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RELATIONS OF LANGUAGE AND THOUGHT

The View from Sign Language
and Deaf Children

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The Modular Effects of Sign Language Acquisition

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LANGUAGE AND THOUGHT 1: THE SAPIR-WHORF HYPOTHESIS

One of the topics that introductory linguistics courses almost always cover is the Sapir-Whorf hypothesis. According to the textbooks, around 1930 Edward Sapir (see Sapir, 1949) and (somewhat later) Benjamin Lee Whorf (Whorf, 1956) made two proposals: "First, that all higher levels of thinking are dependent on language. Second, that the structure of the language one habitually uses influences the manner in which one understands his environment. The picture of the universe shifts from tongue to tongue" (Chase, 1956, p. vi).

Careful reviews of this position usually point out that Sapir and Whorf were not the originators of such ideas, nor did they even put them so strongly (see Hill, 1988, for a nice review). But the idea that language shapes thought—or cognition—has been debated in various forms over the years and now usually goes by the name of the Sapir-Whorf hypothesis.

Despite their popular appeal, the two hypotheses, linguistic determinism and linguistic relativity, have been largely discredited by thorough crosslinguistic study and experimentation. The linguistic determinism hypothesis is the easiest to put to rest. The facts that (1) one can discover the patterns in a language other than one's own and that (2) cross-cultural miscommunications are frequent even among speakers of the same language attest to the ability of human cognition to think outside of language (cf. Hill, 1988).

The linguistic relativity hypothesis has been subject to greater scrutiny. Whorf's famous example of the Hopi view of time ("becoming later") as

embodied in the linguistic system (e.g., the absence of tense marking on verbs) has frequently been discredited, in terms of both the actual linguistic system and the relationship between such a system and such a worldview.

A more particular example of potential linguistic relativity comes from the study of color terms. In a widely cited work, Berlin and Kay (1969) argued that perception and memory of colors are more closely related to a universal human capacity to perceive color than to the various ways in which different languages break apart and label the color spectrum. While their work has been subject to some criticism (Hill, 1988), it stands as one example of the failure of a strong form of linguistic relativity.

In large part, then, the strongest forms of the Sapir-Whorf hypothesis cannot be true. Although languages differ, and cultures differ, it is usually concluded that where differences exist between languages, they may be entirely accidental or they may reflect—not determine—speakers' worldviews. However, in at least one way, the relativity hypothesis does receive support. As Hill (1988) points out: "The original demonstrations by Boas . . . and Sapir . . . that the sound patterning of our languages constrains our perception of speech sounds, have never been refuted" (p. 31).

Sapir showed that a speaker's perception of speech sounds was influenced by the role these sounds played in the native language. To an untrained native speaker, it is difficult to distinguish the various phones that make up one phoneme. For example, the /p/ in "pot" and the /p/ in "spot" are pronounced very differently, but native speakers of English to whom this fact has never been pointed out are rarely even aware of it. Sapir (1933) remarked: "I have come to the practical realization that what the naive speaker hears is not phonetic elements but phonemes. . . . It is exceedingly difficult, if not impossible, to teach a native to take account of purely mechanical phonetic variations which have no phonemic reality for him" (pp. 47-48). That is, by virtue of having learned to speak, the way we perceive speech is affected. There are two aspects of this phenomenon we should examine more closely.

First, there is abundant evidence that adults perceive linguistic contrasts from their own native language much differently from their perception of contrasts not in their own language (in addition to Sapir's work, see Lisker & Abramson, 1964, 1970; and Miyawaki et al., 1975). This effect of linguistic experience can be observed not only in adults but even in very young children. A series of studies has found that children as young as 1 year old differentially perceive sounds from their own language as compared with sounds from other languages (see Eimas, 1991, and Werker, 1991, for reviews of many of these studies). It seems, however, that the very youngest infants tested have not yet narrowed down their phonetic inventories. That

is, they perceive all contrasts in the way that adult native speakers of the languages using these contrasts do. Hence, the perception of specific phonetic contrasts is clearly an area in which language acquisition affects perception. In this sense, linguistic relativity is correct: the way we perceive the world (in particular, the way we perceive speech sounds) is strongly affected by the characteristics of our native language.

The second issue to consider has not been tested, but it is even more germane to the point of this book. Does having learned spoken language affect the perception of speech in general? That is, are there aspects of speech perception that depend on early exposure to spoken language but not on early exposure to a particular language? Even more broadly, does having learned spoken language in general affect any other aspect of perception or cognition? Is there some aspect of our perception that has been influenced by the fact that we have been exposed to spoken language—something that would be different if we had not been exposed to speech?

It is reasonable to bring up the possibility that exposure to spoken language affects perception or cognition. In comparison, we know that exposure to the vertical and horizontal lines normally present in the environment affects the development of a kitten's visual perception (Blakemore, 1974). Why shouldn't exposure to the special acoustic properties of speech affect perception, especially auditory perception?

It is easy to see why this issue has not been tested. In order to see whether the acquisition of spoken language affects perception or cognition, it is necessary to compare individuals who have acquired spoken language with individuals who have not. Unfortunately for the experiment, individuals who have not acquired spoken language are rare, and there is always a reason for their lack of spoken language that would invalidate the comparison. Only people who have suffered from severe mental, social, or auditory deprivation fail to acquire a spoken language. It would be possible to experimentally test the hypothesis by keeping neurologically normal children in a rich, loving environment with sound but not speech (cf. the kittens). However, this is, of course, impossible.¹

Thus, we cannot directly test the question of whether the acquisition of spoken language affects the perception of language or other cognitive functions (aside from the particular phonetic contrasts found nonuniversally). But we can raise the issue with slightly different particulars, resurrecting the Sapir-Whorf hypothesis by virtue of the recognition that human language is not confined to the oral and aural modality. We can now ask: Does growing up with exposure to a signed language affect cognition? If so, how?

I will take it as well proven that the natural signed languages of the deaf are fully developed human languages on a par with naturally developed spo-

ken languages (see Chapter 2). The conclusion that American Sign Language (ASL) is an independent, noncontrived, fully grammatical human language comparable to any spoken language has been supported by over 30 years of research. Recent research has shown that ASL displays principles of organization remarkably like those for spoken languages, at discourse, semantic, syntactic, morphological, and even phonological levels (see, e.g., Klima & Bellugi, 1979; Wilbur, 1987; and Sandler & Lillo-Martin, forthcoming, for reviews). Furthermore, it is acquired (Lillo-Martin, forthcoming), processed (Emmorey, 1992), and even breaks down (Poizner, Klima, & Bellugi, 1987) in ways analogous to those found for spoken languages. The similarities between signed and spoken languages are strong enough to make the differences worth investigating. In the third section of this chapter, I will argue that although there are differences in detail, the similarities are strong enough to conclude that essentially the same language mechanism underlies languages in either modality.

Then, if we accept the proposition that language reflects rather than determines thought, we might expect that signed languages will have no more of an effect on thought (or cognition) than Hopi or English. There are even further theoretical reasons not to expect such effects, to be discussed in the next section. There, I will outline the modularity hypothesis, which maintains that language is served by a processor independent in specific ways from other cognitive devices. I will briefly discuss the basic evidence for this hypothesis from consideration of spoken languages and suggest that it is a viable hypothesis for signed languages as well. If one and the same module underlies the acquisition and processing of spoken and signed languages, then we are led to expect signed languages to have certain characteristics but not others. In the third section, I will discuss the basic characteristics of signed languages and argue that they are consistent with the modularity hypothesis. Thus, our prediction will be that acquisition of a signed language does not affect cognition.

However, as I will summarize in the fourth section, there have been claims made that significant differences are found between the cognitive processes of deaf/signing children or adults and those of hearing/speaking ones. Does this mean Whorf was right after all? Are there, as he maintained, innumerable variations among languages that lead to innumerable cognitive variations among groups of speakers? In the final section, I will show that the credible differences that have been claimed are tightly limited in domain, and I will argue that this is exactly what is expected to follow from the model presented. In so doing, I will often speculate and raise questions for future research.

THE VIEW FROM THE MODULARITY HYPOTHESIS

To begin with, let us consider a hypothesis about the human faculty for language (based solely on studies of spoken languages): the modularity hypothesis, as described by Jerry Fodor (1983). According to Fodor, language has the characteristics of an "input system"—more similar to visual processing than to higher order thinking.² That is, the cognitive mechanisms that subserve language are specialized, automatic, and domain specific; according to this hypothesis, language is not served by the same cognitive devices that are used for problem solving or reasoning. Of course, the output of the language processor must feed general cognition, just as the output of the visual processor provides input to thought processes; and our responses to language can be mediated by such factors as whether or not we believe what we hear, just as our responses to what we see can be influenced by our knowledge of the world. But we hear language as language whether we believe it or not, just as we see a tiger as a tiger even if it is in the living room.

Let us use the diagram in Figure 3.1 to illustrate. The diagram shows the language processor operating as an input system: like the visual processor

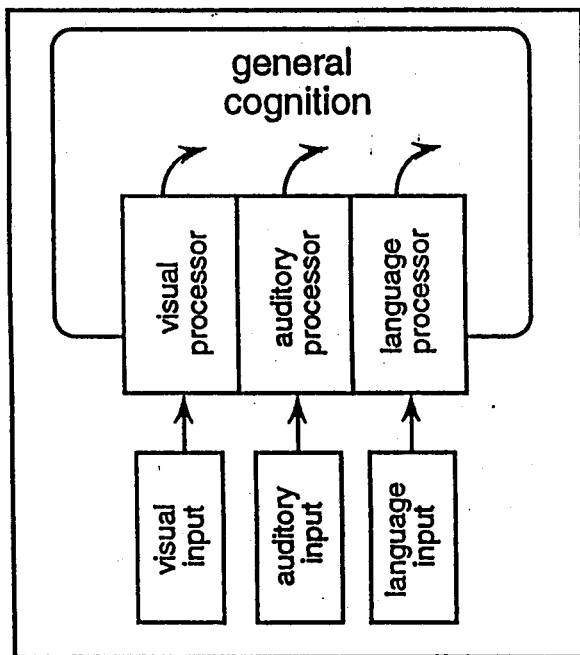


Figure 3.1. The Modularity Hypothesis: Input Systems

and the auditory processor, it takes input, processes it, and sends the output of its processing to general cognition. (Many additional processors, of course, have been omitted from the figure.) General cognition does not guide the processing of the input systems; it does not even have access to deep representations. Only the output of the input processors is available for the operations of general cognition.

To be more specific, Fodor (1983) provided a list of characteristics that he claimed modular systems display. Modules are domain specific, so the module for visual processing is distinct from the module for linguistic processing (cf. note 2). Their operation is mandatory, so we can't help but hear linguistic input as language. There is limited central access to the representations of a module; although the final output of a module's processing is available to central cognition, its internal layers of representations are not. The operation of a module is fast, but the operations of problem solving are frequently not fast. Modules are informationally encapsulated; that is, information from outside the module is not available during the module's processing.³ Modular systems have shallow outputs; that is, they conduct only a minimal amount of processing before sending output to the central processors. They are associated with fixed neural architecture; specialized brain regions subserve language and other input systems. Modules exhibit characteristic and specific breakdown patterns, as in the aphasia and agnosias. Finally, the ontogeny of modules exhibits a characteristic pace and sequencing, such as the well-known milestones children go through in acquiring (spoken or signed) language.

My hypothesis is that natural signed languages are served by the same language module that serves spoken languages. That is, the domain specificity of the linguistic processor refers to language processing, not speech processing.⁴ If so, sign language processing should display the same characteristics as other modules, such as having mandatory, fast operation with limited central access to shallow outputs, displaying informational encapsulation, and being served by a fixed neural architecture with characteristic breakdown and ontogeny. There is some evidence that this is so. For example, the processing of American Sign Language is as mandatory and fast as spoken language processing; signers experience sign language aphasia after damage to similar neural areas as those implicated in spoken language aphasia, and children acquiring ASL go through the same stages and pass the same milestones as those acquiring spoken languages (see, e.g., Klima & Bellugi, 1979; Emmorey, 1992; Poizner et al., 1987; Newport & Meier, 1985; and Lillo-Martin, forthcoming).

What more can be said about the language processor specifically? Let us make a combination of Fodor's modularity and Noam Chomsky's modular-

ity (1981). Unlike Fodor, Chomsky proposes that the operations of the phonological, morphological, syntactic, and semantic components may themselves be modular. See Figure 3.2 for illustration. The figure shows that the language module receives input and passes its output to the processing of the central systems (general cognition) without receiving feedback from the latter. The language module itself consists of Universal Grammar (UG) and is divided into submodules, here represented as phonology, morphology, syntax, and semantics. This representation is not meant to be exhaustive but is illustrative of the internal workings of the language module.

Now, if the same Universal Grammar explains signed and spoken languages, then its principles should be abstract enough to hold across modalities. As linguistic information is input (from whatever modality), it is processed by the amodal submodules and output to general cognition, displaying the characteristics of an input system/module.

