

Language, tools and brain: The ontogeny and phylogeny of hierarchically organized sequential behavior

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Abstract: During the first two years of human life a common neural substrate (roughly Broca's area) underlies the hierarchical organization of elements in the development of speech as well as the capacity to combine objects manually, including tool use. Subsequent cortical differentiation, beginning at age two, creates distinct, relatively modularized capacities for linguistic grammar and more complex combination of objects. An evolutionary homologue of the neural substrate for language production and manual action is hypothesized to have provided a foundation for the evolution of language before the divergence of the hominids and the great apes. Support comes from the discovery of a Broca's area homologue and related neural circuits in contemporary primates. In addition, chimpanzees have an identical constraint on hierarchical complexity in both tool use and symbol combination. Their performance matches that of the two-year-old child who has not yet developed the neural circuits for complex grammar and complex manual combination of objects.

Keywords: brain; chimpanzee; construction; cortex; development; evolution; language; language development; neural development; ontogeny; phylogeny; primate; tool use

This target article has two goals: The first is to relate the ontogeny of hierarchical organization in speech and in combining objects with the hands (henceforth "manual object combination") to brain development and brain function. The construction and use of tools are particular instances of object combination. The second goal is to explore the evolutionary roots of language, tool use, and their neural substrates by examining evidence from contemporary primates.

In manual object combination, the hands are used to put two or more objects together, as in tool use or construction activity. The following examples indicate how (1) tool use and (2) construction activity involve manual object combination: (1) The hand holds a hammer, which strikes a nail held by the other hand, and (2) two pieces of pipe are manually screwed together to make a longer piece of pipe.

In hierarchical organization, lower-level units are combined or integrated to form higher-level ones. As an example of hierarchical organization applied to construction activity, suppose the above-mentioned pipe is part of the process of building a house. The two pieces of pipe are lower-order units relative to the longer pipe. The longer pipe is then joined with other elements to construct the higher-order unit, a shower. The shower is combined with other units at the same level (e.g., a toilet, itself composed of lower-order units) to make the still higher-order unit, a bathroom, and so on.

Human language is also hierarchical in structure.

Phonemes, the sound units of language, are combined to form morphemes or words, the meaning units; these in turn are combined to form sentences, the propositional units; finally, sentences can be combined to form the discourse level of human language (Hockett 1960). An important fact for present purposes is that each level grows in hierarchical complexity as ontogenetic development unfolds.

The relationship between language and object combination, including tool use, has important implications for "cognitive modularity." According to Fodor's (1983; see also multiple book review of Fodor: *The Modularity of Mind*, *BBS* 8(1) 1985) basic notion of modularity, language and object combination would be separate cognitive modules if each were (1) genetically determined, (2) associated with distinct neural structures, and (3) computationally autonomous.¹ The emphasis in this article is on the second criterion. I therefore ask how distinct the neural mechanisms responsible for language are from those that are responsible for tool use and other forms of object combination. The question is approached both ontogenetically and phylogenetically.

The existence of a common neural substrate for language and object combination would be evidence against the hypothesis that these capacities draw on two independent modules, whereas the existence of two distinct neural substrates would be positive evidence for the modularity of these two functions. Developmental data should be particularly useful for understanding the rela-

tion between language and object combination because any search for neural substrates must take into account the fact that the human brain is not static after birth; it undergoes a great deal of postnatal development.

1. Hierarchy in language and object combination

The next two sections show that both object combination and language attain increasing hierarchical complexity as ontogenetic development proceeds.

1.1. The development of hierarchical organization in manual object combination

Lashley (1951) was the first psychologist to notice that complex serial behavior could not be explained in terms of associations between contiguous acts; order must be generated by some higher-level organization. Manual object combination tasks have formed the basis for a research program on the development of hierarchical organization in children (Beagles-Roos & Greenfield 1979; Goodson & Greenfield 1975; Greenfield 1976; 1977; 1978; Greenfield & Schneider 1977; Greenfield et al. 1972; Reifel & Greenfield 1981). Systematic development toward increasingly complex hierarchical organization has been repeatedly observed for object combination in every medium: nesting cups (Greenfield et al. 1972), nuts and bolts (Goodson & Greenfield 1975), construction straws (Greenfield & Schneider 1977), blocks (Greenfield 1976; 1977; 1978; Greenfield & Hubner n.d.; Reifel & Greenfield 1981), and two-dimensional pictures (Beagles-Roos & Greenfield 1979).

As an example, let us take the strategies for combining nesting cups shown in Figure 1. The first manipulative strategy for combining the cups, pairing, involves an asymmetric relationship in which a single active object acts on a single static one. In the second strategy, called the "pot," multiple active objects act on a single static one. In the third strategy, the subassembly, two objects are combined into a pair, which is then manipulated as a single unit in the next combination (Step 2). The strategies develop in this sequential order beginning at 11 months of age (Greenfield et al. 1972). With respect to hierarchical organization, Strategies 1 and 2 involve only one level of combination: Two or more cups are combined in a chain-like sequence to make the final structure. In Strategy 3, the subassembly method, there is an additional level of hierarchy: Two cups are combined to form a higher-order unit, which is in turn combined with a third cup to make the final structure.

Given that the subassembly strategy develops last, the developmental progression is toward increasing hierarchical complexity. As suggested by the developmental theory of Heinz Werner (1957), hierarchical complexity in construction activity can be taken as an index of "manual intelligence."

That the patterns of development of hierarchical organization may be universal is suggested by the fact that they were also exhibited by the Zinacantecos, a Maya Indian group in Southern Mexico, in two kinds of object combination tasks, nesting cups where the sequence has just been described (Greenfield et al. 1989; Greenfield & Childs 1991; Greenfield et al. 1972) and the constructing

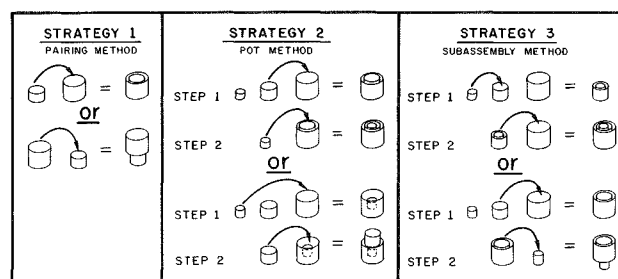


Figure 1. The development of strategies for combining nesting cups. Strategy 1 precedes strategy 2, which in turn precedes strategy 3. The age range of children tested was from 11 to 36 months of age (Greenfield et al. 1972).

of striped patterns with wooden sticks (Greenfield & Childs 1977).

The construction of striped patterns by placing wooden sticks in a frame showed a similar developmental sequence toward increasing hierarchical complexity. For example, whereas younger children could accurately reproduce patterns in which a pattern unit was created by combining sticks of two colors, only older children could reproduce patterns in which two different units, each composed of a different combination of two colors, had to be combined to form a higher-order pattern unit (Greenfield & Childs 1977).

The hypothesis of an innate developmental basis for the nature and sequencing of object-combination strategies becomes even more compelling when one considers that Zinacanteco babies and children had no toys and very few object-manipulation materials in their natural environment. The development of increasing hierarchical complexity of the combinatorial strategies therefore occurred despite the introduction of unfamiliar materials and tasks by the foreign experimenters.

1.2. An example of increasing hierarchical complexity in grammatical development

As it develops, grammar becomes increasingly complex in hierarchical structure, as illustrated by the earliest stages in Figure 2. The child starts with one-word utterances (e.g., Figure 2a). In the next developmental step, two words are combined to form a higher order grammatical relation; for example, the relation of attribution is shown in Figure 2b. The next level of grammatical complexity finds adjectives and nouns combining to form a superordinate noun phrase, which, in turn, enters into a still higher order combination with a verb (Brown 1973). The latter can be exemplified by the utterance from Brown's (1973) corpus, *want more grapejuice*, which is diagrammed as a tree structure in Figure 2c. Comparing Figures 2a, 2b, and 2c makes the growth in hierarchical complexity clear.

Modern linguistic accounts of grammar also emphasize hierarchical structure in mature human language. Although Chomsky's original accounts (1957; 1965) of tree structure as a representation of both the underlying grammatical structure of a sentence and its surface manifestation are no longer popular, more recent analyses have not abandoned the centrality of hierarchical organization (e.g., Hyams 1986). There is widespread agree-

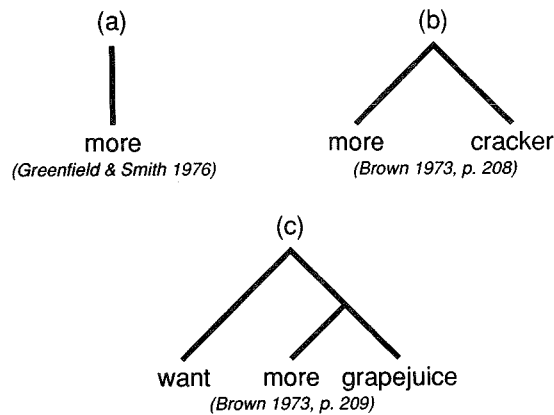


Figure 2. The development of increasing hierarchical complexity in early syntax. Nodes are not labeled in order to avoid a commitment to a particular theoretical description. The important point about (c) (which would not be disputed by any theory) is that *more* plus grapejuice forms a single complex unit, which, in turn, relates to *want*.

ment with Chomsky's (1959) argument that language cannot be analyzed as a sequential or Markovian chain of stimuli, but must be considered in terms of its hierarchical organization.

1.3. An example of parallel structural development in the domains of grammar and object combination

The earlier writings of Greenfield and colleagues emphasized the parallels between the development of object combinations and word combinations (grammar). Whereas Figure 1 presented the developmental sequence of manipulative strategies for combining nesting cups, Figure 3 depicts parallels between the same sequence and the development of children's sentence types, as formulated by Greenfield et al. (1972). Note that the grammatical analogies portrayed in Figure 3 involve quite complex structures. On the other hand, it would also be possible to say that *more cracker* (Figure 2b) involves a pairing strategy on the level of word combination, whereas *want more grapejuice* (Figure 2c) involves a subassembly. Although many analogies are possible, the problem of finding one based on the more interesting cognitive property of homology is of central importance in the sections that follow.

On the level of manual action, each combinatorial strategy constitutes a way of ordering sequential action by using a hierarchical organization of greater or lesser complexity to construct relations among objects. Grammar does the same for words. Each stage of object combination in Figure 1 appears to result from a developmental constraint on hierarchical complexity, lifted at the subsequent stage. For example, the child at the pairing stage (left side of Figure 1) seemed no more capable of creating a "pot" structure, the next level of hierarchical complexity (middle of Figure 1), than would a child at the one-word stage be capable of producing a two-word sentence. This impression of constraint comes from the fact that each child in Greenfield et al.'s (1972) nesting cup study was shown the most hierarchically complex strategy (the subassembly in Figure 1) as a model to

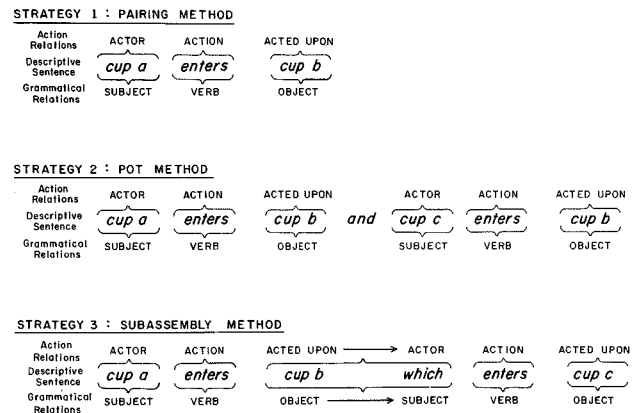


Figure 3. Structural analogy between object combination strategies and sentence types. Each sentence both describes a corresponding object combination strategy and parallels it in structure. In ontogenesis, the three sentence types develop in the same order as the three object combination strategies (Greenfield et al. 1972).

imitate. The actual strategies used in response to this uniform model, however, showed varying degrees of simplification that were inversely related to age, with no 11-month-old child ever achieving the subassembly strategy over eight trials with the cups.

