Labov's work - presented in Austerlitz (1974) - where respondents varied in their judgments about the grammaticality of presented sentences. The really interesting thing here is that the people taking the test understood the instructions - oral and written - which they were given concerning the test. It is hard to see how they could all have the same understanding of what was said to them unless they had pretty much the same grammars. The language that was used in their instructions would most likely have contained a variety of syntactic constructions, but there are no reports of the people completing the test remarking on the ungrammaticality of any of the instructions they were given. It is easy to find people varying in groups and even as individuals over time when you ask them to assess sentences such as:

'Every one of the boys didn't go there'.

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The fact that individuals will differ in their judgment over time shows that what is being tested is not the individual's competence because an adult's competence does not vary - although their performance might. A large number of factors determine whether an individual would judge a sentence like the one given above as perfectly grammatical or borderline or totally ungrammatical. But the fact that there is a variation in the reported judgments does not in fact constitute an argument against convergence among grammars it is not a test of grammars but rather a test of performance. Intuitions about sentences involving scope relations of quantifiers and negation vary because of the many factors that bear on performance.

The book also contends (pp.107 - 137) that the existence of language universals can be explained by the fact that a Popperian learning system will automatically yield such universals. They are not the result of the nature of the language faculty but instead derive from the Popperian system of learning. Again, the author does not show that a Popperian account is better than a nativist account at capturing the facts. If the case against nativism is to be proven, then those language universals that we can agree to exist must be explicable either within a Popperian system *only* or at least they should be explained better by a Popperian learning theory. The book does not do this.

The book targets Pinker's 'The Language Instinct'. Why Sampson decides to devote so much effort to the book is somewhat puzzling. Pinker's book is aimed at a fairly general audience and as such is not really a worthwhile target. Among the arguments against Pinker we find the assumption that surnames are actual words in individual languages. This means that the existence of a person in England with 'sri' as the first three letters in his surname means that 'sri' is a possible initial consonant cluster in English (p.83).

The author also can't resist taking a pot-shot at what he imagines Pinker's political views to be. The tone of his response to Pinker is one of condescension which can be irritating and detracts from any points that the author may be trying to make as well as being irrelevant to the substantive issues. But the worst attack is saved for Bickerton. The book seeks to portray him as an intellectual lightweight in a most unappealing way (p .76):

"[Bickerton's] stuff must be a real hit with Green students on Hawaii".

It is surely not too much to expect that personal abuse would be kept out of the realm of intellectual debate.

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The book also takes on Jackendoff stating that Jackendoff writes well: “Jackendoff is one of the best writers among the linguistic nativists” (p.76), but not forgiving him for being a nativist. “If Jackendoff reaches for the word ‘instinct’ so hastily in these cases, how can we trust him to use the word judiciously where there are real questions to be resolved” (p.79). The main issue picked up on in Jackendoff’s work relates to sign language. At one stage the author says (arguing against Jackendoff’s analysis of Universal Grammar):

“Jackendoff has shortly beforehand pointed out that one aspect of American sign language grammar is not paralleled in any spoken language. The sign-language equivalent of the third person pronouns (she, they, his) consists of gesturing (with hand shapes and movements that depend on whether the reference is singular, plural or possessive) to locations in the signing space which have been assigned to successive topics of conversations. ‘The effect is that there are as many different third-person pronouns available as there are discernible locations in the signing space’. No spoken language has a pronoun system remotely like this, so how can it be part of our innate knowledge.” (p.78)

The author is missing the point here. Sign language expresses person, number, gender and possession as spoken language does and not some other features not found in spoken language. So the system the same as spoken language. The use of pronouns in sign language is exactly the same as the use of pronouns in spoken language and is based on universal grammar, what is different is the modality. The availability of the signing space means that a signer can introduce a greater number of pronouns into the discourse as each pronoun can be allocated a location in the signing space without leading to any confusion. This is the same as the fact that I could use as many second person singular pronouns as I wanted as long as I looked at the individual that I was addressing with that pronoun. Looking at the person that I was referring to with the pronoun is really the same as the signer using the sign space to tag pronouns. This is not just true of second person pronouns. I could be talking about, say, the members of a football team and refer to each individual using a third person pronoun. I could be quite clear in my own head who is being picked out each time, it would just be confusing to the person I was talking to. This is because there would be no way for them to distinguish the referents of each of the pronouns. In sign language I can exploit the possibility of the signing space by tagging each meaning of a pronoun with a location in space so that the whole thing does not get confusing. The potential in both oral and sign language is the same, but sign can exploit the signing space in a way that oral language cannot. The point is that the limitations are not imposed by language but by the modality. What Jackendoff is saying is that the same Universal Grammar underlies language no

matter what the modality.

The last chapter in the book ('The Creative Mind') presents Sampson's view of Popper. It is not without problems and the author devotes much time to arguing that Popper didn't always mean what he said and that Sampson's analysis of Popper is the best. However, the crucial lines in the chapter for the purposes of nature vs. nurture are:

"Minds are not a topic open to scientific theorising." (p.139)

and

"The position adopted in this book is that conscious human minds are not physical entities. Talking about minds is not a way of talking about high-level aspects of brain functioning. Minds and bodies are different and separate things which act on one another." (p.138)

For all those people who believe that minds are indeed a topic open to scientific theorising this will render anything else the book has to say suspect. Modern nativists (as well as most non-nativists) view the mind as amenable to scientific theorising. For Chomsky there is no other possible way to study the mind (Chomsky 1993, 1995). It is hard to see how a significant debate can take place between two such opposite points of view. The author is committed to the cause of a Popperian analysis of language learning/acquisition but will never be able to convert a nativist to his way of thinking until he can gain an understanding of the motivations for adopting a nativist position or reconcile himself with the possibility that nativism may be a stance that an intelligent person can adopt. The author seems to imagine himself fighting a rear guard action against the hordes of misguided nativists who, as he sees it, are close to winning the hearts and minds of the masses. One important aspect of this book is that it can be read as a means of testing how one feels about linguistic nativism. Unfortunately for Sampson's crusade, it had the effect of making me prefer the nativist analysis even more than before.

(more background information about the author, Geoffrey Sampson, and the book 'Educating Eve' is available at: http://www.grs.u-net.com/)

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LINGUIST List: Vol-9-734