What evidence can be adduced in regards to the modularity hypothesis? An important class of evidence for domain specificity comes from studies of the perception of speech, many of which have been undertaken at Haskins

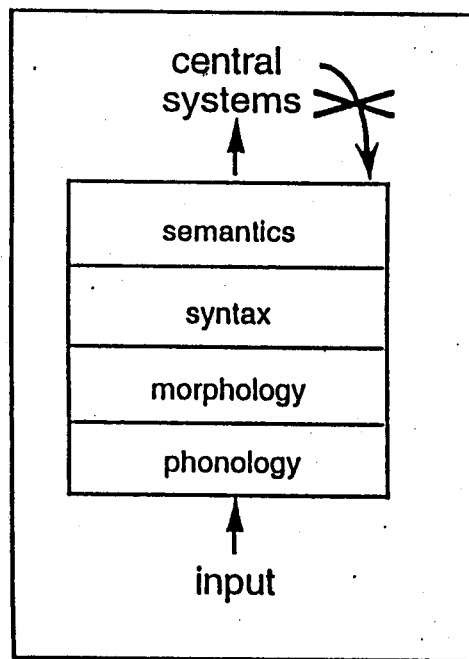


Figure 3.2. The Modularity Hypothesis: The Language Module

Laboratories (see, e.g., Liberman, Cooper, Shankweiler, & Studdert-Kennedy, 1967; Mann & Liberman, 1983; Mattingly & Liberman, 1985; and Mattingly & Studdert-Kennedy, 1991). As Fodor (1983, p. 49) points out, these experiments show that "the very same signal that is heard as the onset of a consonant when the context specifies that the stimulus is speech is heard as a 'whistle' or 'glide' when it is isolated from the speech stream. The rather strong implication is that the computational systems that come into play in the perceptual analysis of speech are distinctive in that they operate only upon acoustic signals that are taken to be utterances." These results support the hypothesis that a specialized processor analyzes acoustic information in very particular ways only if the input is linguistic (the modularity hypothesis).

Fodor cites a study by Marslen-Wilson and Tyler (1981) as evidence for the mandatoriness of the language module: "Even when subjects are asked to focus their attention on the acoustic-phonetic properties of the input, they do not seem to be able to avoid identifying the words involved. . . . This implies that the kind of processing operations observable in spoken-word recognition are mediated by automatic processes which are obligatorily applied" (Marslen-Wilson & Tyler, 1981, p. 327, cited in Fodor, 1983, p. 53).

Fodor acknowledges that some individuals, such as trained phoneticians or painters, can "undo the perceptual constancies" and "hear their language as something like a sound-stream," or see the world "as a two-dimensional spread of color discontinuities varying over time" (1983, p. 54). But he argues that such highly skilled processing is qualitatively different from normal processing and should not be taken as a counterargument to the claim of mandatoriness. Furthermore, in his 1985 précis, Fodor argues that experiments that demonstrate particular points using non-normal processing (such as the perception of speech in noise) cannot necessarily be taken to provide evidence about normal processing and therefore cannot be used as counter-evidence to the modularity hypothesis.

As for limited central access, Fodor cites two of the numerous experiments that demonstrate that only meaning is maintained for long after sentence processing; the exact syntax becomes lost momentarily (see Sachs, 1967). We can, by employing extra processing, force ourselves to recall certain aspects of processing. Other, lower levels might never be accessible, but their impact can still be seen in processing. For example, Pisoni and Tash (1974) found that reaction times to same/different judgments for pairs of sounds that are perceptually indistinguishable were different depending on whether the stimuli were actually acoustically identical or differed only in noncontrastive (subphonetic) acoustic properties. It is, at best, very difficult

to access lower levels of language processing; in normal processing, these levels are used but are not available for conscious report.

Fodor provides similar kinds of evidence for his other claimed characteristics of modules, which I will not summarize here. Let me put forward that the evidence for the hypothesis is strong and consider what implications it would have, if it is correct, for signed languages. First, however, I would like to discuss another hypothesis that also has bearing on the questions raised here.

Modularity and Language Acquisition

Although it is theoretically independent, another hypothesis is often connected with the modularity hypothesis: the nativism hypothesis. According to the nativism hypothesis, some aspects of the language module are innate. The nativism and modularity hypotheses can be presented together with the principles and parameters theory of grammar (e.g., Chomsky, 1981), which maintains that the principles and parameters of Universal Grammar are innately given. Because the parameters allow for linguistic variation, the child must be exposed to a particular language to "set" them at the appropriate values. However, since the range of possible linguistic variation is limited, the child's job is greatly reduced in comparison to hypothesis testing or other theories of learning.

Just what does the child need to learn? One important proposal is the lexical hypothesis (Borer, 1983). According to this hypothesis, the range of linguistic variation is restricted to the lexicon. Surely children need to learn the words of their language, since there is a wide variety between languages in lexical items (although even within the lexicon, there must be universals, such as the types of lexical or functional categories that are available, lexical redundancy rules, argument structure and lexical semantics, etc.). According to the lexical hypothesis, all variation is in the lexicon. Parameter settings, according to this view, are associated with particular lexical items.

What evidence can be adduced in regards to the nativism hypothesis? One of the strongest theoretical motivations for the nativism hypothesis is known as Plato's problem. The problem is this: How is it that we know so much, given our limited experience? This question can be raised in numerous domains (Plato raised it in connection with geometry), but there is clear evidence for it within language. By the time a child has an adultlike grammar (say, by age 3), the child obviously "knows" things in his or her mental grammar for which he or she never received evidence. Many studies of the child's language acquisition circumstances have made it clear that negative evidence—that is, input that certain strings are not grammatical sentences—

cannot be part of the input the child brings to the language acquisition task (see, e.g., Morgan & Travis, 1989, and Lasnik, 1989). But without innate knowledge, negative evidence would be crucial for the child to respect constraints, such as the constraint that rules out example (1d) below. In the lack of negative evidence, a constraint must prevent the child from ever trying to produce (1d), even on analogy with (1b), for example.

- (1) a. I like ice cream with chocolate.
 b. What do you like ice cream with?
 c. I like ice cream and chocolate.
 d. *What do you like ice cream and?

The existence of constraints in the adult grammar has led many researchers to hypothesize that they must be part of the innate endowment that children bring to the language acquisition task (see, e.g., Chomsky, 1986, and Hornstein & Lightfoot, 1981). Other researchers have tested this hypothesis by looking at young children's language, and they have found overwhelming support for the innateness hypothesis. Children as young as 3 years can be shown to respect these constraints—in fact, they show this as soon as they can be tested on the appropriate structures. Although children make mistakes like saying "goed" for "went," they never produce sentences like (1d). (See Crain, 1991, for a review of evidence that children do respect constraints at an early age.)

So constraints, apparently, must be a part of Universal Grammar. Principles governing limited crosslinguistic variation are also proposed to be part of UG. Individual lexical items, on the other hand, are clearly not part of UG. Where is the line drawn between good and poor candidates for inclusion?

Three hallmarks of innateness have been proposed by Stephen Crain (1991). First, knowledge in the absence of experience is a candidate for innateness. Any constraint that is not learnable on the basis of positive evidence must be part of UG. Second, the proposed principles of UG should be universal. We therefore do not expect languages to differ in unlearnable ways. Third, early emergence in the course of acquisition is expected of universal principles (that is, there should be evidence that the child respects universals as soon as they can be tested). These hallmarks show us that language-particular, learnable facts are not candidates for UG, and thus, this is where languages can be expected to vary.

Suppose that this innate language module serves both spoken and signed languages. What predictions are made? There are two general predictions we

should consider. First, what predictions are made regarding the structure of signed languages? Second, what predictions are made regarding the influence of language on cognition?⁵

If one and the same module serves languages in either the visual or the auditory modality, then modality must be unspecified in the module (see Chapter 4). This means that the linguistic principles found to operate in signed languages cannot be significantly different from those found to operate in spoken languages. More precisely, taking the modularity hypothesis together with the nativism hypothesis, if any differences are found between signed and spoken languages, they must be learnable on the basis of the positive evidence that young children receive. This will be the topic of the next section.

Aside from this prediction concerning the structure of signed languages, the model presented has implications for the question of whether or not language (modality) affects cognition. Under the model, by the time the output reaches general cognition, information about the modality is no longer present. Hence, the modality of language use can have no influence on cognition. This hypothesis will be discussed in the final section of this chapter.

SIGN AND SPACE

If the same module that underlies the acquisition and processing of spoken languages underlies signed languages, what predictions can be made about the structure of signed languages? Signed languages should display the same characteristics as spoken languages, at least insofar as these characteristics are dependent on the hardwired specifications of the language module. In other words, signed languages should be different from spoken languages only as much as spoken languages are different from each other. If this is the case, then we are justified in claiming that the language module is amodal—principles of organization must be abstract enough to apply to either signed or spoken languages.

It is important to clarify which characteristics of language are dependent on the module. In studies of spoken languages, it has been found that certain (abstract) characteristics hold universally. Such properties are candidates for Universal Grammar, the hardwired specifications of the language module with which we are most concerned. Constraints that specify which elements of a sentence cannot be related by rules are prime candidates for UG, especially because they are not learnable. On the other hand, the presence of an optional rule (for example, the scrambling rule, which reorders constituents

in Japanese) is learnable since positive input will be present in the linguistic environment in the form of sentences in which the rule has been applied.

Thus, signed languages may well have some properties distinct from those of spoken languages and still be consistent with the modularity hypothesis, provided that the evidence for those properties is clearly available in the input. We would not expect, however, to find modality differences below the surface—that is, aspects of signed languages that are crucially different from spoken languages yet unlearnable in the sense described above.

Let us now briefly examine some characteristics of the structure of signed languages (in particular, ASL) to see how well the predictions of the modularity hypothesis fit with the facts. I will focus the discussion on those areas most likely to exhibit modality effects to see whether or not purported effects are learnable.

Syntax

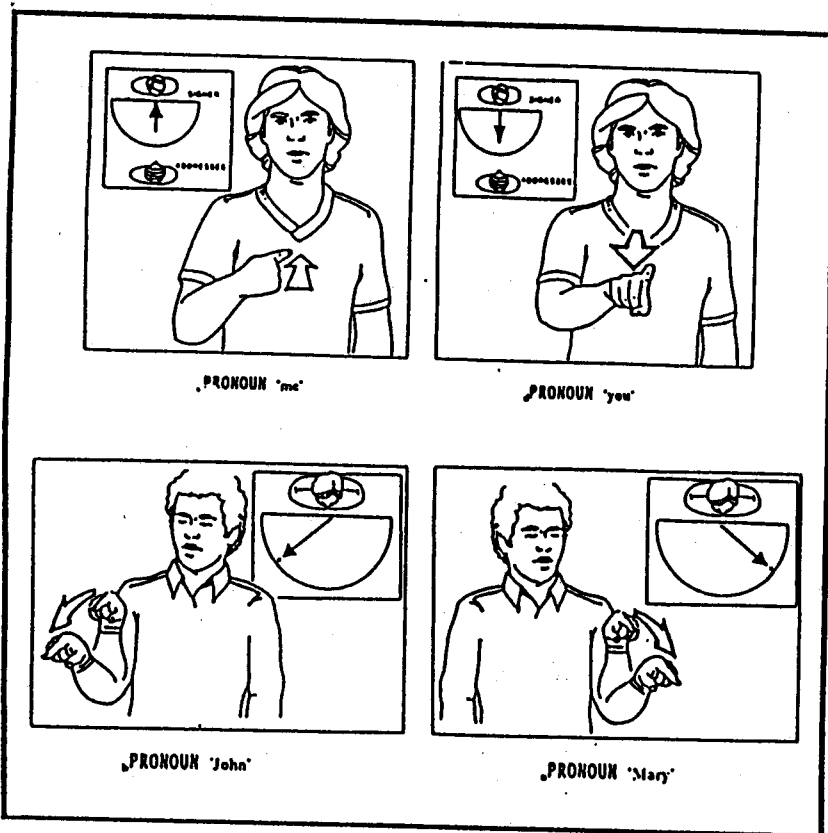
The research that has been done to investigate the syntax of ASL has in general found that it does adhere to the principles of UG. For example, Susan Fischer (1974) found that ASL obeys Ross's Complex NP Constraint (1967); Carol Padden (1983) found that ASL obeys Ross's Coordinate Structure Constraint; and I (Lillo-Martin, 1991) found evidence for both of these constraints as well as the Wh-Island Constraint and other subcases of Subjacency and the Empty Category Principle. Recent studies have proposed that ASL is not distinct from spoken languages in general in its phrase structure (e.g., Aarons, Bahan, Kegl, & Neidle, 1992; Boster, 1996; Liddell, 1980; Padden, 1983; Petronio, 1993), in wh-movement (Lillo-Martin & Fischer, 1992; Petronio & Lillo-Martin, 1997), and in other areas, although in many of these areas, ASL may be more akin to Chinese or Hungarian than English.

One area of ASL syntax that has attracted a considerable amount of discussion concerns the use of pronouns and other referential devices. In this domain there is the potential for strong modality effects, as evidenced by the term *spatial syntax*, which is frequently applied (e.g., Bellugi, vanHoek, Lillo-Martin, & O'Grady, 1988; Bellugi, Lillo-Martin, O'Grady, & vanHoek, 1990). Let me first describe this area in some detail to illustrate its spatial effects and then provide a nonspatial analysis.