2. Analogy or homology?

In evolutionary theory an analogy is based on a structural or functional parallel without any common origins, whereas a homology involves not only parallel structure but parallel origins in the phylogenetic history of the species. In developmental psychology homology refers to common structural origins in the ontogeny of individual members of the species (Bates 1979). Whereas phylogenetic homology is defined as descent from a common antecedent structure within an ancestral species, ontogenetic homology can be defined as descent from a common antecedent structure within the same organism.

There is a close relationship between the two usages, because the phylogeny of a species is a history of ontogenies. With respect to the parallels between language and object combination, analogy would be much weaker than homology. Analogy, implying distinct cognitive modules, is quite compatible with modularity; homology, implying a single underlying cognitive module for language and manual object combination, is not.

Greenfield and colleagues were limited in their experimental methods to demonstrating analogies between the development of linguistic grammar and of manual object combination. They speculated, however, that these analogies might be based on an underlying homology. Greenfield et al. (1972) wrote, "The importance of the action-grammar analogy lies in the possibility that the same human capacities may be responsible for both types of structure" (p. 305). Nevertheless, the question remained open.

Behaviors are considered homologous only if they are regulated by the same (neuro)anatomical structures (Hodos 1976; Lenneberg 1967; Steklis 1988), so the way

to decide between analogy and homology is to determine whether the neural basis of hierarchically organized computational structure is specific to language or is also used to support hierarchical organization in another area of development, manual object combination.

2.1. Neural evidence in favor of homology

Grossman (1980) used the double dissociation technique with patients who had brain damage in known locations to provide evidence of a common neural substrate for hierarchical organization in grammar and manual construction activity. He gave the tree structure task developed by Greenfield and Schneider (1977) to adults with different sorts of cerebral injury. One group was composed of agrammatic patients with Broca's aphasia; this group was central to Grossman's argument for a central processor for hierarchically structured material, including language. The other groups were fluent aphasics, nonaphasics with injuries lateralized to the right hemisphere, alcoholic Korsakoff patients, and normal controls.

2.1.1. A theoretical approach to Broca's area. Because Broca's area is central to the rest of the argument in this article, it is important to be specific about it. It is located in the ventral region of the left frontal lobe of the cerebral cortex, but there has always been disagreement about what its exact boundaries are. One reason is that there is a larger, more complex region involved in Broca's aphasia than the discoverer of the area realized in the nineteenth century (Deacon 1990a). A second, even more important reason is that the functions of this area are carried out, not by a single localized brain region acting in isolation (Deacon 1990a), but by various circuits, extending beyond the region itself. A major goal of this article is to provide evidence for a theory of different functions, subareas, and connections within the left ventral frontal region of the cortex without trying to identify a particular subarea as *the* Broca's area.

2.1.2. Evidence from adult aphasics. Broca's aphasia is associated with lesions in Broca's area, often involving portions of the adjacent facial motor cortex (Geschwind 1971) and prefrontal cortex (Deacon 1989). A major subgroup of Broca's aphasics is unable to produce syntactically organized speech, a major component of agrammatism.

Agrammatic Broca's aphasics lack hierarchical organization in their syntactic production. Here is an example of agrammatic speech from Goodglass and Geschwind (1976, p. 408): "And, er Wednesday . . . nine o'clock. And er Thursday, ten o'clock . . . doctors. Two doctors . . . and ah . . . teeth. Yeah . . . fine." Structurally, this speech is mainly a string of one-word utterances. There is no utterance with a syntactic tree structure even as hierarchically complex as that shown in Figure 2c. Grossman predicted that such patients would also have trouble in constructing nonlinguistic tree structures. He suggested that the parallels between language and hierarchically organized construction activity identified by Greenfield and colleagues were not mere analogy, but had a common basis in the brain itself. His hypothesis was that Broca's area functioned as a supramodal hier-

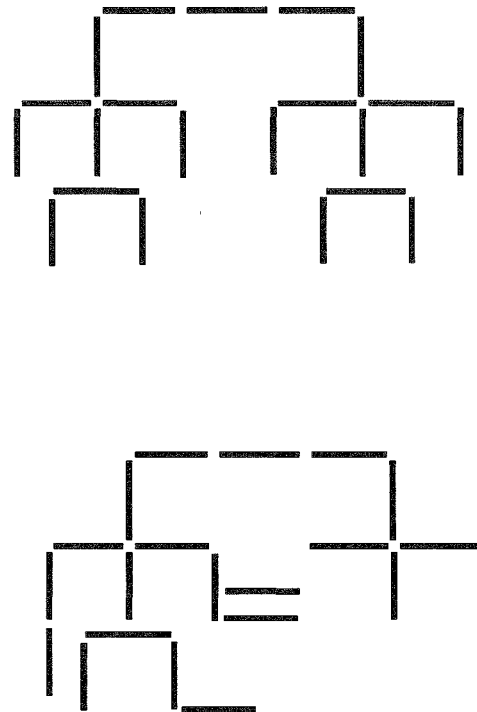


Figure 4. Hierarchical tree structures used as models in Grossman's (1980) study. The upper tree is symmetrical, whereas the lower tree is asymmetrical.

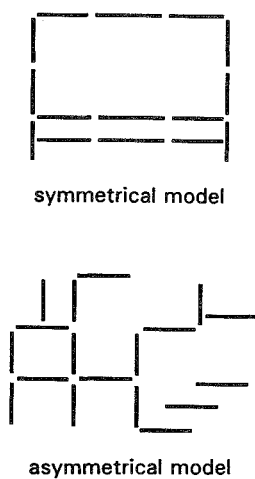
archical processor organizing grammar and manual object combination.

To test this hypothesis, each subject in Grossman's study was given two hierarchically organized tree structures to copy using tongue depressors. The one on the top of Figure 4 had been developed by Greenfield and Schneider (1977) for a developmental study of children aged 3 to 11. The one on the bottom was developed by Grossman to add the structural feature of asymmetry.

The results supported the hypothesis of a supramodal hierarchical processor. In reconstructing the model tree structures from memory (where a mental representation would be required), the Broca's aphasics did not have a general problem in construction but a specific deficit in representing the hierarchical organization of the models. (This deficit did not show up when the model was present.) A construction was counted as replicating the hierarchical structure of the model if it "exhibited two or more sub-complexes vertically subordinate to a unifying structure" (Grossman 1980, p. 301). Of all the pathological groups, the Broca's aphasics were the most successful (and closest to the normals) in matching the number of sticks used in the models. They were the *least* successful (and farthest from the normals), however, in recreating the model's hierarchical structure under a memory condition, where the model was taken away. Figure 5 shows two examples of nonhierarchical constructions created from memory by two Broca's aphasics, as well as the contrasting hierarchically organized constructions produced by fluent or Wernicke's aphasics.

The hierarchical constructions of the fluent aphasics with lesions in the left posterior area of the brain further supported the conclusion that the left frontal region of the

BROCA'S APHASICS



FLUENT APHASICS

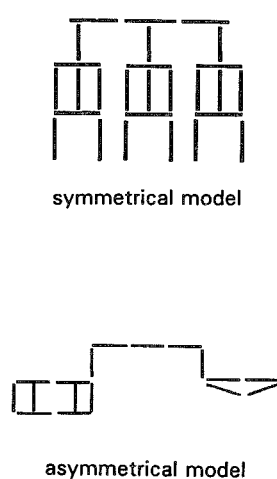


Figure 5. Reproduction of symmetric and asymmetric models by Broca's and fluent aphasics (Grossman 1980).

brain, in which Broca's area is found, processes hierarchical structure in both the grammar of language and the combination of objects. Fluent aphasics produce speech that is semantically empty but has hierarchically organized (if not always correct) syntax. Here is an example from Goodglass and Geschwind (1976, p. 410): "The things I want to say . . . ah . . . the way I say things, but I understand mostly things, most of them and what the things are." Interestingly enough, fluent aphasics also did very well at reproducing the hierarchical structure of the models, although their tree structures, like their sentences, were not always correct (compare Figure 4 and the right side of Figure 5).

In summary, data from the fluent aphasics with their intact left frontal area of the cortex further supported the relationship between this region and hierarchical organization in both language and construction activity.

Further evidence along the same lines was provided by an examination of the strategies used to construct the symmetrical model. Greenfield and Schneider (1977) had looked at the degree to which the "surface structure" of the construction process (the serial order in which pieces were added) reflected a mental representation of the tree structure. The youngest children (age six) who successfully copied the model used a nonhierarchical, chain-like

strategy in which an element would be placed adjacent to the one added just before it (see left side of Figure 6). Seven- and nine-year-olds followed the hierarchical organization of the model in their "surface structure" strategy, proceeding from superordinate (top) to subordinate (bottom) components (middle of Figure 6). Finally, many of the 11-year-olds used a top-down method in which they just skipped from one branch to another in building the structure (right side of Figure 6). This strategy was considered to indicate internalization of the hierarchical organization of the model.

Grossman (1980), using a similar measure of shifting from one part of the structure to another, found that the Broca's aphasics were most chain-like in their placement strategy. Thus, if the foregoing analysis is correct, Broca's aphasics gave the least evidence of having a mental representation of the overall hierarchical structure. The fluent aphasics, in contrast, used the hierarchical strategy *more* than the normal control group.

Note that in the foregoing Broca's speech sample the only grammatical relation to be expressed is conjunction (*and*). Conjunction is basically syntactic chaining. As such, it is an analogue to the chaining strategy used by Broca's aphasics to construct a physical tree structure in Grossman's experiment. (Although there is insufficient space to discuss the current controversies concerning the underlying nature of Broca's aphasia or agrammatism [see Bates and Thal 1989], our analysis might ultimately shed light on this theoretical problem.)

In summary, the pattern of group differences indicates a *specific deficit* in hierarchical organization associated with lesions in a specific region of the brain: Broca's area in the left hemisphere. Neural specificity is further supported by the fact that this performance was not only associated with Broca's aphasia; it was also *absent* in any other group, pathological or normal. Hence we have a double dissociation. [See multiple book review of Shallice: *From Neuropsychology to Mental Structure*, *BBS* 14(3) 1991.]

2.1.3. Evidence from direct study of the ventral region of the left frontal lobe through positron emission tomography. Direct observation of normal brain function through positron emission tomography (PET scan) has yielded new evidence concerning the functions of the ventral region of the left frontal lobe, what the researchers call Broca's area (Fox et al. 1988). The area functions in conjunction with the relevant area of motor cortex: the

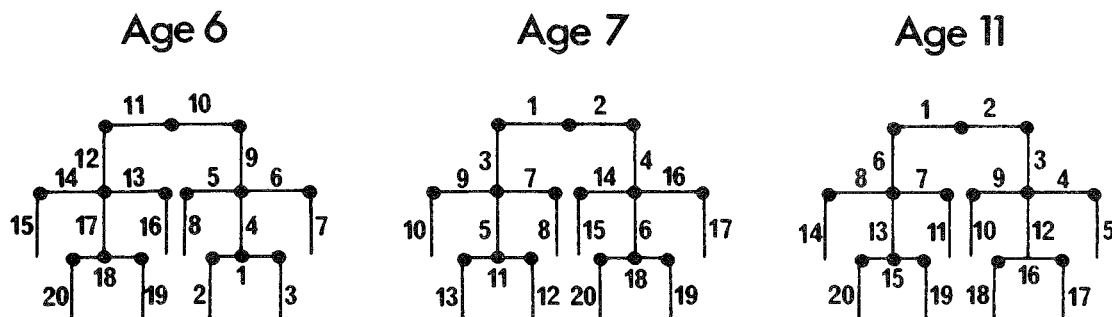


Figure 6. Typical construction strategies at different ages. Numerals indicate the serial order in which the pieces were added (Greenfield & Schneider 1977).

mouth and tongue area for imitated speech, the hand area for hand movements. Thus it is part of a number of different cortical circuits or networks involving various parts of the motor cortex. The area can also decouple from the motor cortex when movement is mentally represented but not carried out. When subjects were asked to imagine a hand movement, Broca's area "lit up" in the PET scan, but the hand area of the left motor cortex did not. These findings provide strong and direct evidence that the general region in which Broca's area is located has a directive or programming function for simple responses in a variety of modalities. Other research assessing regional cerebral blood flow during various tasks has established that Broca's region is implicated in (1) grammatical descriptive speech and (2) motor sequencing (Roland 1985).