When reference is made to people (or things) physically present in a signed discourse, pronoun signs referring to those people take the form of an index finger directed toward the referent. In simpler terms, for signers to refer to themselves, they point to their own chest; to refer to an addressee, they point to the addressee's chest; and to refer to some third person, they

point to that person's chest. To refer to people (or things) not present, a signer chooses a location for that referent and either "imagines" that the referent is located there or fixes the referent in that location, using the same pointing gestures (Liddell, 1990). These pronoun signs are illustrated in Figure 3.3.

This system is spatial in that spatial locations are used as locations toward which the pointing sign is made; unlike the locations of other signs (which are meaningless sublexical units), these locations are used to enable the signer to pick out different referents (i.e., meaningfully). (Liddell [1990] uses the terms *articulatory locations* and *location fixing* to distinguish between these two uses of space.) The result is that ASL pronoun signs seem to be quite different from the pronouns of spoken languages in four ways (see Lillo-Martin & Klima, 1990, for a discussion of the first three).



First, there is apparently an infinite number of ASL pronouns. This results because any spatial location can be used as a location for a referent. Although signers do not, in practice, use more than around three distinct locations for different pronouns in any discourse segment (presumably because they or their conversational partner would forget who goes where if a large number of distinctions were used), in principle, between any two locations used, another location for a pronoun can be chosen. This situation would seem to entail an infinite lexicon—surely an undesirable state of affairs. It would not seem possible simply to omit specification of location in pronoun signs since the location serves to distinguish referents—a semantic function. It is also clearly impossible to list pronoun signs in every possible location as different signs.

The second way in which ASL pronouns seem to differ from spoken language pronouns is that they are unambiguous in reference. Once a location is fixed for a referent, a pronoun directed toward that location picks out that referent only. An example is given in (2). The pronoun "she" in the English example is ambiguous since it can refer back to either Karen or Susan. (In context, it might be clear which referent is intended, but structurally either one is possible.) In contrast, in the ASL example there is no way to be ambiguous. The ASL pronoun must be articulated toward location *a* or location *b*, clearly identifying the intended referent (*t* indicates a marker for topicalization).

(2) a. Karen saw Susan yesterday. She had already finished the paper.

b. $\frac{t}{\text{YESTERDAY}}$, $\frac{t}{\text{KAREN}}$ INDEX $\frac{t}{\text{SEE}}$, $\frac{t}{\text{SUSAN}}$.
two possible continuations:

$\frac{t}{\text{PAPER}}$ PRONOUN WRITE FINISH.

$\frac{t}{\text{PAPER}}$ PRONOUN WRITE FINISH.

In spoken languages, pronoun signs frequently pick out a class of referents, identified by features such as person, number, and gender. Even in languages with more refined classes, such as the gender classes of Bantu languages, a pronoun will pick out one member of that limited class, and frequently the reference will be completely disambiguated by context, but the pronoun itself does not identify its referent unambiguously. Use of a pronoun in one class does not distinguish among multiple possible referents within that class. In ASL, on the other hand, the referent is unambiguously picked out by use of the pronoun signs.

Figure 3.3. ASL Pronominal Signs (from Lillo-Martin & Klima, 1990; copyright, Dr. Ursula Bellugi, The Salk Institute)

Although ambiguity is still averted, the third way in which ASL pronouns differ from spoken language pronouns concerns what is called *referential shift*. Under certain conditions, the reference of a pronoun changes. This referential shift is signaled by a shift in the signer's body position. For example, if "John" has been established in a location to the signer's right, the signer can then shift his body toward that location, with the result that the reference of pronouns changes. In particular, what changes is the reference of pronouns directed at the signer. Although normally taken as first person in reference, such pronouns will be interpreted as referring to some other person, made syntactically explicit in the relevant contexts. An example of referential shift is given in (3), where POV indicates a marker for point of view.

(3) $\overline{\text{a} \text{ JOHN } \text{a} \text{ POV } \text{b} \text{ PRONOUN LIKE } \text{b} \text{ JANE;}}^{\text{a} \text{ POV}}$

$\overline{\text{b} \text{ POV LIKE } \text{a} \text{ PRONOUN.}}^{\text{b} \text{ POV}}$

John_i (from his_i point of view) I like Jane;
and (she_j) (from her_j point of view) likes him_i.

A phenomenon similar to the ASL referential shift is direct discourse in speech; this type of context does give rise to pronoun shift in both types of languages. For example, both the English and the ASL versions of the sentence "John said, 'I'm leaving now'" contain an apparent first-person pronoun that has reference to someone other than the speaker/signer. However, in ASL the contexts for referential shift go far beyond direct discourse. Referential shift is used when the thoughts or actions of another are reported or when the report is made from another's point of view (Engberg-Pedersen, 1995; Lillo-Martin, 1995; Poulin & Miller, 1995). At first glance, this might seem quite different from the use of pronouns in spoken language and a use which is specifically available in the spatial modality.

The fourth difference between ASL pronouns and spoken language pronouns is that ASL pronouns apparently contain additional information about real spatial/topographical locations. When locations are fixed for nonpresent referents, there are two ways of going about choosing the locations to be used. In the first, the locations used are apparently arbitrary, and the choice of locations reflects a tendency for maximal perceptual salience. So, for example, "John" might be located to the signer's right side, and when "Mary," the next referent, is introduced, she might be located to the signer's left side (see Bahan & Petitto, 1980). The relative spatial relationships between John, Mary, and the signer are used purely syntactically and say nothing about their real physical locations.

However, apparently the same pronoun signs can be used with the intention of specifying exact physical locations. In this case, by establishing John on the right and Mary on the left, the signer is conveying the information that John was on his right and Mary on his left. This topographical use of space is utilized extensively to convey locative relationships. Furthermore, additional morpho-syntactic devices can make use of this same topographical space. For example, classifier signs, which have morphemes to represent the size and shape or semantic category of their referents, are frequently employed in this system to indicate precise physical locations. Although I am focusing on pronoun signs here, the same questions about the use of space can be raised for classifiers and other morphemes that use this system. This spatial representation of space would seem to be unique to the visual modality and a prime candidate for a true modality effect.

These differences between the ASL pronominal system and spoken language pronouns seem to stem directly from the spatial nature of ASL and therefore to undermine the strong modularity hypothesis outlined above. However, I will now present alternative explanations for the various effects of the ASL system. My alternative does involve modality effects of a sort, but they are learnable, and as I have argued, learnable modality effects do not pose significant problems for the modularity hypothesis.

First, following some of the arguments made in Lillo-Martin and Klima (1990) and Meier (1990), the ASL pronominal system can be analyzed as containing two pronouns: a first-person pronoun (_iPRONOUN) and a non-first-person pronoun (PRONOUN).⁶ All NPs in ASL, like NPs in spoken languages, are marked with a referential index. The referential index determines coreference and noncoreference between NPs at a level of interpretation, and NPs so marked with indices are subject to universal linguistic constraints. In ASL, unlike spoken languages, the indices are realized on the surface. This is the modality effect: although referential indices are needed in the grammar of all languages, regardless of modality, most likely only in sign languages will the referential index be phonologically realized. To my knowledge, no spoken language shows an overt manifestation of referential indices, and all of the sign languages that have been reported on do show this.

The phonological realization of the index requires that all co-indexed pronouns be articulated at the same location; contra-indexing requires different locations. The location used for a referent will remain constant throughout a discourse until it is changed by a new location being assigned by the signer (when, for example, he or she forgets where a referent has been located) or by one of a set of verbs that trigger such a change. When locations do not represent physical positioning, the choice of location will be determined by the perceptual salience strategies referred to above. I will return to the instances in which locations for referents do represent physical locations.

The analysis presented so far accounts for the first and second differences between signed and spoken language pronouns: there are two pronouns in ASL, not an infinite list, and the reference of each pronoun is determined by the referential index. Given this, the third difference might pose a larger problem: if the location of a pronoun (R-locus) corresponds to its referential index, then how will the pronoun be able to refer to a different person?

The solution to the third problem lies in the analysis of referential shift. I have analyzed the body movement that signals the referential shift as a predicate, which indicates that the following material is presented from the point of view of the subject of that predicate (Lillo-Martin, 1995). To illustrate, reconsider sentence (3) above, repeated below.

(3) $\overset{a}{\text{JOHN}}$ $\overset{a}{\text{POV}}$ $\overset{a}{\text{PRONOUN}}$ LIKE $\overset{b}{\text{JANE}}$;

$\overset{b}{\text{POV}}$ LIKE $\overset{a}{\text{PRONOUN}}$.

John_i (from his_a point of view) I like Jane_j;

and (she_j) (from her_j point of view) likes him_i.

In this sentence, there are two referential shifts. The first part of the sentence gives John as the subject, then the POV predicate appears, agreeing with John. From this point until the next POV predicate, the sentence is told from John's point of view. This means that any first-person pronouns will refer to John, not the signer. In the second conjunct, the matrix subject is null (cf. Lillo-Martin, 1986), but the reference of the null subject is unambiguous; since the POV predicate agrees with the location for JANE given in the previous conjunct, the subject of the second conjunct, and person whose point of view is taken in the remainder of the sentence, is Jane.

By what mechanism does the POV predicate allow the reference of pronouns in the following clauses to change? The ASL first-person pronoun following a POV predicate behaves much like the logophoric pronouns of West African languages such as Ewe and Gokana (cf. Clements, 1975; Hyman & Comrie, 1981; Koopman & Sportiche, 1989). Following the analysis of such logophoric pronouns proposed by Koopman and Sportiche, I (Lillo-Martin, 1995) have proposed that the ASL first-person pronoun can also act as a logophoric pronoun. The POV predicate takes a clausal complement that is headed by an operator. This operator binds all logophoric pronouns in its scope and therefore causes the shift in interpretation. Hence, the referential shift becomes fully accountable under the mechanisms already provided in UG.

A further point about the changes in reference under the POV predicate is required. Note that non-first-person pronouns do not change their reference.

This is important, as it shows that only the logophoric, first-person pronoun is affected by the operator. However, this might lead the reader to wonder what happens when first-person reference is desired (i.e., reference to the signer). One option would be to change the POV back to the signer; under the signer's point of view, the first-person pronoun would, of course, refer to the signer. However, if it is desired to make reference to the signer, still within someone else's point of view, the first-person pronoun will not do. Rather, the signer needs to be established at a new (non-first) location, and a non-first-person pronoun can be used to refer to him or her. In the unmarked case, the location used for the signer is not arbitrary, however, nor is it determined by the perceptual salience procedure. Rather, the signer's location will be established as the location that the signer occupied before the shift to another's point of view. So if under the shift the signer moved his or her shoulders to the right (facing toward the left), the new location for the signer under POV will be on the left. This new location falls within the use of linguistic locations for representing physical locations, to which I now turn.

There has been some debate in the ASL literature regarding whether or not there is a clear distinction between syntactic and topographical uses of space. (The main opponent of such a distinction is Liddell, 1990, 1995, but see Emmorey, 1996; Emmorey, Corina, & Bellugi, 1995; and Poizner et al., 1987, for evidence that the distinction is psychologically real.) Notwithstanding, both sides agree that spatial locations are in some cases strictly sublexical, but in others they convey information about locations. When location is used as a sublexical element, it need only be listed in the lexical entry for a particular sign—surely a learnable aspect of the language (later I will discuss sublexical aspects of ASL phonology in more detail).

When location is used to convey information about location, it is usually considered to have morphological status. Location is not different from hand configuration or movement in having this dual role—all three parameters can be sublexical or have morphological status. The only problem is that no detailed analysis of the location morphemes has been completed. Ted Supalla (1986) argued that the large variety of movements observed in classifier signs can be broken down into a small number of movement morphemes that combine sequentially or simultaneously to create more complex forms. However, he did not break down locations in the same way.

It is possible that location could be broken down into a small number of basic morphemes that can be combined in various ways. As with the movement morphemes proposed by Supalla, by utilizing simultaneous as well as sequential combinations of morphemes, a relatively small number of morphemes might be able to adequately represent the linguistically significant uses of spatial locations. Alternatively, Liddell and Johnson (1989) proposed

an analysis of space using numerous vectors, distances, and heights that resulted in over 100 different points in space toward which a sign could be directed. Although large, this number is finite, and this system thus represents a possible way of breaking down the visual-spatial input into modality-independent morphemes.

I should note, however, that Liddell (1995) claims that even the Liddell and Johnson model is not sufficient for representing the spaces that can be used. He argues that the locations are unlimited—hence unlistable, non-morphemic. Instead, he claims that “linguistic features can be used to describe the handshape, movement, and certain aspects of the orientation of the hand, but the locations the signs are directed toward cannot be described through linguistic features” (p. 36). If this is correct, the specification of how the nonlinguistic information is represented or processed will require extensive analysis.

With a morphological analysis of location, on the other hand, it is clear that no learnability flags need be raised. The surface realizations of these morphemes give overt evidence for their existence and analysis. Presumably, children learning this system will use a morphological analysis and not even attempt to learn the system as an iconic, analog mapping of space to space. This presumption is based on the results of numerous studies of the acquisition of other aspects of ASL, in which it is clearly seen that the iconic bases of certain signs (including pronominals [Petitto, 1983] and verb agreement [Meier, 1982]) do not play a role in their acquisition (see also Lillo-Martin, forthcoming, for review). There are no modality-specific constraints required here.

With a morphological analysis of the input, the modularity hypothesis is not violated. It is important that by the time the input reaches the syntactic processor, no modality-specific information is necessary. If, literally, a picture of the visual input is necessary for syntactic analysis, this would contradict my claims. But notice that a mental picture that arises as a result of the linguistic structure is not contrary to the claims made here. It is expected that such a mental picture might be constructed from the meaning of the morphemes and their syntactic analysis, just as with spoken languages.

Morphology

ASL morphology is rich and productive. Temporal aspect, agreement with person and number, distribution, instrument, location, and other information can be marked by morphological processes. In its form, ASL morphology does reflect its visual-spatial modality. Most morphological processes in ASL do not use affixation; rather, the form of the root itself changes. For exam-

ple, much of the verbal morphology uses characteristic changes in movement by altering the direction of the movement, changing the size of the movement, and/or adding repetition to the movement.

More specifically, ASL marks verb agreement with subject and object using the spatial syntax described above. An agreeing verb moves from the location associated with the subject to the location associated with the object, as illustrated in Figure 3.4.

I have already discussed aspects of the analysis of the spatial representation of (real or imagined) space, but there is still the issue of how the morphological combination is represented. This, too, was originally taken to be a modality-peculiar component of ASL grammar, and stress was given to the simultaneous nature of the morphological pieces (Klima & Bellugi, 1979).

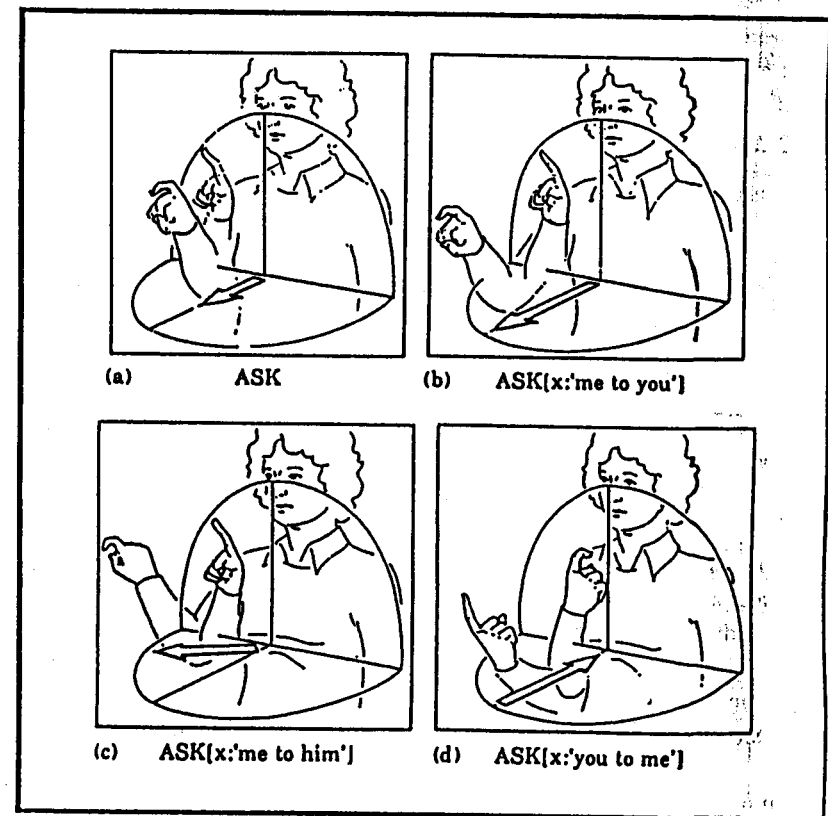


Figure 3.4. ASL Verb Agreement (from Klima & Bellugi, 1979; copyright, Dr. Ursula Bellugi, The Salk Institute)

However, although spoken languages do frequently use affixation for morphological processes, an analog to, analysis of, and representation of the ASL process is available by considering Semitic root-plus-vowel combinations in an autosegmental approach. For example, Liddell (1984b) proposed to represent ASL verbal roots as schemas with underspecified movements, with aspect as morphemes that can fill in the movement specifications. This kind of analysis indicates that ASL morphology isn't so strange after all. The analyses utilize the kinds of theoretical machinery that are necessary for spoken languages anyway, despite their superficially different appearance. The rules and principles of morphological combination—those aspects that would be part of UG—are constant across modalities.

Phonology

ASL signs have sublexical structure. (Stokoe [1960], wanting to be accurate in his use of roots, called the individual units *cheremes*, but currently researchers studying this level of ASL structure call it *phonology*.) For some time, the focus in describing sign sublexical structure was on the simultaneity of occurrence of the sign pieces. Signs can be described using one of a limited number of hand configurations, locations, and movements—all of which combine simultaneously (see, e.g., Stokoe, Casterline, & Croneberg, 1965). If simultaneous combination is a crucial part of sign structure, it might well seem to be a modality effect.

However, more recent research on ASL phonology has pointed out the importance of sequentiality (beginning with Liddell, 1984a), and almost all current accounts of ASL phonology include representations of both sequential and simultaneous information. In fact, current models of ASL phonology employ the same terminology, representational systems, and theoretical devices as current models of spoken language phonology (see, e.g., Brentari, 1990; Coulter, 1993; Liddell & Johnson, 1986; Padden & Perlmutter, 1987; Perlmutter, 1992; and Sandler, 1989, 1993). As one example, in Figure 3.5, I have reproduced a model of the structure of spoken language phonological representations from Clements (1985) alongside a model of the structure of ASL hand configuration representations from Sandler (1989).

The names of the features, the node labels, or the proposed tiers are often the only hints that the language whose phonology is being represented is potentially radically different. Here, of necessity, there is a modality effect, since the node labels are different and the skeleton itself has a different shape. But, importantly, the constraints on how to combine elements, what a hierarchy can and cannot look like, and what processes can apply are constant, regardless of modality. I take this as evidence that the same lan-

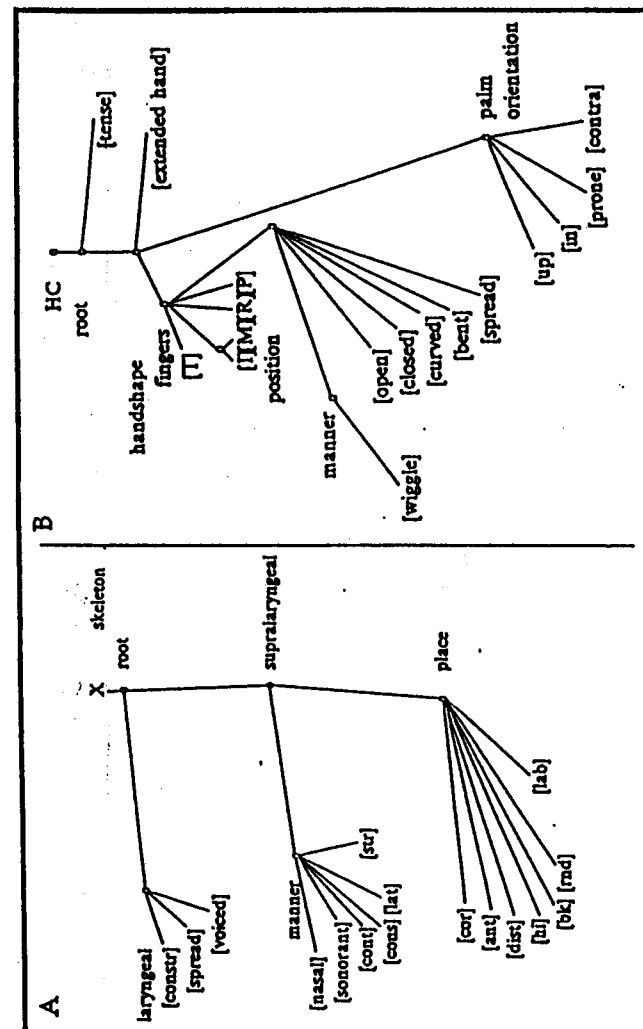


Figure 3.5. Spoken (A) and Signed (B) Language Feature Hierarchies (from Sandler, 1989). This model has been revised in Sandler (1995, 1996), where the terminal features shown here have been replaced by a smaller, more abstract set of unary components. The basic hierarchy of feature classes has remained unchanged.

guage module is employed in the processing of signed and spoken languages, once the most superficial input patterns can be translated into amodal, abstract units.

A Bimodal/Amodal Language Module

Let me put together the evidence reviewed so far and make my proposal more precise. I accept the arguments that language structure and processing display the characteristics of a module, roughly as defined by Fodor but amended following Chomsky to include submodules for different aspects of processing (including, in particular, auditory phonetic processing as one module that provides input to the next higher linguistic module). I take the similarities between signed and spoken language to be a good indication that the same computational system underlies both at some levels. In particular, those aspects of Universal Grammar that are amodal (including logical form, syntax, morphology, and parts of phonology and the lexicon) constitute modules that are common for signed and spoken languages. The input to the common modules must come through separate modules, processing auditory versus visual information. My proposed model is illustrated in Figure 3.6.

The diagram in Figure 3.6 shows separate phonetic processors for auditory and visual input. There is good evidence that auditory phonetic processing is modular. Some evidence for this was discussed in the previous section, and much of Fodor's book employs this kind of evidence. Whether or not there is a separate visual phonetic processor (and if so, how it develops) is less clear. Some research has pointed to specialized processing of sign linguistic information (e.g., Poizner, 1983; Poizner, Fok, & Bellugi, 1989), but more work is needed. I hypothesize the existence of a separate visual phonetic processor for sign languages, but this may turn out to need revision.

Does the proposal illustrated in Figure 3.6 violate domain specificity as Fodor intended it? Wendy Sandler (1992), in a very interesting paper on signed languages and modularity, has argued that signed languages do counterexemplify a Fodorian modularity, and the apparent violation of domain specificity provides part of her rationale. She does not consider a model like the one proposed here to be consistent with Fodor's claims since he argues for domain specificity and informational encapsulation of the language module using specifically auditory phonetic terms and evidence. However, I think the proposal made here is largely consistent with Fodor's proposal, and there is evidence that Fodor himself would agree. In the transcript of a panel discussion on the modularity of speech and language, published in

Mattingly and Studdert-Kennedy (1991), Fodor was asked about the kind of input that a module could accept. He replied:

I think it is not only possible but entirely likely that the notion of domain that you want for specification for domain specific systems would be quite abstract. In fact, it's more interesting, if it turns out that way. You can imagine a possible world in which there aren't any psychophysical constraints on linguistic exchanges at all. People can do them with little rubber balls, by waving their hands, or making noises in the back of their throats, or by doing songs and dances. It doesn't matter. Would it follow from that that there is no modularity thesis for language in that world? No, not at all. . . . That wouldn't be the death of the modularity thesis; that would be extremely interesting. (pp. 369-370)

Sandler points out another issue that deserves discussion here. She notes that certain linguistic phenomena seem to cluster in signed languages. As one example, I pointed out earlier that it is likely that all signed languages, and only signed languages, have phonologically realized referential indices. Sandler discusses the infrequency of linear morphological processes in signed languages in comparison to spoken languages and the abundance of nonlinear devices (such as displayed in ASL verb agreement) in signed languages. She says "Though not outside the confines of forms and processes predicted by phonological theory, sign languages appear to be universally distinguished from spoken languages in the ways described" (p. 339). That is, these putative modality effects are not cases of processes that fall outside the boundaries of UG but tendencies for certain UG processes, but not others, to appear in signed languages. According to the modularity hypothesis defended here, such characteristics are completely accidental: there is nothing in the model that predicts that sign languages will have certain characteristics more than others. This is an issue that deserves further thought; these phenomena do remain unexplained under my proposed model.

In summary, in this section I have shown that those differences that do exist between ASL and spoken languages do not undermine the modularity hypothesis. Rather, although the differences are surely due to modality, they are learnable because they rely only on the surface characteristics of the input. They do not violate the observed principles of UG. Hence, the data show that the structure of ASL is consistent with the modularity hypothesis.

Given this, we can ask what predictions the modularity hypothesis makes regarding the influence of the acquisition of a signed language on cognition. It should be clear that under the modularity hypothesis no differences are expected. I have argued that information about modality is not present in the language processor past the initial phonetic/phonological processor. Thus, if

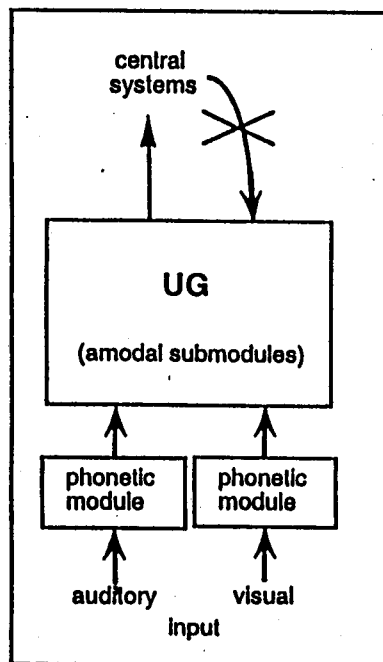


Figure 3.6. Amodal UG with Auditory and Visual Input Modules

the information flow is strictly one way, no information about modality will be present by the time linguistic information reaches general cognition, hence no effect on cognition is expected for signed versus spoken input. Is it the case that modality of language acquisition has no effect on cognition? Some researchers have argued that it is not. In the next section, I will review some of the arguments that have been made involving this matter. However, in the final section, I will argue that the observed differences are consistent with the modularity hypothesis, and I will show how to account for them.