A number of investigators have noted more generally that the left hemisphere controls sequential manual as well as linguistic production (Calvin 1990; Kimura 1979; Lieberman 1990; Steklis & Harnad 1976). Their work provides context for the more specific findings.

2.1.4. Evidence from childhood aphasia. Cromer (1983) tested a group of children with "acquired aphasia with convulsive disorder" on hierarchically organized drawing and construction tasks, based on Greenfield & Schneider's (1977) mobile (also used by Grossman [1980] and shown at the top of Figure 4). These children lacked all language and, in addition, appeared to lack such hierarchical organizing skills in other domains as the perception of rhythms. Although these aphasic children could draw and construct the modeled tree structure by using a chain-like serial method, they could not do so when required to use hierarchical planning to build up the model in terms of its subunits. Their scores reflecting the hierarchical organization of serial acts were significantly lower than those of age-matched profoundly deaf and normal children. In this study, the correlation between language and action is more global than in Grossman's (1980) study because the aphasic children (ranging in age from nine to 16 lack *all* aspects of language, not merely hierarchically organized grammar. Nevertheless, the study is of interest here because it provides converging evidence for a generalized hierarchical processor at an earlier point in development.

2.2. Neuropsychological evidence against homology

Curtiss, Yamada, and Fromkin (Curtiss & Yamada 1981; Curtiss et al. 1979; Yamada 1981) also used neuropsychological cases and several of Greenfield's grammar-of-action tasks to explore the relation between grammatical structure and action structure. Their subjects were eight mentally retarded individuals aged 6½ to 20. They found that certain members of their sample were skilled at the hierarchically organized construction tasks but weak in grammatical structure, whereas others had hierarchically complex grammatical structures but were limited to extremely simple constructions. This pattern of results indicates a *dissociation* between the neural substrate for the hierarchical organization of grammatical structure and the hierarchical organization of object-combination activity.

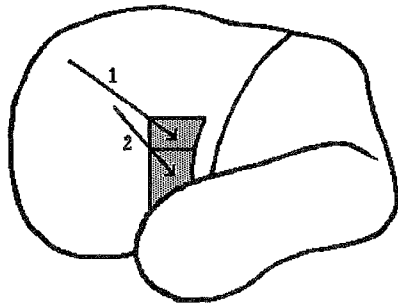
On the one hand, the results of Cromer (1983), Fox et

al. (1988), Grossman (1980), and Roland (1985) suggest there is a unified supramodal hierarchical processor and hence a homologous relationship between hierarchical organization in language and manual construction. On the other hand, Curtiss, Yamada, and Fromkin's results indicate separate neural modules for hierarchical organization in each domain; their results reduce parallel hierarchical development in language and manual object combination to mere analogy. How can these conflicting results be integrated and reconciled?

2.3. Using neural circuitry and its development to resolve the conflict between analogy and homology

The first clue to a resolution lies in the fact that Broca's area must be connected to more anterior areas of the prefrontal region of the brain, areas that specialize in programming and planning of all kinds (Fuster 1985; Luria 1966; Stuss & Benson 1986). Hierarchical organization is intrinsic to planning because, at its most basic level, a plan subordinates component elements to a superordinate goal (Bruner & Bruner 1968; Miller et al. 1960). This planned quality is also central to object combination activity and to complex sentential structure (Ochs Keenan 1977). Indeed, Petrides and Milner (1982) have demonstrated that patients with left frontal lobe excisions but intact Broca's areas are very much impaired, relative to a variety of control groups, on the strategic or planning aspect of a sequential manual task.

In fact, as mentioned earlier, many Broca's aphasics have also suffered damage to the adjacent prefrontal area (Deacon 1989). It is in this circumstance that agrammatism appears (Lieberman 1988; 1990). It may be that some Broca's aphasics show disruption in the hierarchical organization of both grammar and manual object combination activity because of damage to two different circuits emanating from the region of Broca's area (Brodmann's areas 44 and 45). The circuit for the hierarchical organization of manual sequences would include the anterior superior prefrontal cortex (Brodmann's area 9; Roland 1985). A second circuit for the hierarchical organization of grammar would include an area of the prefrontal cortex just superior and anterior to Broca's area (Ojemann 1983; Stuss & Benson 1986), probably Brodmann's area 46. (Roland's findings are based on the measurement of regional cerebral blood flow; Ojemann's are based on electrical stimulation mapping; Stuss & Benson's are based on clinical brain lesion data.) The participation in two different circuits involving the anterior prefrontal region would result in the differentiation of Broca's area itself. The hypothesized circuits are shown in Figure 7. The arrows indicate the direction of control. The lower circuit would be associated with the syndrome called Broca's aphasia. One group of Broca's aphasics has difficulty in speech production; these presumably have damage to the circuit linking Broca's area to the facial motor cortex (see right side of circuit 2, bottom of Figure 11). If only the circuit linking Broca's area to the facial motor cortex were damaged, the motor aspects of speech production (articulation), including the motor aspects of word combination, would be impaired, but there would be no real agrammatism. If the prefrontal part of the circuit were damaged, however, agrammatism would result. It might therefore be more accurate to reserve the



- 1 = manual object combination circuit
 2 = grammar circuit
 ● = region of Broca's area

Figure 7. Hypothesized neural circuits for the hierarchical organization of complex object combination and complex grammar. This is a schematic representation based on a synthesis of data from Fox et al. (1988), Ojemann (1983a; 1983b), Roland (1985), Simons and Scheibel (1989), and Thatcher (unpublished data, 1991). The anterior pole of the grammar circuit (Brodmann's area 46) is based on Ojemann (1983a; 1983b). The anterior pole of the manual object combination circuit (Brodmann's area 9) is based on Roland (1985). The role of Broca's area is based on Fox et al. (1988), Roland (1985), and Simons and Scheibel (1989).

term Broca's aphasia for the articulatory deficits arising from damage to the circuit controlled by the classical Broca's area, reserving the term agrammatism for deficits involving the anterior part of the cortical grammar circuit depicted in Figure 7, controlled by a region anterior to Broca's area itself.

The conclusion that two separate circuits are involved leads to a resolution of the conflict. Because of the topographical proximity of these two circuits, most often a lesion to the left frontal area would destroy both; occasionally, however, one of the circuits would be spared. In the former case, there would be an association between the hierarchical organization of language and manual object combination; in the latter case there would be a dissociation. Curtiss et al.'s (1979) subjects with diffuse neural damage might then happen to have had one intact circuit without the other, leading to the observed dissociations between syntactic structure and manual action structure.

Although Fox et al. (1988) call this region Broca's area and treat it as unitary, our theory would posit that it has, by adulthood, differentiated into two functionally distinct though neighboring areas, one controlling speech, the other manual action, as described earlier. Because of the imperfect resolution of the PET scan, these two subareas were probably visualized as one single region in Fox et al.'s (1988) research.

Some evidence for this hypothesis of two separable circuits comes directly from Grossman's (1980) data. Although the memory constructions of the Broca's aphasics were hierarchically organized (i.e., they exhibited two or more subcomplexes vertically subordinate to a unifying structure) significantly less often than with normal controls or fluent aphasics, an inspection of the original protocols, kindly supplied by Grossman, indicated that a minority of the agrammatic subjects was able to construct hierarchical arrangements in the tree structure tasks. The implication is that the agrammatic pa-

tients with damage to both the grammatical and the manual programming circuits (the majority) failed to represent their tree constructions hierarchically, whereas patients with damage to the grammatical circuit alone (the minority) were able to represent and build hierarchically organized constructions.

Until now the picture is one of separate circuits connecting partially contiguous brain regions so that it is possible, although not likely, to damage one circuit without the other. Specific circuits directly connecting various cortical areas, particularly distant ones, however, are not present from birth; such circuits or networks are the product of gradual postnatal differentiation (Thatcher et al. 1987). New neural connections are added in a diffuse manner through early infancy. After that, processes of "pruning" of synapses (Huttenlocher 1979) in combination with selective dendritic and axonal growth (Kolb & Whishaw 1985) lead to more specific and differentiated neural circuits between spatially separated cortical areas (Thatcher et al. 1987). Hence, Broca's area might start out, early in development, as an undifferentiated neural region, programming both manual action and language production. At this point, one would expect the functioning of this cross-modal area to be quite diffuse and immature as well. As Broca's region developed differentiated circuits or networks involving more anterior portions of the left prefrontal cortex, the structure of manual action and of language would become more divergent, autonomous and complex.

Although this position might seem at first to conflict with the rostral (frontal pole) to caudal (precentral gyrus) trend for increasing modality segregation in the frontal lobes (Deacon 1990c), the conflict is more apparent than real. It is necessary to distinguish between a neural *area* and a neural *circuit*. It is the growth of connections to the rostral area of prefrontal cortex that creates specific circuits for the complex structures in manual action and language. This does not imply that the rostral prefrontal areas in question are modality specific. Indeed, I would predict that the rostral area around Brodmann's area 46 would not only participate in the circuit required for the production of syntax, but also in the circuit for syntactic comprehension. Consequently it would, in hearing speakers, have a common abstract function across two modalities, vocalization and audition. In addition, I predict that the same rostral prefrontal area would participate in the production of sign language syntax in deaf people, thus demonstrating its use for an abstract function not specific to a particular sensory modality.

It is the presence of multiple short range connections in all directions, hypothesized to exist early in development, that makes the caudal inferior frontal area (roughly Brodmann's area 44 and 45, also known as Broca's) *cross-modal*. This is the undifferentiated state referred to above. The area is not really *amodal* or *supramodal* in the same sense as the more rostral (anterior) areas are: It does not have an abstract, modality-free function. Its cross-modal connections are in fact quite specific: They are limited to such neighboring areas as the orofacial motor area.

This could explain the shortcoming of predicting from a caudal-rostral (i.e., back to front) developmental trend in frontal cortex that relatively complex motor skills should mature before the learning of even simple skills that

require supramodal integration of many modalities. Such a view, for example, leaves unexplained the very early development of intentional thumb-sucking, a cross-modal integration of hand and mouth. (Note that the term modality has been extended from sensory modalities to the output modalities of relevance here.) The cross-modal connections of an undifferentiated Broca's area could explain this early development.

Most important, at maturity, there would be separate subregions of Broca's area for language and manual action. Hence, the state of the mature left frontal lobe of the cortex would conform to the rostral-caudal (i.e., front to back) trend for increasing modality segregation: At maturity Broca's area could be less modality specific than motor cortex, more modality specific than prefrontal cortex.

2.4. Theoretical predictions

A number of predictions flow from the theoretical model developed so far:

1. The hierarchical organization of language and manual object combination, including tool use, should be closely linked and interdependent early in development, the two domains becoming more autonomous as brain differentiation proceeds.

2. The ontogeny of left frontal lobe circuits should furnish the cortical basis for the hierarchical organization of speech and manual object combination, including tool use.

3. The schedule of cortical differentiation should correlate with the relative interdependence and autonomy of the two domains in behavior.

The first prediction is the focus of section 3, the second and third of section 4.

3. The ontogeny of relations between language, object combination, and tool use

3.1. The organization of language and manual object combination are more closely linked when language development begins

A retrospective look at the grammar of action studies carried out by Greenfield and colleagues indicated that the analogy between grammar and object combination was much stronger in the nesting cup study (see Figure 1) done with the youngest children (aged 11 to 36 months) than it was in the subsequent studies carried out with children aged three and up. Whereas in the first study (Greenfield et al. 1972) it was possible to describe precise parallels between the structure of word combination and object combination (see Figure 3), this was impossible for the more complex structures modeled for the older children (e.g., the tree structure at the top of Figure 4, used by Greenfield & Schneider 1977).

Other clues in this direction come from the close connections and parallels between language and action up to age two. For example, the child's one-word and two-word utterances, spanning the age period one to two, are placed in a sensorimotor framework supplied by the child's own perceptions, actions, and gestures (Bloom 1973; Brown 1973; Greenfield & Smith 1976). Lock (1990) points out that at 13 months, the child has a parallel

repertoire of vocal and manual gestures, to the point of equipotentiality for either spoken or sign language, depending on circumstances; Volterra (1987) has provided the empirical evidence for such a conclusion. Bates (1988) notes that meaningful relations between language and other modes of action last until approximately two years of age. With each subsequent stage of development, however, the child's linguistic productions become increasingly autonomous from sensorimotor activity (e.g., Greenfield et al. 1985; Karmiloff-Smith 1979).