SPATIAL LANGUAGE AND COGNITION

In order to look for possible effects of early sign language acquisition on cognition, it is relevant to look at studies of cognition in deaf children and adults. In this section, some such studies will be reviewed. However, it is important to point out that many studies have not clearly distinguished between deafness and early sign language as background variables. For this reason, many studies of deaf children and adults will turn out not to bear on

the issue under consideration. The discussion in this section will lead to and focus on studies that do make the distinction between deafness and early exposure to sign language.

Differences in Children

There was a time, not so long ago, when many researchers claimed that deaf children were "backwards" cognitively—that they displayed as much as several years' retardation in cognitive development compared to their hearing counterparts (for reviews, see, e.g., Hoemann, 1991; Marschark, 1993; and Wood, 1991). Such a difference might be attributed to the deaf children's early exposure to sign language, and the claim might then be made that language does indeed profoundly affect cognition. However, most of this earlier research failed to take fully into account the deaf subjects' language backgrounds. In fact, in many cases the putative intellectual (or achievement) delay was attributed to the deaf child's lack of language (i.e., spoken/written language), with very little, if any, attention paid to whether or not the student had been exposed to a signed language.

To my knowledge, it has not yet been firmly established to what extent language—any language—is a prerequisite for certain cognitive functions (see Chapter 1). Many scientists (including Piaget and Fodor) have recognized preverbal and nonverbal intelligence, leading to the conclusion that at least some thought is possible without language. However, the possibility that some other thought is dependent on the acquisition of language (but it doesn't matter which one) will not go away. Gale, deVilliers, deVilliers, and Pyers (1995) have resurrected this claim, with data indicating that language-delayed oral deaf children do not perform with age-matched controls on theory of mind tests, even when carefully tested nonverbally. Only when the students have passed certain complex grammatical tests of complementation do they pass the nonverbal tests.

But this finding is not about the effects of early exposure to a particular language; it concerns whether certain grammatical constructs, be they presented in English or ASL or Japanese, are prerequisites of certain cognitive functions. Here, I want to focus on the effects of early exposure to signed languages in particular, so I will try to avoid discussing claims that language very broadly affects cognition. This means that many early studies of cognition in deaf children will be excluded since they have ignored the presence or absence of signed languages versus spoken languages in the deaf students tested.

Other earlier studies were marred by the experimenters' failure to communicate with the child subjects; again, by ignoring signed languages, the

experimenters did not take advantage of what would frequently be the most effective communication method. Still other studies tested linguistic competence instead of general intelligence, confusing the two. Such observations have led most researchers to the conclusion that the early studies were too flawed to conclude anything about the relationship between deafness or language use and cognition.

Recognizing this, Hans Furth (1961) and others demonstrated that the apparent differences between deaf and hearing subjects disappear when children are tested on nonverbal measures, taking into consideration the differences between language and cognition. For example, Furth (1961) showed in a series of experiments that when language experience is taken into account, deaf children do not display a significant lag in cognitive abilities compared to hearing children. As he summarized 30 years later: "Overall then, the studies reported . . . indicate no clear general deficiency in the logical development of profoundly deaf persons, if anything rather a great similarity. Yet they are massively different in experience and competence in societal language. What implications can be drawn from these results for a theory of thinking and language and their mutual relation?" (Furth, 1991, p. 219).

The implication, clearly, is that knowledge of a specific language—or, to Furth, knowledge of any language—is not a prerequisite to specific types of logic. In numerous studies (see, e.g., Braden, 1985; Rodda & Grove, 1987; and Hoemann, 1991), an overall finding of no difference in cognition between deaf (signers) and hearing children has been supported.

However, Furth points out that most of his subjects are deaf children who not only are delayed in their development of "societal language" (i.e., English) but also may fail to receive sufficient exposure to ASL to have developed it along the normal time course for language acquisition. This is a problem that has plagued many studies of deaf children's cognitive abilities, even after it became clear that the procedures for testing deaf children must take language abilities into consideration. That is, most studies of deaf children's cognitive abilities—even those employing tests that examined nonverbal or performance intelligence using nonverbal, gestured, or even signed instructions—report the results without a clear distinction between groups of children who have had early, late, or no exposure to ASL. Thus, it is very difficult to interpret these studies, especially as they bear on the question at hand: whether early exposure to a signed language affects cognitive development.

More recently, some consideration has been given to this issue, although it is still not adequately controlled in many studies. In several cases, re-

searchers have compared groups of deaf children of deaf parents (DCDP) with deaf children of hearing parents (DCHP). When both groups are drawn from similar educational and social backgrounds, it is often assumed that only the DCDP have had early sign language exposure. While this is frequently the case, it should not be assumed that all DCDP have early exposure (usually only one deaf parent is required to classify a child as DCDP, but some deaf parents, especially those married to hearing people, do not sign or do not sign all the time), nor should it be assumed that no DCHP have early exposure, especially since this is becoming a more popular option in early intervention programs. Nevertheless, we can keep this in mind as we consider several studies comparing these groups.

What most studies comparing DCDP and DCHP have found is an advantage for early sign language exposure. Advantages have been claimed for educational and social achievement (e.g., Mayberry, Wodlinger-Cohen, & Goldin-Meadow, 1987; Meadow, 1967), as well as for cognitive functions. I will focus on the latter.

Carol Kusché and her colleagues (Kusché, Greenberg, & Garfield, 1983) noted previous studies that found an advantage for DCDP over DCHP and raised the important question, "Why?" Some studies explained the advantage as a function of the early use of manual communication (e.g., Meadow, 1967), while others appealed to differences in child-rearing practices between deaf and hearing parents of deaf children (e.g., Sisco & Anderson, 1980). Kusché et al. (1983) discussed a number of other factors that could be relevant and reasoned that a plausible, testable hypothesis was that genetic factors underlay the difference. In order to study this hypothesis, they collected data from 78 deaf high school students, including 19 DCDP and 19 DCHP controls, 20 deaf children with hearing parents but deaf siblings (DCDS) who were considered likely to have genetic deafness but not necessarily early sign language exposure, and 20 DCHP controls for the DCDS.

Kusché et al. (1983) found that the DCDP did outperform their DCHP controls on nonverbal IQ (WISC-R or WAIS) and reading (SAT) measures, and they furthermore found that the DCDS outperformed their DCHP controls on IQ and one SAT measure (language achievement). The DCDP were not significantly different from the DCDS on IQ. Since the DCDP self-reported earlier acquisition of sign language than the DCDS did, and the DCDS did not report earlier acquisition than their controls, Kusché et al. argued that "it is unlikely that better communication in infancy and childhood could explain" the higher intelligence of the DCDS compared to their controls and the lack of a difference between the DCDS and the DCDP. Instead, they offered a tentative genetic explanation: "We believe it is possi-

ble that natural, cultural, and/or historical selection have resulted in superior nonverbal intelligence for deaf individuals when genetic etiologies are involved" (p. 464).

There are, however, at least two reasons why the possibility that early sign language rather than a genetic advantage underlies the IQ difference between DCDP and controls should not yet be abandoned. One concerns the source of data on the subjects' early language acquisition. The high school subjects in this study were asked to complete a questionnaire that asked (among other things), "How old were you when you learned to sign?" The average age at which DCDP reported learning to sign was 3.11 years, and the age of acquisition was correlated with achievement scores. This indicates either that not all DCDP did have early exposure to sign (contrary to the previous assumption) or that the self-report was not always accurate. The second question concerns whether or not some of the DCDS did receive early input. Although their self-reported age of acquisition of sign language was 6.84 years, it is possible that some of these students had essentially early exposure to sign from older deaf siblings. Since the study does not report how many of the DCDS had older versus younger deaf siblings, the possibility that some of them had early exposure cannot be discounted.

To follow up on the question of whether early sign language or genetic factors plays a more important role in the observed advantage for DCDP over DCHP, Abraham Zwiebel (1987) studied 6-14-year-olds in Israel. His study included 23 deaf children with deaf parents and deaf siblings (DPDS), 76 deaf children with hearing parents and deaf siblings (HPDS), and 144 deaf children with hearing parents and hearing siblings (HPHS). All children were instructed orally; only the DPDS group could be expected to have had early exposure to sign language (Israeli Sign Language), while the HPDS group was considered to have had partial manual communication at home.

Zwiebel obtained scores for his subjects on three intelligence measures: the Snijders-Oomen Nonverbal Test for the Deaf, the Goodenough-Harris Human Figure Drawing Test, and teacher ratings of intellectual potential. He found that the DPDS group was superior to both of the other two deaf groups on all three intelligence measures used and equivalent to hearing controls (HC). He concluded: "It is the *environmental* variable that explains the superiority of the DPDS children" (1987, p. 19; emphasis in original).

If early exposure to sign language does confer an advantage on DCDP compared to DCHP, what kind of advantage is it? Is it simply the advantage of some language over no language, or is it more directly related to sign language per se? There are a few indications that it might be a particular advantage of exposure to a signed language rather than more simply any linguistic environment.⁷

Some studies of the putative performance IQ difference between deaf children with and without early ASL exposure have found that not only are DCDP superior to DCHP on some tests but also they are superior to HC. For example, Conrad and Weiskrantz (1981) cite Karchmer, Trybus, and Paquin (1978), who report nonverbal mean IQs for DCDP of 107.8 on the WISC; Sisco and Anderson (1980) found a mean of 106.7 on the WISC-R. If early exposure to sign language provides an advantage for DCDP simply because it allows them to have early, normal language acquisition, why should it result in scores higher than those for HC who also experience early, normal language acquisition?

Conrad and Weiskrantz (1981) argue that perhaps there is no advantage for DCDP over HC. They studied groups of 8-11-year-old DCDP (N = 38), DCHP (N = 19) with younger deaf siblings (who presumably have an inherited deafness with decreased chance of multiple handicaps but still no early exposure to sign language), and HC (N = 45) in Britain using the British Ability Scales. Unlike the earlier (U.S.) studies, they did not find significant differences among the three groups. They suggested that the earlier findings might have suffered from a sampling bias, although there is no direct evidence. Recall that Zwiebel also found that Israeli DCDP were equivalent to, not superior to, HC (although he also found them superior to DCHP, unlike Conrad and Weiskrantz).

On the other hand, Jeffrey Braden (1987) finds convincing the consistency of the finding that DCDP are superior to HC on performance IQ measures. However, he points out that it is imperative to separate the factors underlying intelligence scores in order to appropriately address the question of why DCDP should be superior to HC. He supposes that DCDP are faster in information processing than DCHP or HC. Although this was supported by his experiment with deaf high schoolers (31 DCDP, 31 DCHP, and 37 HC), his study also failed to replicate the advantage of DCDP over HC in IQ, tested using Raven's Progressive Matrices. Braden reports that the literature shows the highest scores for DCDP on timed IQ tests, while untimed tests such as the Progressive Matrices result in similar scores between DCDP and HC groups. Given the surprising nature of some of Braden's results, his hypothesis is not fully supported, but his observation that different components of nonverbal intelligence might be differently affected should be pursued.

Patricia Spencer and Linda Delk (1989), in a study comparing visual-spatial abilities and reading comprehension, discovered differential advantages and disadvantages for 77 DC (both DP and HP) compared to HC norms. Spencer and Delk found that deaf 7-8-year-olds performed significantly higher than HC norms on the Motor-Free Visual Perception Test

(MVPT), a test of receptive visual perception. On the other hand, the deaf subjects were significantly lower than HC norms on the Visual-Aural Digit Span Test (VADS), which tests recall memory for visually presented digit sequences. This comparison was not the point of Spencer and Delk's paper, and they do not attempt to explain it. However, it is the kind of difference that may turn out to be crucial to understanding what ways, if any, early sign language exposure may affect cognition.

One study that specifically addressed the ways in which early exposure to a signed language might differentially affect aspects of visual cognition in young deaf children was conducted by Ursula Bellugi and colleagues (Bellugi, O'Grady, et al., 1990). Since this study made specific hypotheses about how the particular superiorities displayed by DCDP over HC could be accounted for by early sign language exposure, I will discuss it in some detail.

Bellugi, O'Grady, et al. examined deaf children of deaf parents on a number of tests of spatial cognition and also found the deaf children to be superior to hearing children on some of them. It is important to point out that the subjects tested by Bellugi, O'Grady, et al. were exposed to ASL from birth by their deaf parents and/or older siblings. Thus, it is possible to connect any differences between the performance of the deaf children and the hearing children to the deaf children's early exposure to ASL. (Of course, with these populations it is impossible to rule out other possible explanations having to do with hearing loss itself; no deaf children without early exposure to ASL were tested as a control group in this study.)

One of the clearly striking differences was found on the Benton Test of Facial Recognition. In this test, the child sees black-and-white photographs of unfamiliar faces: in some conditions, a target must be matched from six alternatives; in other conditions, the alternatives are given with changes in angle or lighting. Norms are available for hearing children from the age of six years; but Bellugi, O'Grady, et al. tested deaf children as young as 3. They found that even the youngest of their 42 subjects performed better than the 6-year-old hearing norms, and the deaf 6-10-year-olds consistently outperformed the hearing norms.

Bellugi, O'Grady, et al. attribute the difference in performance between deaf and hearing children on this test to the importance of faces in ASL. In addition to conveying affect, as it does with hearing/speaking people, the face conveys grammatical information in ASL, and in many cases it is the sole source of such information (i.e., manual markers need not be present). Thus, deaf children exposed to ASL pay attention to faces more than hearing children do. They conclude, "This suggests that linguistic experience may impact on nonlinguistic cognitive development" (p. 293).

Another task that resulted in large differences between deaf and hearing children was developed by Bellugi, O'Grady, and colleagues. In this study, a film was made by attaching an LED to the fingertip of a consultant who traced pseudo-Chinese characters in a dark room. The resulting picture was a moving light in which some of the movements reflected the strokes of the character while others were transitions between strokes. Deaf and hearing Chinese first-graders were asked to write the symbol that each point-light display represented. As the responses to one item shown in Figure 3.7 make clear, the deaf children were much more adept at this innovative task than were the hearing children.

Alongside the results of these two tests supporting meaningful differences between deaf and hearing children, Bellugi, O'Grady, et al. also found no

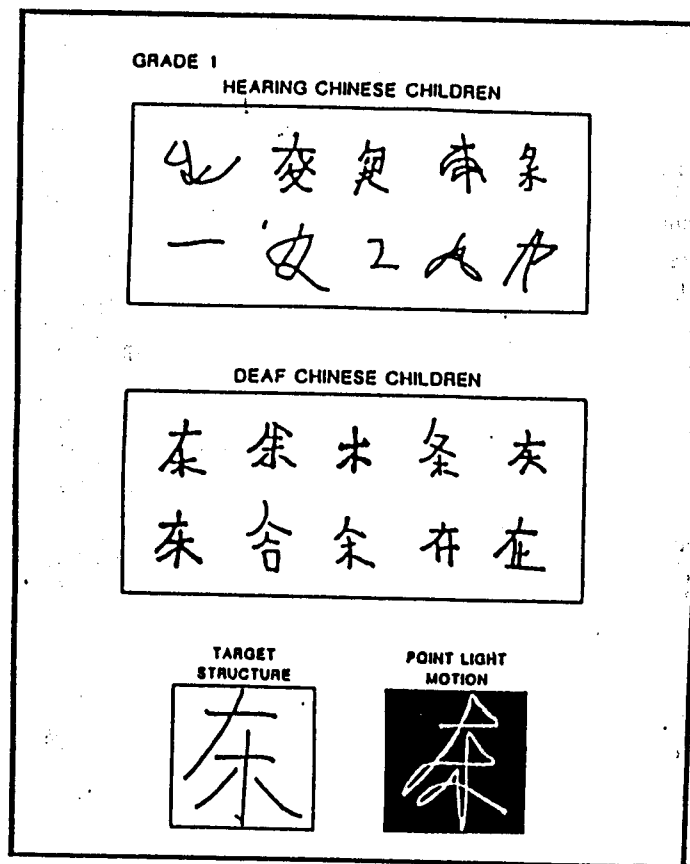


Figure 3.7. Chinese Pseudocharacters Test (from Bellugi et al., 1990; copyright, Dr. Ursula Bellugi, The Salk Institute)

differences between the groups on some tasks. For example, in tests of drawing, such as a subtest of the Boston Diagnostic Aphasia Exam and the Visual-Motor Integration Test, no obvious differences were found between deaf and hearing children. (Spencer and Delk [1989] also used the VMI in their study and similarly found no differences between deaf and hearing children.)

Putting together all of the evidence presented here, there is apparently some evidence for an advantage in some aspects of cognition among deaf children with early exposure to sign language. Certainly much more work is necessary to confirm this, especially with the aims of keeping the control groups consistent and investigating the nature of the supposed advantage more thoroughly. The advantage seems to be selective—only certain aspects seem to be enhanced. It is necessary to consider in more detail just which aspects are advanced and how precisely these effects could be related to early sign language exposure. More studies with children can help in addressing this question. In addition, some studies with adults have shed some light on it. We now turn to these studies.

Behavioral Studies with Adults

Karen Emmorey and colleagues (Emmorey, Kosslyn, & Bellugi, 1993) pursued the possibility that early exposure to a signed language would result in enhanced performance on some spatial cognitive tasks by testing three groups of subjects on a series of mental imagery and rotation tasks. Importantly, they were able to dissociate effects of early language exposure from effects of deafness by including a group of hearing children of deaf parents—native signers who are not deaf—in addition to a group of deaf signers and a group of hearing nonsigners.

In Emmorey et al.'s image generation task, subjects initially memorize the shape of printed uppercase alphabetic letters placed within a 4-by-5 square grid. During the task, they are presented with a lowercase written stimulus for which they are to generate the image of the previously learned letter. The test comes 500 milliseconds later, with an "X" placed somewhere on the grid (or within a set of brackets with no grid); subjects decide whether or not the "X" falls on a square occupied by the printed letter they recall. This test and its results are illustrated in Figure 3.8.

As is clear from the graph, both the hearing and the deaf signers are significantly quicker at this task than the hearing nonsigners but not significantly different from each other. On the other hand, the signers were not different from the nonsigners on an image maintenance task that used the

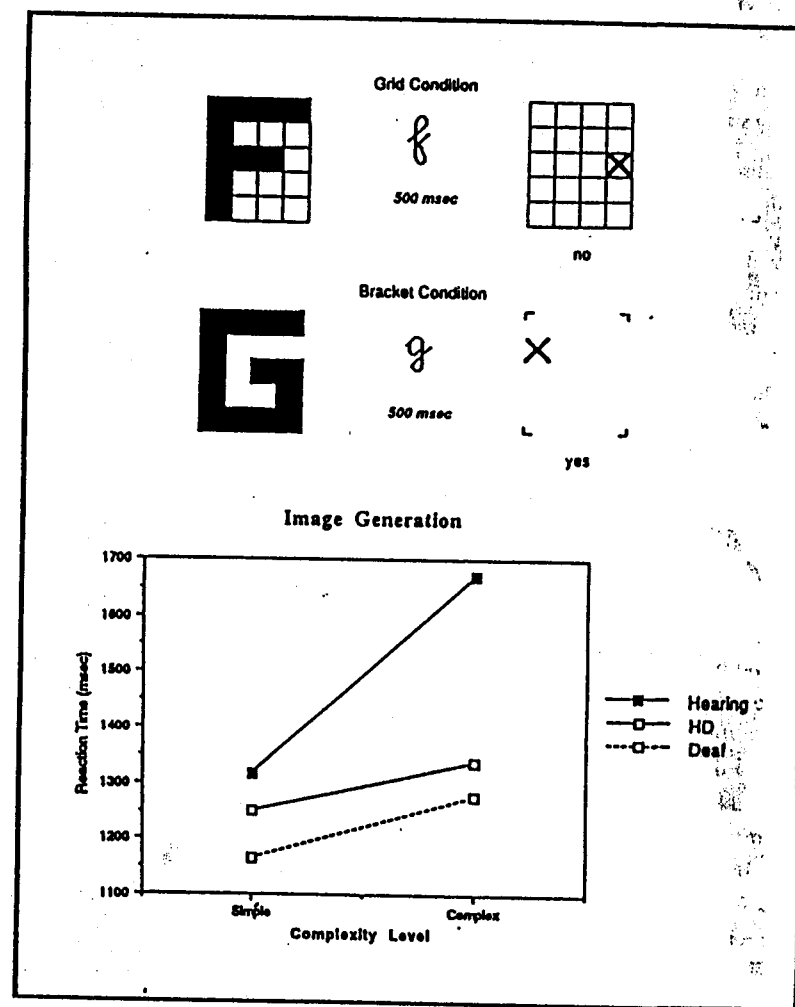


Figure 3.8. Image Generation Task: Sample and Results (from Emmorey et al., 1993; copyright, Dr. Ursula Bellugi, The Salk Institute)

same materials. In the maintenance task, the stimulus printed letter is presented to the subjects just before the probe "X" is presented.

The finding of a difference between the generation and maintenance tasks was followed up by a task examining mental rotation and mirror reversals. Subjects were presented with pairs of figures in which the left figure was upright and the right figure was rotated 0, 90, 135, or 180 degrees. Further-

more, the right figure was either the same pattern as the left or mirror-reversed. Subjects decided as quickly as possible whether the shapes were the same or a mirror-reversal. This test and its results are illustrated in Figure 3.9.

The results of this study showed that the deaf signing subjects were significantly faster than the hearing nonsigners at all degrees of rotation. As in the image generation task, the hearing native signers patterned like the deaf

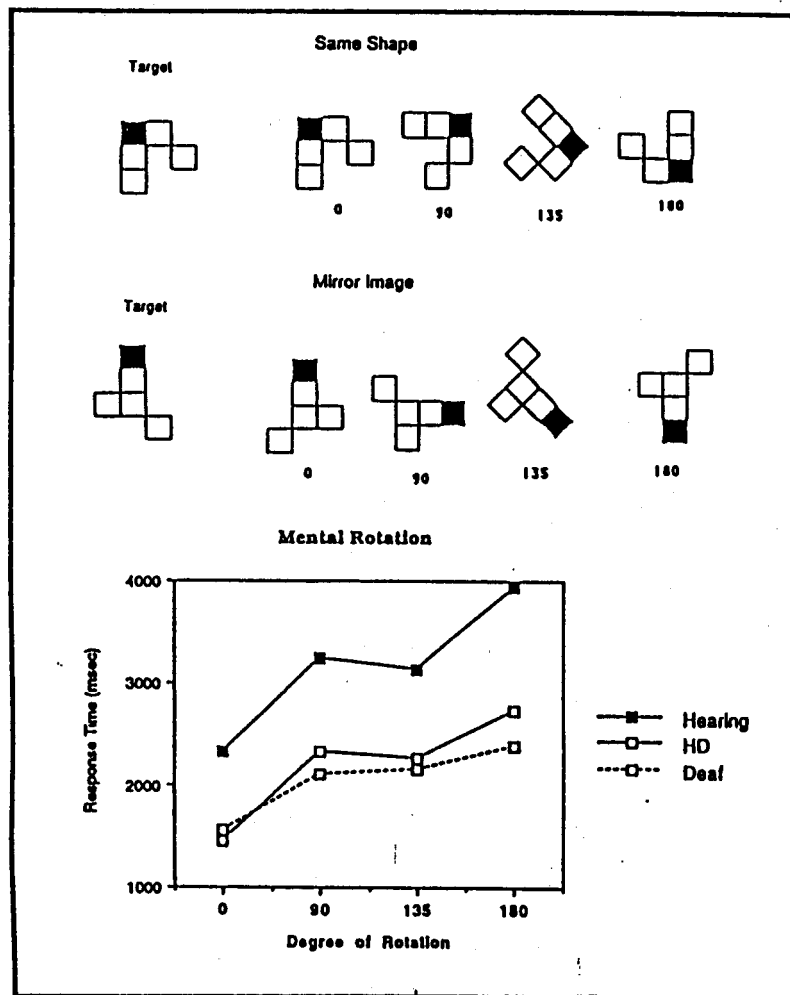


Figure 3.9. Mental Rotation Task: Sample and Results (from Emmorey et al., 1993; copyright, Dr. Ursula Bellugi, The Salk Institute)

signers—that is, they were significantly faster than the hearing nonsigners. Since the signers were faster than the nonsigners even in the 0 rotation condition, the results were interpreted as showing that the signers were superior to the nonsigners in detecting mirror reversals per se, not rotation.

How can the results of these behavioral studies be connected to the early exposure to sign language experienced by both the deaf and hearing native signers? As Emmorey et al. (1993) point out, both image generation and reversal interpretation are skills that would be frequently called on in the processing of a signed language. Referents in an ASL discourse are associated with locations in space and later referred to by use of these locations (see Liddell, 1990; Lillo-Martin, 1986; Lillo-Martin & Klima, 1990; Padden, 1983; and many others). In interpreting a pronoun, for example, that makes use of a location, a comprehender must recall the referent associated with that location or generate the image of the sign previously made there. Similarly, when locative relations are expressed in ASL through the use of the signing space, the signer typically uses the space from his or her own point of view, so the comprehender must mentally reverse the spatial relationships in order to compute the correct spatial array.

Hence, according to Emmorey et al. (1993), early exposure to sign language leads to an enhancement in spatial cognition, especially in certain domains that are crucially relied upon in sign language processing. This observation leads them to conclude: "Our findings suggest that the processing that underlies one sort of human language is not entirely modular. . . . Central aspects of ASL processing are not domain specific and are not insulated from other types of visual processing" (p. 30).

Is this conclusion necessary, given the model of modularity reviewed in the second and third sections of this chapter? Is there a way to make these observations compatible with the modularity hypothesis—and is there any reason to do so? Before addressing these questions in the final section, I would like to bring up one more type of data that suggests that early exposure to language does affect cognition, in spite of the predictions arrived at in the second section.