If the early development of hierarchical structure in both language and manual object combination is being organized by the same undifferentiated brain region, then one would predict not only parallel sequences of structural development in the two domains, but also synchrony in developmental timing. The parallel structures depicted in Figure 3 are definitely not synchronous, however. For example, two objects (e.g., nesting cups) can be combined long before two words (Figure 2b) can be combined into a primitive sentence. The temporal gap until sentences of the complexity shown in Figure 3 can be produced is much longer still.

3.2. Sound combination and object combination develop synchronously in a structurally parallel sequence

Lieberman (e.g., 1984) emphasizes Broca's area as the seat of phonological as well as grammatical programming. He has therefore suggested (personal communication, 1988) that developmental parallels to grammars of action should be sought in *phonological rules for combining sounds*, not merely in grammatical rules for combining words. As the following analysis shows, this strategy has begun to yield very rich results.

3.2.1. A note about methodology. It should probably be mentioned at the outset that the main source for the parallels to be described lies in diary data from three children. Whereas there are many excellent studies of phonological development in various languages, none includes parallel observations of object combination. The diary observations of phonological development have nonetheless been supplemented by the findings of Smith (1973) and Macken (1979). Many other comprehensive studies of phonological development (e.g., Ferguson & Farwell 1975) have taken an analytic perspective so different that they do not provide the kind of information about the differentiation of word structure that is relevant here.

3.2.2. The evidence. The earliest meaningful words begin toward the end of the first year when children begin to combine two objects intentionally. Most interesting, the phonological and syllabic structure of these first words bears a formal resemblance to the initial way in which objects are combined. The earliest words are reduplicated consonant-vowel syllables such as *dada* or *mama* (e.g., Greenfield 1972; Greenfield, unpublished data). In data from one child, Lauren (Greenfield 1972), this occurred at 8 months, 3 weeks. In these sound combinations, a single consonant is combined repeatedly with the same vowel. Children's first intentional combinations of objects occurring around the same time have a parallel structure: One object is repeatedly touched to a second one (Piaget 1952).²

Diary data from three children – Lauren, Matthew, and Nicky (Matthew and Nicky's data are from the study described in Greenfield & Smith 1976) – indicate a second stage of word formation, following a few months later, in which a single consonant can be combined with a single vowel to form a word (e.g., Nicky's *na* for "no" at 12 months, 1 week of age; Lauren's *ma* for "milk" at 12 months, three weeks of age). This state also has a parallel stage of object combination occurring at roughly the same age: One object is combined with another, as when one cup is placed in or on a second, as in the nesting cup study (Greenfield et al. 1972). (The pairing strategy is shown at the left side in Figure 1.)

A third stage of word formation is characterized by a process called consonant harmonization (Smith 1973; Macken 1979). In consonant harmonization, the first sound, a single consonant, remains constant as it is successively combined with two different vowels. The earliest examples from my data are Nicky's *daddy* at 16 months, 2 weeks; Lauren's *baby* around 13 months; and Matthew's *cackuh* (cracker) at 12 months, 3 weeks.

On the level of object combination, there is also a parallel strategy in which the first object to be picked up remains constant as it is successively combined with two other objects in turn. In the nesting cup study, this strategy occurred when a baby would place the first cup in or on a second one and would then remove it without ever letting go of the cup, placing it in or on a third cup (Greenfield et al. 1972). Lauren was observed using this strategy for combining objects at 12 months, 1 day, when she successively placed a red circle in the red and blue holes of a form board. (Note that the timing is about one month before the first observation of her comparable word form; this timing seems quite close, considering that no systematic diary had been planned for object manipulation.)

The next development in word formation also involves harmonization, this time of the vowel. In this structure, the initial consonant varies, whereas the second sound (a vowel) with which it combines remains constant. The earliest examples in the Greenfield data are as follows: from Lauren, *tinky* (stinky) (the *n* is considered part of the vowel sound) at 15 months, 1 week; from Matthew, *kya bye* (car bye-bye) at 15 months. (Note, in Matthew's example, that the combination of two words seems to follow as a consequence of more complex syllable structure in the formation of a single word; this may provide a key to the mechanism that provides the transition from single-word utterances to combinatorial speech.) This important point will be elaborated in section 3.4.

The parallel object combination strategy was called the "pot" strategy in the Greenfield et al. (1972) nesting cup study (Figure 1); in that strategy, the initial moving cup varies while the "pot" with which each cup combines remains constant. In the nesting cup experiment, this strategy became dominant at 16 months of age. In addition to appearing at the same place in the developmental sequence as the corresponding stage of word formation, the age of appearance is within a month of the age for the corresponding word formation strategy.

The next stage of word formation involves combining already developed syllabic subassemblies into higher-order units. This can involve adding a consonant-vowel

combination to a second consonant to form a phonologically more complex word (e.g., *ball*, from Matthew at 15 months, 3 weeks) and/or making a two-word sentence out of two previously constructed sound combinations (e.g., *bye-bye tat* [cat] from Lauren at 15 months, 2 weeks).

In parallel fashion, the final stage of object combination identified in the nesting cup study was also termed the subassembly strategy. In that strategy, at least one previously constructed subassembly of cups functioned as a unit, combining with another cup or subassembly of cups (see right side of Figure 1). This strategy first appeared at age 20 months in the study by Greenfield et al. (1972).

In summary, from about 9 to 20 months of age, children pass through parallel and quite synchronous stages of hierarchical complexity in forming spoken words and combining objects. Although the quantitative evidence is preliminary, the qualitative parallels in sequencing and timing between the two domains are striking. In addition, it is clear that developments in word formation and object combination are taking place in a single chronological period that ends around two years of age.

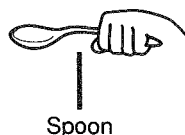
Hence, preliminary evidence indicates that the first requirement for establishing a developmental homology – synchronous and parallel developmental sequences – can be satisfied. Although it could be argued that with such young children it is easy to find simultaneous development in several domains, it is not easy to find identical structural substages; nor is it easy to find a close correspondence in the timing of the substages (cf. Fischer & Hogan 1989). Most crucial, such sequences are not the final criterion for homology. What is being argued is that they make it worthwhile to look in this age range for evidence of the development of a common neural substrate, a topic considered in section 4.

3.3. The ontogeny of tool use

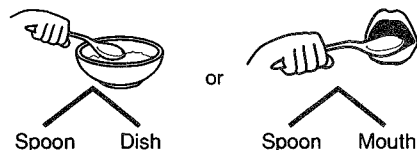
In this section, the earliest development of tool use in human infants is shown to be a special case of the development of object combination programs already described. In trying to establish the earliest tool use as a special case of the earliest stages in the development of grammars of action, I rely on a recently published study by Connolly and Dalgleish (1989) on the ontogenesis of the use of a spoon, the Euro-American infant's first tool. Basic tool use can be thought of as just that type of object combination in which a single acting object serves as an instrument to act on a second object, thereby achieving a specified goal (cf. strategy 1, the pairing method, in Figure 1). Although Connolly and Dalgleish (1989) did not look at the development of tool use through the lens of grammars of action, their observations have such beautiful detail that it was possible to reconstruct stages of hierarchically organized object combination.

Just as in the earliest stage of object combination already described, some of the youngest babies (11–12 months) in Connolly and Dalgleish's study were observed to put a spoon repeatedly in and out of the dish. Other children of this age simply put their spoons in and out of their mouths; Gesell and Ilg (1937) had observed this behavior and placed it at 10 to 12 months of age. In both cases, the strategy consists of taking one object (the

TOOL AS OBJECT



PAIRING



SUBASSEMBLY

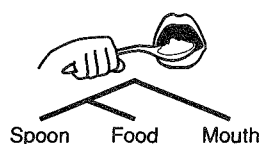


Figure 8. Developmental stages in the hierarchical organization of spoon use, based on data from Connolly and Dalgleish (1989). The first stage (top) is not referred to in the text because it is precombinatorial. The top drawing simply depicts an earlier stage at which the child is limited to grasping the spoon, like any other object.

spoon), which has the acting role, and repeatedly combining it with a second object that is acted on. As in the pairing stage of the nesting cup strategies, the child is restricted to combining two objects at a time: either spoon and dish or spoon and mouth, but not all three. (See pairing stage of spoon use, Figure 8). Thus tool use reveals the same combinatorial rule described above as

the first stage in the structural development of object combinations, with its parallel in the first stage of sound combination (see first panel of Figure 9).

The next stage of spoon use is also structurally parallel to a later way in which infants combine two objects at a time: The infant first touches the spoon (Object 1) to the food (Object 2) and then to the mouth (Object 3) (but no food arrives at the mouth). This is a variant of the pairing strategy observed in the nesting cup study in which the infant places an acting cup in or on a second cup, then removes it (without letting go) to place it in or on a third cup. This strategy parallels the strategy of word formation in which the same initial consonant successively combines with two different vowels (e.g., the *baby* example presented earlier).

The last stage of spoon use (bottom of Figure 8) is parallel to the subassembly strategy, the final nesting cup strategy (see right side of Figure 1). In this stage, the infant combines spoon with food, initially through a side-to-side scoop, and then moves the subassembly of spoon plus food to the mouth, the final object. Once again, there is a parallel stage of sound combination (the right side of Figure 9).

Although no analogue to the intermediate "pot" strategy was reported by Connolly and Dalgleish (1989), I would predict that there was one, based on theoretical considerations as well as some suggestive observations in the Connolly and Dalgleish article. I would predict an intermediate stage like the following: Baby brings spoon directly to mouth with one hand while bringing food to mouth with the other. The mouth very literally would serve as a "pot" for two objects, food and spoon. This is structurally analogous to the "pot" method shown in the middle of Figure 1. (See middle of Figure 9 for the parallel between this structure and a corresponding structure of word formation.)

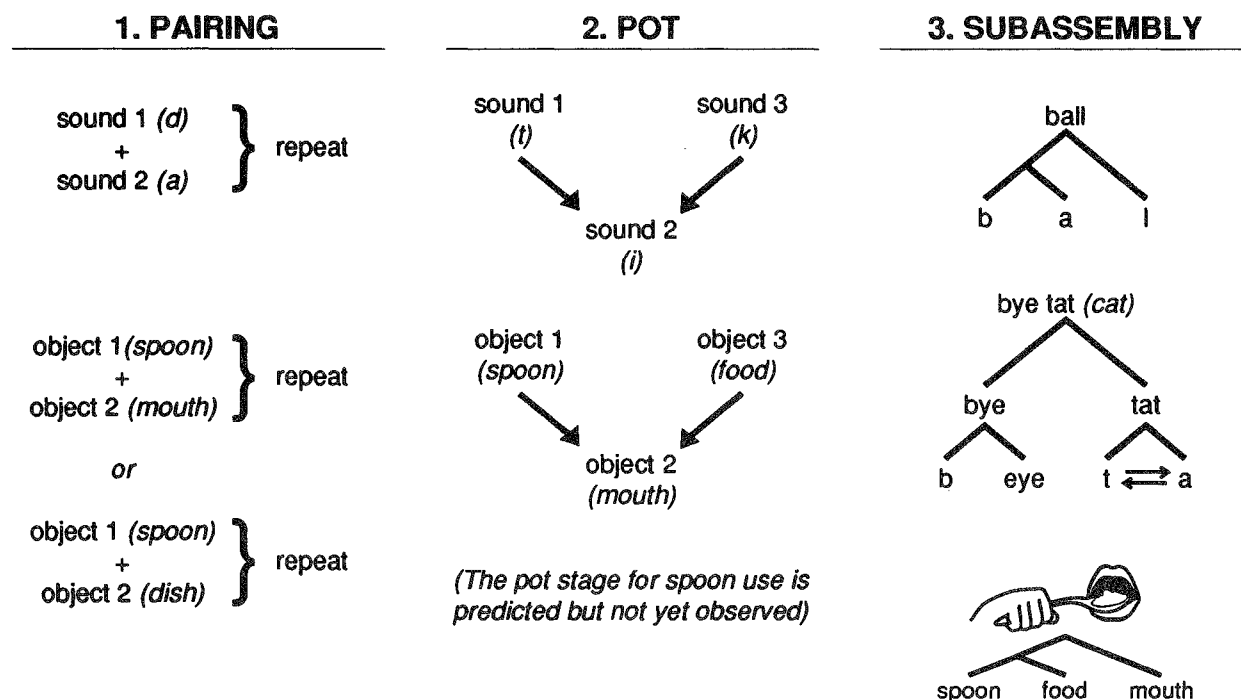


Figure 9. Parallels in the development of sound combination and object combination: The case of spoon use.