Evoked Potential Studies with Adults

Helen Neville (1991) used behavioral and electrophysiological methods to examine language and nonlanguage cognitive processing in the brains of deaf and hearing individuals, signers and nonsigners. In studies of language behavior, Neville and colleagues found that hearing nonsigners, hearing native signers, and deaf native signers all displayed characteristic left-hemisphere superiority for processing their native language, whether it was

speech or sign. However, late learners of a language do not show the same patterns for processing that language as do native learners. Thus, early exposure to a language affects the brain's organization for processing that language.

In the domain of nonlinguistic spatial processing, Neville and colleagues report a distinctive dissociation, separating the effects of auditory deprivation from the effects of early exposure to a signed language. Only the deaf subjects they tested showed use of the areas usually used in the service of auditory processing for visual-spatial processing. Thus, auditory deprivation leads to brain reorganization independent of early language exposure. However, both the deaf subjects and the hearing native signers showed strong left-hemisphere involvement during a task that required the detection of the direction of movement of a square presented in the visual periphery. The hearing nonsigners tested did not show left-hemisphere specialization for this task. Thus, early exposure to sign language apparently affects the organization of the brain for certain nonlinguistic behaviors. As Neville (1991) summarizes: "The modality through which language is first acquired significantly impacts the fundamental specializations of the two hemispheres for nonlanguage processing" (p. 269). This strong conclusion demands a serious examination of the proposed modularity hypothesis and its relation to sign language.

LANGUAGE AND THOUGHT 2: MODULARITY AND MODALITY

How can the modularity hypothesis summarized in the second section be compatible with the results summarized in the previous two sections? If language processing is informationally encapsulated from general cognition, how can the particular language a child is exposed to influence cognition? By the time information input to the language module reaches general cognition, all information about the modality is no longer accessible. Conversely, there is limited central access by general cognition into the internal representations of the module. Must we conclude that the modularity hypothesis is incompatible with the facts about sign language?

It is not necessary to draw that conclusion. Let us examine the workings of the language module once again, paying attention to the input side of the module. Fodor (1983) draws a distinction between vertical and horizontal processing. The vertical processors are the input modules, such as visual processing, auditory processing, and language. The horizontal processors cut across domains. Fodor generally discusses them in relation to the central

systems. Problem solving is a typical example of a horizontal faculty since it requires access to several domains. In addition to processes like problem solving, there are horizontal processes that are used in support of various systems, such as memory; Fodor calls these *computational resources*. There may well be competition across domains for limited computational resources, and Fodor admits to an uncertainty regarding just how autonomous they may be. In other words, the processors (including, for example, memory) that are used in the service of language may well be the same processors that are used for other, nonlinguistic tasks. Such a model would help to account for the results of multitask experiments, which show a decrease in, say, some aspect of language performance while engaging in a simultaneous nonlinguistic task, such as finger tapping.⁸

Since virtually all relevant studies have been concerned with the computational resources that underlie spoken language, the possibility that some aspects of spatial cognition count as computational resources for language has not been entertained within the modularity program. However, with the recognition of signed languages as natural human languages, and the resulting expectation that the same language module might underlie both signed and spoken languages, it should now be obvious that the computational resources that underlie sign language acquisition and processing might well include (specific) spatial cognitive processes. Let us consider this possibility using the diagram in Figure 3.10.

As the figure shows, input to the language module (UG) might come in either auditory or visual modalities, given the present hypothesis. Either way, computational resources are necessary in the service of the operations of the language module.⁹ So for spoken languages, certain computational resources are called on that might be independent of those used in the service of signed languages. For example, spoken languages may use rapid temporal phonological processing. On the other hand, signed languages may employ computational resources not used by spoken languages, such as certain spatial cognitive processes.

Now it should become clear how early exposure to a sign language might affect cognition without violating the modularity hypothesis or proving the Sapir-Whorf hypothesis. If early exposure to a language leads to the development or enhancement of certain cognitive skills, they may be precisely those that are used as its computational resources. Extra practice or, more likely, use during a certain period of development might lead to selective enhancement. Neville (1991) was able to place her results within a framework of neurological development that makes sense of the present data and is also compatible with the modularity proposal made here. She reports that in the development of the sensory systems (and their neural substrates),

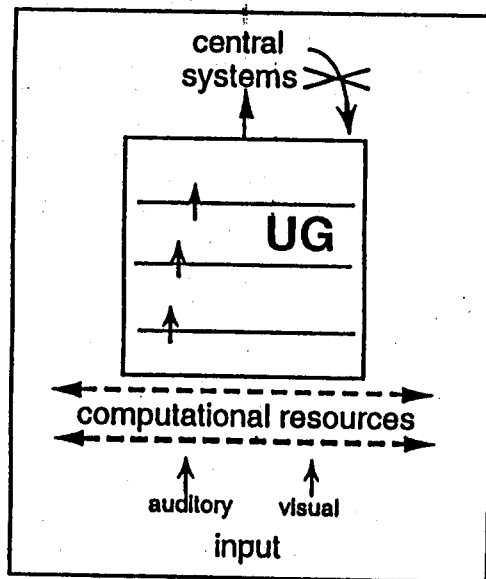


Figure 3.10. The Modularity Hypothesis plus Computational Resources

"there is an initial period of growth that is genetically influenced. . . . Those initial biases are strongly determined . . . but they are not immutable. . . . Following the early period of initial growth, there is a transient period of redundancy. . . . The subsequent pruning of these diverse connections occurs . . . as a direct consequence of activity that selectively stabilizes certain connections, whereas others that do not receive input are eliminated or suppressed" (p. 291).

It is in young childhood that the brain is most plastic, genetically driven but allowing for experience to leave its mark. The suggestion being made here is that early exposure to language may leave its mark not only in the brain's specialization for language but also in other domains of the brain.

According to the picture being painted here, early exposure to sign language can lead to enhancement of the spatial cognitive functions that underlie sign language processing. The question might then be raised: Why shouldn't early exposure to spoken language lead to similar enhancements?¹⁰ I think that it actually has been raised, but because of the ubiquitousness of spoken language acquisition, it has not been asked in these terms, nor can it be satisfactorily answered. To test the hypothesis that early exposure to spoken language enhances the computational resources that underlie spoken language, it would be necessary to compare the workings of

these resources in children or adults who have acquired a spoken language during childhood with a control group who have not. The control group, who have not acquired spoken language in childhood, must have normal intelligence and normal socialization. To control for the effects of auditory deprivation, it is optimal that the control group not be deaf or perhaps that a deaf group and nondeaf group be compared. Fortunately for society, but unfortunately for this experiment, such a (nondeaf) group does not exist. Even the hearing children of deaf parents, who were an appropriate control for the effects of auditory deprivation in the experiments reported here, are not an appropriate control group for this thought experiment because they do acquire spoken language in childhood. The closest group would be deaf children or adults. Thus, we have come full circle. To turn our original question on its head: What effects on cognition does not acquiring a spoken language in early childhood have?

As we have seen earlier in this chapter, although earlier claims were made about deaf children's cognitive deficiencies in comparison with hearing children, there are strong reasons to doubt such reports. However, there is one domain in which there may be differences between deaf and hearing individuals, even when the language differences are taken into consideration during test design and implementation. (Note that in this case, we are considering possible effects of lack of early acquisition of a spoken language, so deaf children with or without early exposure to a sign language are included.)

It has frequently been found that some deaf subjects have shorter sequential memory spans in comparison to hearing subjects or that the mode of rehearsal in short-term memory might be different (see Marschark, 1993, and Rodda & Grove, 1987, for some discussion). For example, Klima and Bellugi (1979) found that deaf native signers showed a shorter memory span for signs than hearing controls did for spoken words. Similarly, Conrad (1970, 1972) found evidence for differences between deaf and hearing children in their coding of English letters; whereas the hearing children showed evidence for sound-based phonological coding, the deaf children in general showed evidence for visual-based coding. On the other hand, Hanson (1982) found evidence for both (sound) phonological and visual coding by native signers, depending on the task. Although the nature and extent of differences in short-term memory between deaf and hearing subjects are in need of more detailed study, the possibility of real, language-related differences cannot be excluded.

The strongest differences between deaf and hearing groups apparently appear in sequential, temporal memory tests. This is a domain that has frequently been cited as crucial to the processing of spoken language. So per-

haps (hearing) individuals who are exposed to a spoken language in early childhood are at an advantage for certain types of short-term sequential memory processes, while individuals who are exposed to a sign language in early childhood are at an advantage for certain types of spatial cognitive processes. But, as Furth (1991) points out, "what effective difference does this behavioral difference make? It has been known for a long time that persons differ in their preferred imagery. . . . No evidence has yet been produced that these differences substantially affect important intellectual processes" (p. 223). In the model presented here, these differences in computational resources do not directly affect the central systems, which is the sense in which the language one grows up with does not affect cognition.

I conclude that the modularity hypothesis need not be abandoned, despite the results that indicate that exposure to a sign language in childhood may have as a consequence differences in certain areas of cognitive processing. Does this provide evidence in favor of the modularity hypothesis? Not directly. We started out with the modularity hypothesis by assumption or, more precisely, on the basis of the arguments put forth for it by others. We discussed evidence that some have taken to be in contradiction to the hypothesis and showed that these data are in fact compatible with it. This provides indirect support for the hypothesis, in that it is found to be consistent with data from a new domain, not considered in its original development. However, there are some areas that need to be more fully worked out, and I have not shown that a nonmodular account would be unable to explain the results presented here. There remains work for adherents of the modularity hypothesis to undertake to convince the reader that the hypothesis is not only viable but preferred. This is work I will endeavor to undertake. To cite one last quote: "Modularity is a potboiler" (Fodor, 1985, p. 33).

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NOTES

1. In very few cases, hearing children of deaf parents might go through a period with limited exposure to spoken language, despite having normal physiology and loving parents. (In most cases, these children receive input in spoken language from relatives, neighbors, friends, television, etc.) A few studies have examined the acquisition of spoken language in this population; however, I know of no studies that have examined phonetic processing or other relevant types of perception in subjects totally without spoken language input.
2. Actually, although I may use comparisons of language processing and visual processing on several occasions, Fodor's notions of a module are much more specific than this. He proposes as candidates for modules "mechanisms for color perception, for the analysis of shape, and for the analysis of three-dimensional spatial relations" (1983, p. 47). Similarly, he suggests that "computational systems that assign grammatical descriptions to token utterances; or ones that detect the melodic or rhythmic structure of acoustic arrays" may be distinct modules. This fine-tuning of modules to quite specific tasks is important to keep in mind throughout this discussion, despite the broad language used in the text.
3. This is one of the most debated of Fodor's claims about language, pitting those who believe that semantic, pragmatic, or real-world information might influence sentence processing against those who believe that such information is only able to correct a misanalysis or to choose between several possible analyses.
4. Considering the caveat of note 2, I find it completely plausible that speech perception may be served by a modular processor as well, and indeed, much of the evidence that Fodor adduces for the modularity hypothesis concerns speech processing. What I am now evaluating is the possibility that the computational systems that assign grammatical descriptions to token utterances may operate independently of modality.
5. There are also predictions about the processing of signed languages that should be considered in the light of the modularity hypothesis. In the interests of the current focus, I will leave this task for another time.
6. Lillo-Martin and Klima (1990) argued, in fact, that the system has only one pronoun, but following Meier (1990), I will adopt the first/nonfirst distinction.
7. This is not to downplay the importance of early (sign) language exposure for deaf children. I put the question this way simply because the focus of this chapter is on whether early exposure to a signed language has an effect different from that of early exposure to a spoken language (for a hearing child).
8. Not all modularists agree with Fodor in this way. For example, Crain and Shankweiler (1987) specifically argue that the working memory underlying language is part of the language module. It is not inconceivable that the two views might be compatible, if, for example, there are language-particular and cross-systems components of working memory.
9. The computational resources may be a third-dimensional base underlying all components of the grammar, not just an input processor. I will leave this possibility aside for the present.
10. I thank Carol Fowler for raising this question.

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In Support of the Language Acquisition Device

Diane Lillo-Martin

I hope that Chapter 3 and my comments here strongly support my position that deafness does not result in "cognitive poverty" (Conrad, 1979, cited by Campbell in Chapter 4). Rather, what I hope to have shown is that a child born deaf is essentially no different from a child born hearing—more specifically, both have the benefit of a modular language acquisition device that guides language acquisition but leaves (most of) the rest of cognition essentially unaffected. Thus, *given the appropriate (visual) input*, the language acquisition device operates as usual: the deaf child acquires a native language, and the rest follows.

I would like to take this opportunity, however, to clarify my interpretation of the workings of this language acquisition device, especially as it applies in special circumstances, since both Campbell and Siple have argued against its existence. I find the evidence for it convincing, particularly in the area of complex syntax—the area that has received the bulk of the attention from linguists working within this approach. Furthermore, I find the model completely compatible with the evidence given for noncanonical situations. As Campbell points out, such cases are germane, but I do not think that they contradict the essence of the theory.

Second, I will add a bit to the discussion in Chapter 3 of phonological awareness in deaf readers' processing of English text. This is a complicated issue, but I find a variety of data consistently pointing to the possibility that profoundly deaf readers can be phonologically aware and that this can be helpful for reading English.

Finally, I would like to briefly respond to a few issues in cognition that the other chapters address.

EVIDENCE FOR A LANGUAGE ACQUISITION DEVICE

In *Aspects of the Theory of Syntax*, Noam Chomsky (1965) proposed that linguistic theory ought to strive for "explanatory adequacy"—that is, an account for linguistic structure that also takes into consideration the limitations in input with which a child is faced. His proposed Language Acquisition Device (LAD) is an innate mechanism that takes as input the primary linguistic data to which a child is exposed and produces a grammar that is consistent with these data; furthermore, it goes beyond the input to correctly generate certain structures that were not part of the input and to recognize the ungrammaticality of certain other structures that were also not part of the input. It is because the data underdetermine the output that such a device is needed—language cannot be learned by inductive generalization alone.

This point is so important to the argument for innate linguistic knowledge that it is worth repeating some of the discussion in Chapter 3. By assumption, the input consists of overt instances of grammatical utterances spoken in meaningful circumstances. Certainly, false starts, fragments, and mistakes will be present in the speech that the child hears, but it cannot be assumed that ungrammatical utterances identified as such (negative data) are available for all children, therefore the theory must be able to account for language acquisition on positive evidence alone. Under these assumptions, partial generalizations pose a hefty challenge to a theory without innate constraints. Let me use an example slightly different from the one I use in Chapter 3.

- (1) a. I think that UConn will beat Syracuse.
b. I think UConn will beat Syracuse.
- (2) a. Who do you think that UConn will beat?
b. Who do you think UConn will beat?
- (3) a. *Who do you think that will beat Syracuse?
b. Who do you think will beat Syracuse?

Example (1) illustrates the fact that many embedded clauses in English declaratives can optionally be introduced by the overt complementizer "that." In example (2), we see the same is true for wh-questions formed on the object of the embedded clause. However, in example (3) we see that it is not true for wh-questions formed on the embedded subject—in these examples, the complementizer "that" cannot be present.

By assumption, the child learning English is exposed only to overt instances of grammatical utterances, so examples like (1a, b), (2a, b), and (3b) should be part of the input, but no information about the grammaticality or ungrammaticality of (3a) will be given. Based simply on the overt evidence, a learning theory without innate knowledge would almost certainly incorrectly predict that (3a) should be grammatical. An innate language acquisition device is needed to rule out examples like (3a): Clearly, the innate knowledge cannot be specific to English since there can be only one language acquisition device that must serve for all possible human languages. Thus, linguistic theory has for the last 30 years developed various accounts of the contrast in (1–3), comparing the phenomenon in English to similar phenomena in other languages, some of which do not show the same surface effects (see, e.g., Chomsky & Lasnik, 1977; Lasnik & Saito, 1992; and many others). To my knowledge, no proposed innate knowledge of a general cognitive sort (rather than linguistic knowledge per se) has been advanced to account for the contrast in (1–3), and it is difficult to see how any proposal that is not particular to language could do so.

Thus, the study of knowledge in the absence of experience provides one of the strongest kinds of evidence for innate linguistic knowledge. It is even possible to make some progress in linguistic analysis by the detailed study of one language or a group of related languages, even when making claims about Universal Grammar (UG) (part of the contents of the LAD), when the poverty of the stimulus is borne in mind. I agree with Siple (Chapter 2) that crosslinguistic work should support proposed universals. But even crossmodality studies using only one sign language are helpful tests of proposed universals, and generalizations and proposals about suspected universals can be profitably made from studying a limited number of languages.

In addition to knowledge in the absence of experience and crosslinguistic generalizations, a third type of evidence for the proposals of UG comes from studies of young children. Innate constraints guide language acquisition by preventing the child from making certain incorrect hypotheses. Thus, we expect to find evidence that children adhere to the principles of UG from a very young age. Of course, the examples in (1–3) are complex. Children do not produce (many, if any) such long-distance wh-questions before the age of 4 or so. But when they do produce such questions, they are not expected to violate the universal constraints that account for the ungrammaticality of (3a).

For further discussion of the issues raised here in support of the UG model of language acquisition, I recommend Stephen Crain's article in *Behavioral and Brain Sciences* (1991). Now I would like to turn to the predictions that this model makes about language acquisition in situations other than that of a young, mentally intact child acquiring a first language.

One of the clearest predictions of this model for noncanonical populations is the potential for dissociations between language and cognitive development. Since, according to this model, UG is modular (specific to language), language acquisition does not depend on cognitive development or social interaction. Of course, language acquisition requires input—a child exposed to English acquires English, after all—but the logical possibility exists for double dissociations between normal (or near-normal) language development in the face of severe cognitive or social deficits and vice versa.

For example, suppose some child (or children with a particular disorder) has a severe mental impairment across a range of cognitive tasks. If language acquisition were based on general cognitive principles, then impairment in cognition would be predicted to result in impairments to language. On the other hand, the UG model allows for the possibility that cognition will be impaired but language preserved. In particular, the model allows for the sparing of those aspects of language governed by UG, even if language-particular aspects (e.g., certain morphological idiosyncrasies) are affected. Children with Williams Syndrome (WS) exhibit just such a dissociation between language and cognition, as pointed out by Campbell. As Bellugi, Marks, Bihrlé, and Sabo (1988) put it, in WS children, the

expressive language is complex in terms of morphological and syntactic structures including full passives, embedded relative clauses, a range of conditionals and multiple embeddings. The children's sentences are complex and the syntax correct, although there are occasional "errors" of overgeneralization of morphology and pronoun usage. Despite these occasional weaknesses, it is interesting that the children spontaneously use specific linguistic structures (such as full reversible passives) in the absence of their purported cognitive prerequisites or concomitants. (p. 183)

Campbell notes that WS children make mistakes in prepositions and syntactic agreement. Depending on the particular errors, these too can probably be classified as language-particular matters of the sort that must be learned, not linguistic universals. If so, WS children provide an example of the dissociation between language and cognition that the UG model expects.

Similarly, autistic children often display a dissociation between advanced language and poor social skills (see Chapter 4). Using the same logic, we can see that a theory of language development dependent on social interaction would not expect to find language preserved in the face of such severe social deficits. On the other hand, this possibility is consistent with the UG model.

The dissociation may be double, at least in the case of language impairment, also cited by Campbell. An independent language module might be

selectively impaired, despite normal intelligence and socialization. Gopnik (1990) argues that this occurs in some cases of developmental dysphasia.

What about the critical period hypothesis? Is UG available for second-language acquisition in adults? What about late first-language acquisition?

The UG model makes no particular prediction about the critical period hypothesis. That is, taken by itself the UG model does not lead to the expectation that there would be a critical period for first- or second-language acquisition. It is possible, as Lenneberg (1967) proposed, that due to neural changes the language acquisition device becomes unavailable to postpuberty learners, but there does not seem to be uniform support for this proposal. Other cognitive or social differences between adults and children could affect late second-language acquisition, as proposed by Newport (1990) (cf. Chapter 4). But Newport herself acknowledges that her proposal could be taken together with the continued existence of a language acquisition device: "Note that such an explanation might be agnostic on whether innate constraints particular to language also exist; it would merely hypothesize that these language-particular constraints are not the locus of maturational decline in language learning" (p. 22).

A growing number of linguists, in fact, studying second-language acquisition with the benefit of the UG model, are finding that the poverty of the stimulus argument for UG in first-language acquisition also applies to second-language acquisition (e.g., Flynn & O'Neil, 1988; White, 1995). Thus, any differences between first- and second-language learners do not disprove the UG model for either situation.

Late first-language acquisition may be different. Few situations are available for study in which a child who is not otherwise deprived or impaired fails to receive input in time for normal first-language acquisition. According to the view defended here, deaf children of hearing parents are usually members of such a group. Deaf children of deaf, signing parents are not; their normal course of language acquisition, as outlined by Siple (cf. Lillo-Martin, forthcoming), indicates that being deaf is *not* bad for language acquisition (see Chapter 4). Not being exposed to an accessible language is.

In the case of late first-language acquisition of American Sign Language (ASL), studies by Galvan (1989), Mayberry (1993a, b), and Newport (1988, 1990) cited by Siple (Chapter 2) indicate that there is a long-lasting disadvantage. Again, the UG model does not specifically predict such an outcome. One important question is whether the linguistic deficits found in late first-language learners are constraint violations. If not, this would suggest the availability of UG, combined with additional factors that make using it less effective in these circumstances. This possibility is entirely consistent with both Newport's account of the disadvantage of language-learning in adulthood and the UG theory.

Thus, some of the evidence from special populations supports the UG model; the rest does not contradict it. One might then ask, as Siple does in a slightly different domain, why would a theory of language acquisition include both UG and aspects of learning? The answer, to me, is clear: both are needed. UG is necessary, especially in the cases of constraints, as illustrated in (1-3) above. But languages differ in lexicon, in morphology, in parameter settings, in the periphery. The Language Acquisition Device may well be used in acquisition in these areas, but where there is more linguistic variation, more learning in some sense is required. I find no conflict in having both. Learning is needed. Although Siple argues that "it is possible to account for some aspects of syntactic development with more general rules of cognitive processing," however, I am utterly unconvinced that it is possible to so account for all aspects, especially in the face of the poverty of the stimulus. Thus innate knowledge is needed as well.

PHONOLOGICAL AWARENESS AND READING

Numerous studies with hearing children have found that skilled reading is associated with phonological awareness, such as the ability to separate words into their component sounds and manipulate these sublexical elements (see, e.g., Liberman & Shankweiler, 1985). Studies with deaf readers have also found that skilled reading is associated with phonological awareness (see, e.g., Hanson, 1989). Why should this be so?

It has been proposed that for hearing individuals, working memory operates most effectively using phonological coding (Baddeley, 1979; Shankweiler & Crain, 1986). Whatever the reason for this, it clarifies the relationship between phonological awareness and reading: a more efficient working memory can be used more effectively in the service of higher levels of processing. A breakdown at the lower levels of reading (phonological analysis/working memory) will have implications for overall reading success.

Should phonological awareness be out of reach for individuals who cannot hear? Not at all. Phonology is an abstract notion, and phonological awareness might be derived through experiences other than sound. Experiences with orthography, speech training, and even explicit training in the phonological relationships among English words through pictures might be used for the development of phonological awareness in deaf individuals (on the latter, see Lillo-Martin, Hanson, & Romano, forthcoming).

Apparently, many deaf children do not succeed in developing phonological awareness. But this is not a necessary consequence of hearing loss, nor

is it exclusive to deaf children; after all, some hearing children similarly do not succeed in developing phonological awareness (and become poor readers). Are alternative strategies available? Perhaps, as Campbell speculates, there is more flexibility in coding strategies for deaf readers (see also Treiman & Hirsh-Pasek, 1983), but so far the bulk of the evidence suggests that phonological coding is the most effective strategy for reading English, even for deaf readers.¹

Does the connection between phonological coding and success in reading English put the signing deaf child at a disadvantage over the oral one? Of course, as Campbell points out, the child whose first language is ASL may show patterns of second-language acquisition in English (as found, for example, by Bochner, 1978, and Lillo-Martin, 1992). But I do not see why this means that "the child who uses speech may be *relatively* less disadvantaged in this domain," as Campbell states. Only if the orally trained deaf child succeeds in learning English through speech prior to learning to read will he or she be "less disadvantaged," for then the process of learning to read will be a process of mapping known forms onto new symbols, as it is for hearing children. If the orally trained deaf child has not acquired English prior to attempting to learn to read, then he or she will need to learn a linguistic system without any efficient language through which to teach it. The signing deaf child must master a second language, but at least the first is available for use in the education of the second.

In other words, only the orally trained deaf child who is blessed with the "magic ingredient," as described by Campbell, that enables some to succeed in acquiring language in a totally speech-based world will be relatively less disadvantaged. In the absence of prior knowledge of which children will be so blessed, I find the hope of magic to be an insufficient justification to deprive children of a first, accessible language.

DEAF CHILDREN AND SOCIAL COGNITION

Campbell worries that deaf children are disadvantaged in the development of social cognition because of their lack of auditory cues for various events. I would like to point out two types of studies in response to this, although I cannot do much more than point them out.

First, Erting, Prezioso, and O'Grady Hynes (1990) have studied the interactions of deaf mothers with their deaf children. They show that deaf mothers draw their babies' attention to events through visual cues, but hearing mothers of deaf children do so less often, perhaps because they are unaccus-

formed to the need for a visual cue. Perhaps parents can compensate for the potential problems Campbell poses by using visual cues to substitute for the missing auditory ones.

Second, Campbell asks, "Are deaf children disadvantaged in relation to hearing and seeing children in developing the bases for understanding about human minds?" A recent study by Gale, deVilliers, deVilliers, and Pyers (1996) argues that for both deaf and hearing children, a mature theory of mind is dependent on the development of the linguistic structures that are used in expressing cognition propositions. Thus, according to them, deaf children are not disadvantaged per se, but only if their language acquisition is delayed.

NOTE

1. In fact, contrary to Campbell's claim, some researchers have found evidence for phonological awareness in readers of Mandarin (Ren & Mattingly, 1990). This finding suggests the importance of phonological coding as the most effective strategy for reading in general.

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