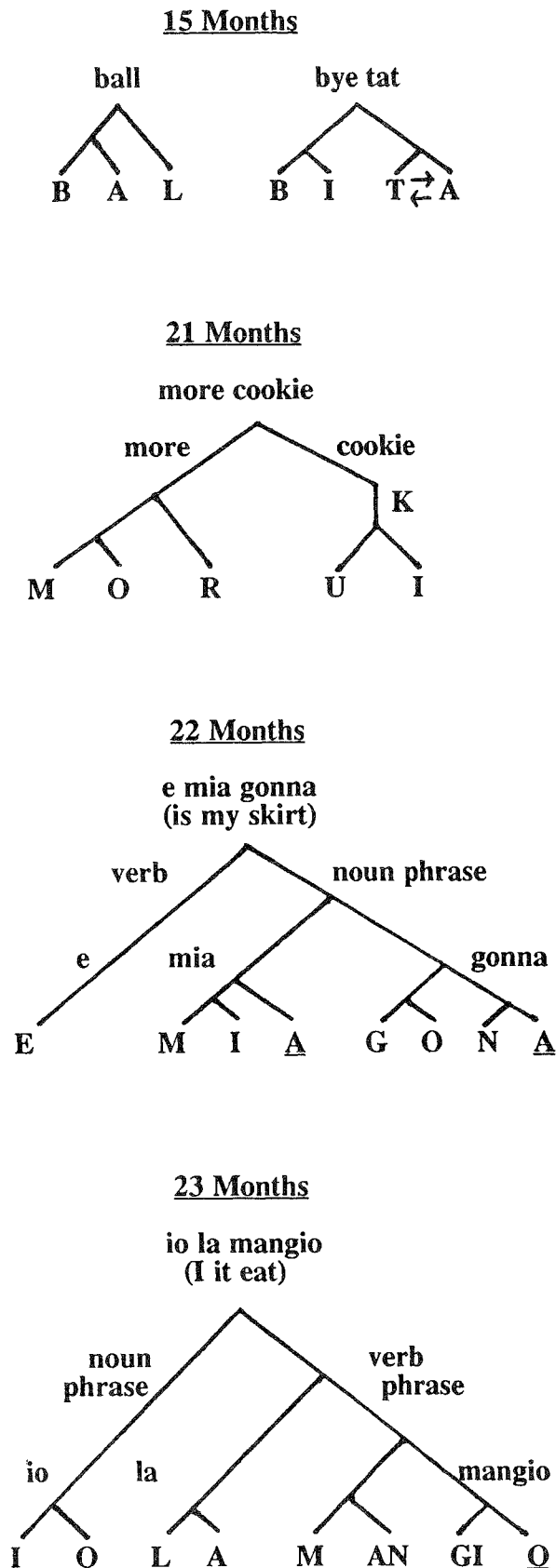


Figure 10. Hierarchical relations between word formation and sentence formation in development. (Underlined letters indicate possessive-noun agreement in "mia gonna," subject-verb agreement in "io la mangio.")

In terms of developmental timing, the sequence of structural stages of tool use covers the period from 12 to 23 months of age studied by Connolly and Dalgleish (1989), approximately the same period in which the nesting cup strategies and analogous word formation strategies were observed. The qualitative stages of spoon use observed so far indicate that the development of tool use is an instance of the hierarchical development of object combination strategies, more generally conceived.

3.4. The relation between sound combination and word combination

The developmental sequence described earlier leads to the following hypothesis: The ability to combine two words under a single intonational contour – that is, making a sentence – is an outgrowth of the ability to combine sounds into increasingly differentiated syllables. (It is the existence of one intonational contour, rather than two, that separates a two-word utterance from two successive one-word utterances.) Thus, Matthew's *kye bye* occurs at the same age as Lauren's *tinky*; the phonological construction of Matthew's two-word utterance is the same as that of Lauren's single word: (consonant 1 + vowel 1) + (consonant 2 + vowel 1). Similarly, a few weeks later, Matthew's single word *ball* has the same number of hierarchical levels as Lauren's two-word utterance *bye-bye tat* (see Figure 10).

This hypothesis fits with the notion that Broca's area is the seat of both phonological and grammatical programming. If the development of phonological combinations and early word combinations is part of a single unified process, it makes sense to put it under the programming control of a single neural area, hypothesized to be the region in which the classical Broca's area lies.

It follows from this hypothesis that the total hierarchical complexity of an utterance involves a synthesis of the phonological and morphological levels of combination, as Figure 10 shows.³

3.5. The differentiation of hierarchical organization in language and object combination

Developmental information about grammars of action and language suggests that programs for combining objects become increasingly differentiated from programs for combining words (linguistic grammars) starting around two years of age. After 20 months of age, the hierarchical organization of language continues to increase. Consider the utterance *more cookie* (Figure 10), for example. It is still a two-word combination, like *kye bye* or *bye tat*, but it has another level of hierarchical complexity: There are now three rather than two levels of branching nodes. In addition, there is now evidence of the beginnings of syntactic organization – word order: In the corpus at this time, the overwhelming majority of two-word combinations observes English word order.

The next example in Figure 10, *e mia gonna*, illustrates the subsequent stage of hierarchical complexity as well as morphological marking. *E mia gonna* ([It] is my skirt) was produced by an Italian child of 22 months (Hyams 1986, p. 138). As Hyams notes, the richer inflections of Italian relative to English make it possible to illustrate this phenomenon at this early age. Using the same method for

noting the hierarchical organization of words and their combination, Figure 10 shows that hierarchical structure has increased: There are now four levels of branching. The child's syntactic marking of hierarchical organization is indicated by gender agreement between the possessive *mia* and the noun *gonna* (denoted in the last syllable). This is a way of noting that *mia* and *gonna* compose a noun phrase "subassembly."

One can also look at the hierarchical organization in terms of the syntactic categories and relations implied by such sentences. Hyams (1986) points out that to mark subject-verb agreement requires the categories of subject and verb. Such categorical organization is manifest by the 23-month-old Italian child who said *Io la mangio* (I it eat – Hyams 1986, p. 143). In this sentence, diagrammed at the bottom of Figure 10, the child has discriminated between the subject pronoun *Io*, which requires agreement, and the object pronoun *la*, which does not. (Agreement is indicated by the *-io* suffix in *mangio*.) This level of hierarchical organization is indicated in Figure 10 by the labels "verb phrase" and "noun phrase." As a comparison of the number of levels and number of branches in the diagrams for *e mia gonna* and *io la mangio* in Figure 10 indicates, the two utterances are conceived as having the same degree of hierarchical complexity. That this complexity of syntactic organization is typical of children in this age group has been found by a number of investigators in recent years (e.g., Bloom 1990; Levy 1983; Valian 1986).

3.6. Discussion

Thus far, the behavioral evidence is very much in accord with the hypothesis that, during the earliest stages of language acquisition, there is a single neural substrate for the hierarchical organization of language and manual object combination, a substrate that subsequently undergoes a process of developmental differentiation.

Although the evidence is sufficient to suggest the theory, one must remember that it was not designed to test the theory. At this point, it would be desirable to design a study expressly for this purpose. A more rigorous test would be to look at both object combination, including tool use, and word formation with age held constant or factored out. This way, one could use variability in developmental rates to test for the ontogenetic yoking of the two skills (Bates 1988). If a positive correlation between performance in the two domains were obtained, the ontogenetic relationship would be more likely to be based on a homology rather than merely an analogy arising from two sets of skills independently developing over roughly the same age span.

In addition, because the claims that are being made imply universality, it would be desirable in future studies to collect evidence from a variety of language groups. Finally, one could also look for the predicted "pot" stage of spoon use that is missing from the report of Connolly and Dalgleish (1989).

Behavioral evidence alone, however, no matter how good, is not sufficient for attributing equivalent hierarchical status and common ontogenetic origins (developmental homology) to structures in disparate domains. This calls for evidence from neural development, to which we next turn.

4. The development of a neural substrate for combinatorial organization: Language and objects

New kinds of neuroanatomical and neurophysiological data enable us to trace the development of brain connections that provide the foundation for structural development in both language and action. I draw on Robert Thatcher's large cross-sectional data set on the development of EEG coherence (indexing neural connectivity) between pairs of locations on the cerebral cortex (Dr. Thatcher has kindly carried out special analyses for use in the present paper; his techniques are described in Thatcher et al. 1987). From the neuroanatomical perspective, I draw on a recently published study of the postnatal development of the motor speech area by Simonds and Scheibel (1989). Whereas Thatcher's EEG data span 19 neural locations over the two hemispheres (see Homan, 1988, for precise placements), Simonds and Scheibel have analyzed brain tissue from four locations, Broca's area (Brodmann areas 44 and 45) and the orofacial area, on the left hemisphere, along with analogous locations on the right hemisphere.

The focus of Simonds and Scheibel (1989) is on the analysis of dendritic branching. Because the dendrite is the cell's input mechanism, dendritic branching provides various measures of a neuron's receptive connectivity with more distant regions of the brain. As long-distance connections develop, connections with neighboring cells and areas decrease. This is part of the process of "pruning" neural connections as development proceeds (Huttenlocher 1979). Hence, the neurophysiological and neuroanatomical data converge in providing information about developing neural networks.

The two kinds of data have complementary patterns of strengths and weaknesses. EEG data are not so localized as neuroanatomical data. They provide direct information on particular neural circuits or networks, however. The neuroanatomical data are precisely localized but provide no information on exactly where the other termini of the circuits or networks are located. The direction of information flow cannot be ascertained from EEG coherence data, but the neuroanatomical study of dendrites isolates input connections to the regions of interest. The two sources of data are complementary in another way as well: Simonds and Scheibel's data stem from a relatively deep layer of the cortex (Layer 5) and therefore reflect relatively greater subcortical and local connections than Thatcher's EEG coherence data, which are based on electrical activity at the surface of the cortex.

We shall first examine neuroanatomical evidence up to 15 months of age from Simonds and Scheibel (1989), with data points at 3 months, 5–6 months, and 12–15 months of age. Using the earlier two developmental periods as a baseline, we see that dendritic branching is greater in the right hemisphere than in the left through six months of age. This emphasis on right hemisphere development occurs *before* the development of combinatorial activity in either speech or object manipulation (which were hypothesized, on the basis of the neuropsychological evidence described earlier, to be left hemisphere functions).

By 12–15 months, the beginning stages of both word formation and object combination (including tool use),

the neuroanatomical picture changes. The orofacial motor zone on the *left* side, used for speech movements, has developed enough dendritic growth to catch up with its counterpart on the right side. At this point in development, dendritic growth in the left orofacial area has developed significantly more than in the contiguous Broca's area. (It is important to note that the term dendritic growth, here and elsewhere in this article, refers to length and branching complexity of dendrites, not to their quantity.)

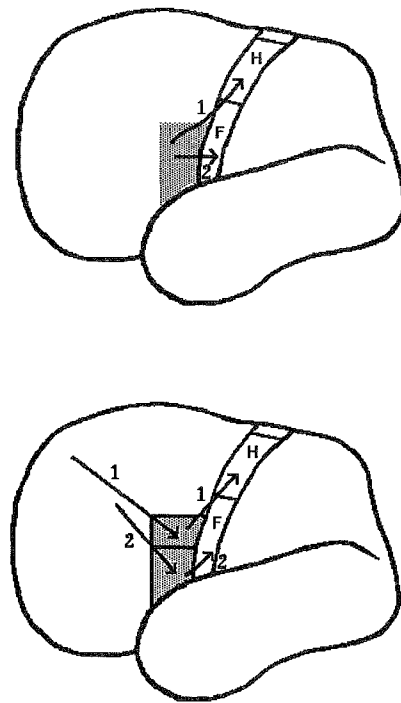
On theoretical grounds, it is hypothesized that the developing input structures in the left orofacial motor area receive input from the neighboring Broca's region, which could provide the motor program for phonological production. At this point then, input connections from Broca's area to the orofacial motor cortex should be relatively rich. The hypothesized connection is shown as Circuit 2 at the top of Figure 11. The empirical reality of Broca's area as the output source of the hypothesized connections can now be empirically tested. Most important for the present argument, if empirically confirmed, this state of affairs would provide neural support for the processes of word formation taking place during this period.

Extrapolating from Simonds and Scheibel's (1989) data on the orofacial area, I predicted a growth spurt of connectivity between the left manual motor cortex and Broca's area at around the same age. Inspection of Thatcher's cross-sectional data set indicates that this circuit has significant connectivity in this age range, reaching a modest first peak of coherence around 16 months of age. This hypothesized connection is shown as Circuit 1 at the top of Figure 11.

Before the development of the circuits connecting the left orofacial and manual motor areas of the cortex to the more anterior region where the classical Broca's area lies, it is hypothesized that vocal and manipulative functions would be poorly differentiated in the infant brain because of a large number of short-range connections between the neighboring orofacial and manual areas. This lack of differentiation in the brain would then be reflected in a lack of behavioral differentiation, including conjoint non-dissociable movements of hands and mouth (Ploog 1988). With the development of more specific connections (e.g., through dendritic growth, hypothesized to link up motor cortex with Broca's area), the diffuse connections within the motor cortex would be eliminated in a "pruning" process.

At the next neuroanatomical data point, 24–36 months of age, dendritic growth in Broca's area has caught up with and exceeded dendritic growth of the left orofacial motor area (Simonds & Scheibel 1989). What we know from these findings is that Broca's area is now receiving more distant inputs from some area of the brain; they do not tell us where. Thatcher's analysis of electrophysiological connectivity in the cortex provides important clues, however.

At this point, I predicted a spurt starting at age two in the neural connectivity between the left anterior prefrontal area and the more posterior region in which Broca's area is located. To test my prediction, Thatcher analyzed his cross-sectional data and found such a spurt of increased connectivity between approximately two and four years of age. During this period the corresponding



- 1 = manual object combination circuit
- 2 = grammar circuit
- = region of Broca's area
- H = manual motor area
- F = orofacial motor area

Figure 11. Hypothesized development of neural circuits for the production of hierarchically organized manual sequences and grammar. In the top drawing, the absence of borders and divisions for Broca's area, as well as the position of the arrows, represents its undifferentiated character at this early stage of development. The circuits in the top drawing are hypothesized to undergo development in the approximate age range of 12 to 16 months. The left-hand portions of the circuits in the bottom drawing are hypothesized to undergo development in the approximate age range of two to four years. These are schematic representations based on a synthesis and extrapolation of data from Fox et al. (1988), Ojemann (1983a; 1983b), Roland (1985), Simonds and Scheibel (1989), and Thatcher (unpublished data, 1991). The anterior pole of the grammar circuit is based on Ojemann (1983a; 1983b). The anterior pole of the manual object combination circuit is based on Roland (1985). The role of Broca's area is based on Fox et al. (1988), Roland (1985), and Simonds and Scheibel (1989). The posterior pole of the circuits (in motor cortex) is based on Geschwind (1979), Roland (1985), Simonds and Scheibel (1989), and Thatcher (unpublished data, 1991).

right hemisphere circuit showed no growth in connectivity at all. The fact that this finding was a prediction from the theory, rather than an *ex post facto* explanation of known data, strengthens the validity of the proposed theory of neural circuit development.

Putting these two pieces together, we can then hypothesize that, functionally and developmentally, Broca's area is starting at age two to receive input from the anterior prefrontal area. Given the incomplete nature of the evidence, this key proposition has the status of a theoretical prediction, ripe for direct empirical test.

The putative circuits are exactly those hypothesized to be operating in agrammatism: Production of complex grammatical speech would involve input from the left intermediate prefrontal cortex (perhaps Brodmann's area 46) (Ojemann 1983a; 1983b) to the inferior part of the left posterior inferior frontal area (probably Brodmann's areas 44 and 45, also known as Broca's area); organization of hierarchically complex programs of object combination would involve input from the anterior superior prefrontal cortex (perhaps Brodmann's area 9) (Roland 1985) to the superior part of the left posterior inferior frontal area. The hypothesized circuits are shown at the bottom of Figure 11. They are identical to those shown in Figure 7; for the sake of exposition, the neural links from Broca's area to motor cortex were omitted from Figure 7.

Because linguistic grammar and action sequences are analyzed in separate areas of the anterior prefrontal cortex (Ojemann 1983a; 1983b; Roland 1985), the hypothesized growth of long-distance connections between Broca's area and the more anterior prefrontal region should provide the neural basis for a differentiation of manual object combination programs from linguistic grammar programs. Given the anterior prefrontal area's function in planning complex, hierarchically organized sequences, the connections between the anterior prefrontal region and Broca's area also bespeak the beginning of much more complex structures.

It is known that cortical differentiation in the frontal lobes proceeds from posterior (or caudal) to anterior (or rostral) (Deacon 1990b). Hence, the hypothesized two-stage development of circuits shown in Figure 11 is in line with this known pattern of development.

The qualitative nature and timing of this development fits perfectly with the behavioral evidence: It is in this period between two and four years of age in which morphologically complex grammar emerges in language (e.g., Brown 1973; Hyams 1986; Valian 1986), generating structures that have no analogue in grammars of action. On the one hand, there is around two years of age an increase in the hierarchical complexity of linguistic productions on the syntactic level, as shown in Figure 10. On the other hand, there is also at this point the introduction of syntactic marking, a qualitatively new development. The increase in hierarchical complexity, with its syntactic marking, is hypothesized to stem from the addition of the anterior prefrontal area to the language production circuit (left part of Circuit 2, bottom of Figure 11) between approximately two and four years of age. (There is no implication here that age four is the end of either syntactic or neural development. It is simply the temporary end-point dictated by current limitations in our knowledge of neural development.)

Also emerging for the first time in this period are complex grammars of object combination that have no analogues in linguistic grammar (e.g., Beagles-Roos & Greenfield 1979; Goodson & Greenfield 1975; Greenfield 1976; 1977; 1978; Greenfield & Hubner, n.d.; Greenfield & Schneider 1977; Reifel & Greenfield 1981). The tree structure at the top of Figure 4 is one such example.⁴

Similarly, it is hypothesized that the expansion of the hierarchical complexity possible in object combination activity stems from the addition of the superior anterior prefrontal area to the object combination circuit (left part of Circuit 1, bottom of Figure 11). The involvement of this

cortical area in the development of manual skills with sequential steps with objects has been demonstrated by A. Diamond (1991), who explored the sensorimotor consequences of earlier maturational stages of the same cortical areas in monkeys and human children.

The conclusion is that neural differentiation of higher order programs for language and object combination occurs in just that period when behavioral differentiation is taking place. According to this view, the syntax of language and the hierarchical organization of object combination are homologous in their "embryological" origins, but they are modular in the neuroanatomy of their mature functioning.⁵

4.1. The nature of the neural model

In neurology, the two major positions have been locationism and equipotentiality. The neural explanation of language has also been subject to these two opposing viewpoints. Yet neither has proven satisfactory (Kolb & Whishaw 1985). Nonetheless, because of the nature of the available data and methods, most neuropsychologists have tended to try to correlate specific linguistic (or other psychological) functions with specific areas of the brain. In recent years, neural network models have become popular. This, in essence, is a new form of equipotentiality, with a strong emphasis on learning.

The style of neural theorizing here, however, is neither locationist nor equipotentialist. It proposes to think instead in terms of *neural circuits* and their *development*. This is an extension of Geschwind's (1972) approach to the neurology of language and owes much to Deacon's (1989; in press) research and theory concerning the anatomy of neural circuits in monkeys. Whereas focusing on brain areas implies a one-to-one correlation between location and function, the circuit approach does not. One would predict that interrupting a given circuit at any point would interrupt the function, thus explaining the failure of strict locationist approaches. This prediction is quite in line with the results of Ojemann's (1983) brain stimulation studies. At a very basic level, the emphasis on circuits also agrees with what is known about the importance of neural connectivity and the transmission of impulses from one neuron to another in brain function.

It has so far been shown that thinking in terms of neural circuits and their development resolves a number of seeming contradictions in the field of aphasiology, such as (1) the existence of articulatory disorders both with and without agrammatism in Broca's aphasia and (2) agrammatism both with and without deficits in building hierarchical object constructions.

In child language development, the importance of understanding the role of the neural differentiation process in which multiple short-range connections are "pruned" to fewer, more specific, and longer-range connections cannot be overestimated. This is the process by which differentiated circuits are created. It is this developmental model that allows us to understand why early speech is so closely intertwined with other sorts of action, whereas later grammar is both more independent from action and more abstract. Such new techniques as brain imaging and computerized EEG have allowed us to begin to create models of developing circuits on an empirical basis.

4.2. The role of the environment

The description of the growth of neural circuitry in no way lessens the importance of interaction with the environment. Although maturational patterns are epigenetic in nature (i.e., canalized to follow certain paths), both neural and linguistic development require active experience to be actualized (e.g., Curtiss 1977; M. Diamond 1988) Fischer et al. (1990), for example, have incorporated both brain development and environmental interaction into a theory of cognitive development.

5. Phylogenetic implications and evidence

As a way of approaching the issues of evolutionary origins and phylogenetic homology, one can ask (1) what is the behavioral evidence concerning the structural development of object combination and symbol combination in primates? and (2) what is the neural evidence? The first question is addressed in sections 5.2, 5.3, and 5.4, the second in section 5.5.

5.1. The logic of an evolutionary argument

Because there is no fossil record of behavior, evolutionary reconstruction in behavioral domains is always a matter of inferring the most plausible scenarios based largely on contemporaneous evidence. The logic of a comparative approach to the evolution of behavior is as follows: If we find common capacities in two related descendent species of a common ancestor, it is possible that both species inherited the capacity in some form from the common ancestral species. If the same behavioral capacity is found in not just two, but *all* the species stemming from a common ancestor, the basis for the behavioral trait in the common ancestor becomes quite certain (Parker 1990). Within this framework, the comparison of language and tool use in chimpanzees, sibling species to *Homo sapiens* with whom we share 99% of our genes (King & Wilson 1975), takes on particular importance in the evolutionary investigation of the language-tool homology.

5.2. Structural development in primate object combination: Tool use and tool construction

There are clear examples of tool use in a number of species of primates that structurally parallel Stage 1 in the grammar of action (pairing strategy, left side of Figure 1): One object (the tool) acts on another (see Visalberghi 1990). McGrew (1990) observes, however, that only chimpanzees can use the same tool on different objects, a variant of pairwise combinations also noted above for children.

In addition, McGrew (1990) notes that chimpanzees are unique in having tool sets in which two tools are used sequentially on a single object (top of Figure 12). The use of a tool set is structurally analogous to children's "pot" strategy, the next stage in the development of grammars of action (middle of Figure 1). Using a stone to strike a nut placed on an anvil (Sugiyama & Koman 1979) is another example of the "pot" strategy in wild chimpanzee tool use: Two active, moving objects (nut and stone) are combined in succession with a single passive object (anvil).

Gardner and Gardner (1988) have shown that captive chimpanzees can use the "pot" strategy in a number of different construction and tool use tasks. For example, in threading beads, the chimpanzee adds a series of objects to one constant object (the string), serving as the common object.

Goodall's (1986) observations of chimpanzee object manipulation seem to confirm that chimpanzee grammars of action are limited in the wild to the "pot" strategy and do not reach the level of subassemblies, the final stage depicted in Figure 1.

But McGrew (personal communication, 1990) reports a kind of borderline subassembly in the wild. When chimpanzees "fish" for ants, they move a stick to attract them; when the ants attack the stick, the chimpanzee treats ants plus stick as a subassembly, moving it to the mouth as object. The leaf sponges used for drinking (Goodall 1986) may also be examples of the same degree of hierarchical complexity.

Indeed, in captivity, both species of chimpanzee, *Pan paniscus* and *Pan troglodytes*, have learned to use a spoon (Gardner & Gardner 1988; Savage-Rumbaugh, personal communication, 1990), thus showing the capacity for hierarchical organization at the level of a simple subassembly (bottom of Figure 8). In addition, Visalberghi (personal communication, 1990) reports that the ontogeny of nutcracking in *Cebus* monkeys is similar to the development of spoon use in human children.

That level may be the nonhuman primate limit, however. For example, both Gibson (1990) and McGrew (1990) have concluded that apes do not use "additive construction" in tool manufacture; in grammar-of-action terms, they do not combine two objects into a tool subassembly that can then act on a third object outside the chimpanzee's own body. Indeed, McGrew concludes from his comparison of chimpanzee and Tasmanian human tool construction that additive construction is a major feature distinguishing human tool construction from that of chimpanzees.

A change in object roles is a correlate of the subassembly strategy, as shown on the bottom of Figure 3: The recipient of the action in the first object combination becomes the acting element in the second. If we apply role change to the toolmaking context, an object that is the recipient of action in the *construction* of a tool changes into the acting element when that tool acts on another object during the tool *use* phase. Lacking subassemblies intrinsic to additive tool construction, chimpanzees would also lack the ability to change the role of the same object from active to passive or vice versa, a competence that, on the syntactic level, is pertinent to relative clause construction (see bottom of Figure 3).

As we would predict from our analysis of grammar of action, humans are also unique in using tools to make tools (McGrew 1990). This would involve still another level of hierarchical complexity and role change: Two or more objects would be combined to make the first tool, which would act on one or more objects (creating the second tool), which would in turn act on still another object; we now have a three-level tree structure with multiple role changes.

The captivity experiments of Koehler concerning chimpanzee tool use and tool construction (1925) confirm this analysis of abilities and limitations inherent in chim-

panzee object combination. Applying the schema for assessing hierarchical complexity to Koehler's evidence, we find that the most hierarchically complex manual construction is a rather borderline example of subassembly: One of Koehler's chimpanzees (named Tschego) bent a bundle of straw in half to construct a stronger stick, then combined this stick with an object to bring it closer to her. More frequent examples of relatively complex object combinations in Koehler's data are limited to structural analogs of the "pot" strategy: Chimpanzees pile boxes in a stack (to climb on them). Matsuzawa (1986b) reports stacking of blocks by a four-year-old captive chimpanzee. As Reynolds (1983) points out, this type of stack cannot be rotated in space as a unit. It therefore lacks the essential quality of a subassembly.

5.3. Parallel structures in chimpanzee symbol combination

If language and tool use evolved together phylogenetically and develop together ontogenetically, then the symbolic combinations of chimpanzees exposed to a human symbol system should be limited to the hierarchical complexity of a simple subassembly on the language level, as it is on the level of tool use.⁶

If chimpanzee symbolic capacity is homologous with the early symbolic skills of young children, one would predict the same structural sequence. Pairwise symbolic combinations should accordingly precede conjoined symbolic structures analogous to the "pot" strategy in grammars of action, which should, in turn, precede subassembly constructions.

Our data come from a program of research on bonobo chimpanzees (*Pan paniscus*) directed by Sue Savage-Rumbaugh (see Savage-Rumbaugh et al., 1990, for a recent summary). Although, as both Pinker and Bloom (1990) and Piatelli-Palmerini (1989) point out, the evolution of human language does not *logically require* evidence from ape language for its hypothetical reconstruction, this is one important avenue of research, one that can provide empirical clues about the capacities of a common ancestor.

Although the structural development of sound combinations was considered a homologue of manual object combination in the human case, symbol combination is considered in the chimpanzee case because the chimpanzees use a system of visual symbols (geometric lexigrams plus a few gestures) that are unitary entities in themselves. Hence, there is no level of word formation; the only possible level of combination is between individual symbols.

In a study of symbol combination in a bonobo named Kanzi, Greenfield and Savage-Rumbaugh (1990; 1991) found ordering rules for two-symbol combinations, as well as the beginnings of an ordering rule for a three-element combination. Before symbol combinations, Kanzi, like children, also had a stage of single symbol utterances. It is of theoretical interest to note also that Kanzi's symbolic combinations were extremely closely linked with the sensorimotor activity in which he was involved. He usually used his combinations to communicate about actions that he was planning or objects he wanted.

Ordering rules for two-element combinations were clearly established at the time his first three-element rule

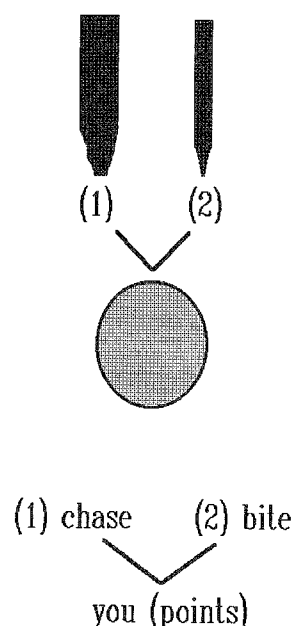


Figure 12. Parallel structure in a chimpanzee "tool set" and a chimpanzee symbol combination. Kanzi would touch or point to geometric lexigram symbols to communicate "chase" or "bite."

appeared, indicating the developmental precedence of a pairing strategy, as one would predict from the preceding theoretical considerations and data. As Figure 2 shows, a three-word combination can have a two-level, branching subassembly structure. Hence it is particularly noteworthy that Kanzi's three-element rule did *not* have such a structure; it had a conjoined structure (analogous to the "pot" structure in grammars of action) instead of the more complex branching structure. His rule-governed three-symbol utterances consisted of two ordered actions and one agent (e.g., CHASE HIDE you (gesture) (see Greenfield & Savage-Rumbaugh, 1991, for a complete corpus). From a structural point of view, two actions combine in a specified sequence with a single agent; this is structurally analogous to placing two tools in a particular sequence to act on a single object (see Figure 12). The combination of consistent symbol order, along with other criteria elaborated by Greenfield and Savage-Rumbaugh (1990; 1991), led to the conclusion that Kanzi had mastered and (in the case of two rules) invented a protosyntax.

At the next level of hierarchical complexity, Kanzi occasionally produced symbol combinations that were at the simple subassembly level shown in Figure 2c for child language. For example, he produced the lexigram utterance BALLOON WATER HIDE the day after he and his caregivers had been hiding balloons filled with Koolaid. Here WATER modifies BALLOON, forming a subassembly that serves as the object of HIDE. The syntactic organization looks similar to that of *want more grapejuice*, shown at the bottom of Figure 2. Hence, the chimpanzee's most advanced combination on the symbolic level matches the structural stage of the chimpanzee's most advanced combination on the tool level.

The rate of development in the two species is very different: Children require approximately one year to go from first word to telegraphic speech; Kanzi had been producing lexigrams for three and a half years when these data were collected. A different rate of development –

heterochrony – is common in phylogenetically related species (Gould 1977), however.

5.4. Discussion and summary

Ontogenetic parallels between action and language in people extend to phylogenetically related species. Indeed, Gibson (1988; 1990) has also hypothesized that human tool use and language differ from that of the apes primarily in degree of hierarchical organization. Using a slightly different terminology, Gibson (1983; 1988) and Reynolds (1983) likewise developed a nearly identical hypothesis. There are no developmental data for chimpanzee tool use. It would be useful to collect such data in the future to test whether the three strategies – pairing, “pot,” and subassembly – occur in the same developmental order in chimpanzees. In the absence of developmental data on tool use, however, the existence of (1) object combination strategies that parallel developmentally sequenced symbol combination strategies in chimpanzees and (2) parallel constraints on hierarchical complexity of chimpanzee activity in the two domains is theoretically relevant evidence.

Thus far, the behavioral evidence from primates is consistent with the idea that the capacities for tools and language evolved together. Is there evidence for a homologous neural substrate? This must be the ultimate criterion.

5.5. Homologous prefrontal circuits in macaque monkeys

The ideal neural evidence would be from chimpanzees but it is available only from macaque monkeys and other more distantly related primate species. The evolutionary logic remains the same, however: To the extent that we find common circuits in macaques and humans, these are likely to have existed in our common ancestor, who, much more ancient, would also be a common ancestor to the chimpanzees; the common neural circuits would therefore be likely to be homologous.

Broca's area homologues have been found in the brains of macaque monkeys (Deacon 1989; *in press*).⁷ Indeed, a homologue of the human grammatical circuit shown at the bottom of Figure 11 has been identified by Deacon (1990a), using axonal tracer methods. Like the human brain depicted at the bottom of Figure 11, the macaque brain also shows a more dorsally located circuit for manual action in the frontal lobe (Brooks 1986; Martino & Strick 1987), including prefrontal inputs (Muakkassa & Strick 1979). There is also evidence that, similar to the child around one year of age, the Broca's area homologue of the macaque lacks differentiation in that it activates both hand and mouth movement (Rizzolatti 1987).

These homologues occur in the context of a species that has shown distinct left hemispheric dominance for processing species-specific vocalizations (Falk 1990). MacNeilage (1988) has indirect evidence of left hemisphere dominance in nonhuman primates because of their right-handed preference in making precise manual gestures. [See also MacNeilage et al.: “Primate Handedness Reconsidered” *BBS* 10(2) 1987.]

5.6. Speculations about neural evolution from nonhuman primates to humans

Because there has been so much expansion of the prefrontal cortex in the course of evolution from nonhuman primates to humans (Deacon 1990a), one would expect neural structures in apes to be able to support very simple grammars of manual construction and manual gesture (and even simpler levels of sound construction), whereas complex structures would call for the much greater connectivity in the human brain described by Gibson (1990), particularly in the prefrontal areas.

One result of a larger brain with more connectivity is greater separation and differentiation of function (Deacon, personal communication, 1991). The Broca's area homologue in the macaque resembles the one inferred to exist in the very young child, in that it activates both hand and mouth. The evolution of a larger brain with more connectivity may well have brought with it the separation of manual and oral control theorized for Broca's area in adult humans (bottom of Figure 11).

5.7. Ontogeny and phylogeny

One reason to consider developmental evidence in an evolutionary reconstruction lies in von Baer's law that in phylogenetically related species early stages of ontogenetic development are generally more similar than later ones. This principle implies “terminal addition”: Evolutionary change focuses on later stages of the maturational process. There is a tendency in this direction because of the conservative nature of evolution: It builds on what is already there. The tendency is far from absolute, however (Studdert-Kennedy 1991). In addition, reasons other than homologous evolutionary origins are possible for the cross-species resemblance of immature stages (Deacon 1990a).

One major problem in relying on von Baer's law in the present case is that the relevant nonhuman primate data come primarily from mature animals whereas the relevant human data come primarily from early development. The argument therefore runs the risk of veering into crude recapitulationism: the idea that stages in child development recapitulate mature stages in our evolutionary ancestors.

It is accordingly important to point out a more basic reason for using developmental evidence in an evolutionary reconstruction. Homologous origins of capacities across species imply homologous ontogenetic histories. Indeed, common embryology is often taken to be the criterion for cross-species homology. Language and manual capacities involve the development of a brain and behavioral capacities that are still immature at birth – they are figuratively, if not literally, embryonic (cf. Lamendella 1976).

In the present case, it is known that the back-to-front sequence of frontal lobe development described in section 4 is common to all mammals (Deacon 1990b). That the most rostral or forward prefrontal areas of the cortex are both the last to develop ontogenetically and more highly developed in humans than in other mammals, including nonhuman primates, is a fact. It is important to note that this fact and its evolutionary significance in no way depends on the validity of either von Baer's law or recapitulationism.

6. Modularity reconsidered

Chomsky (1980) spoke of the language faculty as a “mental organ,” analogous to the heart or the visceral system. Fodor (1983) systematized this view while replacing the term “organ” with the word “module.” According to Fodor (1983, p. 37), a module (1) is domain-specific, (2) has an innately specified structure, (3) is not assembled by combining more elementary subprocesses, (4) is associated with specific, localized and elaborately structured neural systems, and (5) is computationally autonomous. How does the picture drawn of the ontogeny of linguistic and object combination square with these criteria? Do skills in these two domains qualify as modules? [See also multiple book reviews of Fodor: *The Modularity of Mind*, *BBS* 8(1) 1985.]

Let us start with the early stage of cortical development described earlier (top of Figure 11). At that point in development, the organization of manual object combination and sound combination fail to conform to the modularity criteria in some critical respects: (1) Having a portion of their neural substrate (the left frontal region associated with Broca's area) in common, they lack domain specificity, and (2) sharing the resources of Broca's area, they are not computationally autonomous. On the other hand, they conform to Fodor's description of modules in other respects: (1) The two behavioral domains are associated with a specific neural system and, therefore, (2) the source of structure is innate.

After approximately two years of age, the differentiated expansion of the two neural circuits into the anterior prefrontal region (bottom of Figure 11) makes each circuit increasingly domain specific and relatively autonomous. It would seem, therefore, that, with development, a basically nonmodular but innate system has become modularized. The nature of the more mature system, however, is such that it now violates another of Fodor's (1983) criteria: The early circuits constitute subprocesses of the more mature circuits (note the relationship between the earlier and later developing circuits shown in Figure 11). If we are to claim that modularity has come into existence with the later developing circuits, we must reject Fodor's criterion concerning the absence of component subprocesses as incompatible with the nature of neural development. In essence, we must modify the definition of a module.

Fodor (1983, p. 42) views the motor production of speech as involving a module that is separate from that used to process speech comprehension. Because the relevant circuits would not be expected to be the same for the processing of linguistic input (although there could be overlapping components), we basically agree on this point. Evidence concerning the relative precocity of syntactic development in language comprehension (Hirsh-Pasek & Golinkoff, in press) has accordingly been considered neither relevant to the argument nor damaging to the chronology of structural development of speech production outlined earlier.

One possible reason for the precocity of syntactic comprehension relative to production found by Hirsh-Pasek and Golinkoff (in press) might be that the connections between the auditory comprehension area, Broca's region, and/or the anterior prefrontal syntax area mature earlier than connections between oral-facial motor cor-

tex, the speech programming center in the region of Broca's area, and the anterior prefrontal syntax area. Indeed, evidence from the tracer study of macaque brains indicates that there is a vertical division of the traditional Broca's area in which one part connects to auditory processing areas while another connects to a facial area (Deacon, in press). If this division holds in humans, connections with the two parts could well mature at different rates, leading, for example, to the development of syntactic comprehension before production. (Because no data concerning such a division yet exist in humans, it was not taken into account in drawing the brain circuits portrayed in Figures 7 and 11.)

Similarly, although agrammatism does indeed involve deficits in syntactic comprehension as well as production (e.g., Bates et al. 1987; Zurif & Caramazza 1976), it is hypothesized that these involve a distinct neural circuit with common components – possibly the left part of the grammar circuit shown at the bottom of Figure 11. Because deficits in the comprehension of syntax should, according to the theory being advanced, involve their own cortical circuitry, this aspect of agrammatism has been considered to be beyond the scope of the present article.

7. Phylogeny, ontogeny and homology reconsidered

An argument for a double homology – ontogenetic and phylogenetic – has been presented: a homologous neural substrate for the early ontogeny of the hierarchical organization shared by two domains – language and manual object combination – and a homologous neural substrate and behavioral organization shared by human and non-human primates in phylogeny.

According to evolutionary theory, a cross-species similarity in behavioral organization can arise because of homologous origins in a common ancestor. It can also arise because of convergent evolution – as a common adaptive response to a similar set of environmental conditions – based on different (analogous) underlying structures. Convergent evolution between chimpanzees and humans is unlikely because the environmental niches of the species have been increasingly differentiated since the species diverged four to six million years ago. This state of affairs increases the probability that similarities in behavioral organization in chimpanzees and humans have homologous origins.

A sure criterion for the reconstruction of phylogenetic origins, however, is anatomical structure. In language, the focus of anatomical interest, since Lenneberg (1967), has been the brain. If we can connect behavioral organization in two related species to a common anatomical structure, we can definitely establish phylogenetic homology, thus excluding the possibility of analogy and convergent evolution. Although the empirical evidence is much sketchier in primates than in humans, this is the form of the argument that has been presented.

7.1. Possible evolutionary scenarios

One possible evolutionary implication of this argument is that a common ancestor of humans and present-day primates had the left frontal lobe circuitry to support the

ontogeny of both primitive object combination and primitive language functions.

Another possible evolutionary scenario is that a common ancestor of human beings and present-day primates had the left frontal lobe circuitry to support the ontogeny of primitive object combination, but not protolanguage. At a later point in evolutionary history, perhaps after divergence of hominids and the great apes, this circuitry was recruited in the service of linguistic organization. In this scenario, neural organization of combinatorial manual activity serves as a preadaptation (or exaptation) for the combinatorial aspect of language, which subsequently develops by natural selection. This general scenario has been proposed by Reynolds (1976), Kimura (1979), and Lieberman (1990).

A third logical possibility is that a neural substrate for protolinguistic combination served as a preadaptation for manual object combination, which developed later. No theorist has espoused this view, probably because it is assumed that language is the more recent phylogenetic development. In addition, counterevidence exists: There is evidence for tool use in species that diverged from the hominid line millions of years before the separation of hominids and apes (Visalberghi 1990) and in whom no evidence of protolinguistic combination has been found.

7.2. Incompatible evolutionary scenarios

A number of evolutionary scenarios are eliminated by the argument and evidence. First, the existence of cross-species neural homology manifest in corresponding behavioral organization eliminates the evolutionary saltationism of Chomsky (1972; 1980a) and Piattelli-Palmerini (1989), as well as the discontinuity between human language and the capacities of ancestral species espoused by Lenneberg (1967).

The ontogenic and phylogenetic gradualism advocated here is ultimately incompatible with Bickerton's (1990) view of the evolution of language, although there are also important areas of agreement. [See also Bickerton: "The Language Bioprogram Hypothesis" *BBS* 7(2) 1984.] Both Bickerton and I see the early stages of ontogeny and phylogeny as evidence of a common protolanguage. The discovery of simple chimpanzee syntax (Greenfield & Savage-Rumbaugh 1990; 1991) and the development and componential nature of the neural circuitry discussed here contradict Bickerton's claim of total discontinuity between "protolanguage" and "language" (examples of what Bickerton means by "protolanguage" are pidgin dialects, the communication of chimpanzees, and the language of children under 2 years of age). The fact that the differentiated circuits developing after age 2 are built on the earlier more global circuits (see Figure 11) would imply an underlying continuity between the two stages, "protolanguage" and "language."

Continuity in neural development is phenotypically realized in continuity in the development of linguistic organization (Figure 10). Hence, the combining of two subassemblies of sounds creates the first combinations of two words, combinations that may subsequently receive syntactic marking by inflections or word order. Thus there is an interesting ontogenetic continuity between two computational aspects of language: phonological processes of word formation and primitive syntax.

7.3. Selection of the most probable evolutionary scenario

The choice between the two possible scenarios compatible with the evidence and the analysis presented here depends to a great extent on whether or not one thinks that ape language capacities stimulated in captivity have a communicative function in the wild. Although it is commonly supposed that they do not, this may well turn out to be incorrect. Plooj (1978) reports gestural combinations among wild chimpanzees that are quite similar in function to the two-element combinations studied in a captive bonobo by Greenfield and Savage-Rumbaugh (1990).⁸ Most interesting is the fact that this type of gestural communication has been observed in the wild uniquely in the context of mother-child interaction (Boesch & Boesch 1990; Plooj 1978). Furthermore, the examples of gestural communication reported by Boesch and Boesch (videotape, 1990) all occur as chimp mothers apprentice their young in tool use. These researchers find that the only behavior so far observed in wild chimpanzees that requires a long apprenticeship is the use of tools for cracking nuts. This fact suggests that the first evolutionary scenario is most compatible with the evidence. I would posit an evolutionary reconstruction in which tool use and manual protolanguage evolved together, both supported by the programming function of the left frontal region associated with Broca's area. In line with the theoretical position of Lieberman (1984; 1991) and Pinker and Bloom (1990), the evolutionary process I would posit would be natural selection.

The adaptive value of nutcracking as a subsistence technique would be expanded by protolanguage: Enhanced communication would streamline the apprenticeship period for nutcracking tools, thus increasing the survival value of tool use. As tool use became increasingly adaptive, the adaptive power of protolinguistic communication would in turn, be enhanced. In this way, language and tool use, programmed by an overlapping neural substrate, would evolve together through mutually reinforced natural selection. Through a process of language-brain coevolution, the adaptiveness of primitive language and tool use would serve to draw further brain evolution, in particular, expansion of the prefrontal cortical region (Deacon 1990a).

An advantage of this evolutionary scenario, with its reliance on natural selection, is the fact that selection can operate most directly on reproductive efficiency, the ultimate criterion of fitness. According to such a scenario, selection is, by definition, direct because a mother's successful tool pedagogy enhances the survival chances of her offspring. Although reproductive efficiency is the ultimate test of fitness and successful adaptation, evolutionary explanations rarely focus on reproduction and socialization of the next generation, the most vulnerable sites for the rapid operation of natural selection (Konner 1977). Note, finally, that this scenario relies crucially on the Boesch's (1990) new observations of explicit chimpanzee pedagogy, contradicting Premack's (1985) claim that explicit pedagogy is a characteristic unique to the human species.

The theory being advanced here, however, does not depend on the truth of a specific evolutionary scenario. To the extent that the theory is correct, it simply places

constraints on the selection of a compatible evolutionary reconstruction.

7.4. Implications for language development

7.4.1. Continuity in grammatical development. Whether grammatical development is continuous or discontinuous has sparked a lively debate in developmental psycholinguistics (e.g., Borer & Wexler 1987; Gleitman 1981; Hyams 1986; Lock 1990). The model of neural development presented here implies both continuity and discontinuity. For Borer and Wexler (1987), the notion of maturation in itself implies discontinuity; they assume that the maturation of each new grammatical component, beginning with the first one, is independent of earlier linguistic (and, by implication, neural) developments.

Maturation cannot be equated with discontinuity, however. There is continuity with the earlier period in that there is a steady increase in the number of hierarchical levels, from the earliest developments shown in Figure 2 to the later ones shown in Figure 10. The continuity with the earlier structure is hypothesized to stem from the fact that the earlier developing circuit from Broca's area to the orofacial motor cortex (Circuit 2, top of Figure 11) is a component of the later developing circuit (Circuit 2, bottom of Figure 11). The qualitatively discontinuous development of syntax is hypothesized to stem from the addition of the anterior prefrontal area to the language production circuit (left part of Circuit 2, bottom of Figure 11).

Based on an examination of brain damaged and other clinical cases, Benson and Stuss (1989) point out that each functional brain system under the executive control of prefrontal areas can also operate without this control. The absence of such control leads to action without thought, a state highly typical of the child between one and two years of age, who, according to the model being proposed, would lack anterior prefrontal control. Hence, based on both the breakdown and maturational buildup of neural circuits, this model implies both continuous and discontinuous development of language forms.

7.4.2. Providing a "reason" for grammatical development. Brown (1976), after outlining the development of grammatical structures in young children, tried to find a "motor" for their development in the absence of selective social pressures for more complex syntax. He found that more advanced grammar did not improve the child's communicative effectiveness. Moreover, parents neither rewarded good grammar nor punished syntactic errors. Although much more is currently known about the role of interaction in language development (e.g., Snow et al. 1988), the existence of sensitive periods for syntactic development (Newport 1988) indicates that environmental conditions cannot be completely effective if the organism is not at an optimal developmental period. The gradual development of a cortical neural pathway from the left anterior prefrontal area to the region of the classical Broca's area, and thence to the left orofacial motor area may provide the cortical motor of grammatical development for the age period from two to four.

It remains for future research to test this hypothesis, fill in the details (including subcortical connections), and

explore expansions of the circuit to accommodate later syntactic development. Whereas the idea that brain development drives language development goes back at least to Lenneberg (1967) and has been recently espoused by Borer and Wexler (1987), the description of a precise circuit governing grammatical speech, with specification of precise developmental stages, is new.

8. Conclusion

Evidence from neural development has been presented to show that the similarities between the ontogenetic development of combinatorial organization in language and manual object combination (including tool use) are homologous rather than analogous. More specifically, evidence points to the linked ontogeny of object combination and sound combination programs in early development, based on the neural substrate of an undifferentiated Broca's area. After about two years of age, Broca's area differentiates by creating two separate networks with more anterior parts of the prefrontal cortex. From that point, language and object combination begin to develop more autonomously, each ultimately generating its own special forms of structural complexity. Each domain has an innate basis in neural circuitry, just as much so in the early unitary stage as in the later modularized one.

If this theory is confirmed by further research, then language is not modular at birth or even at the beginning of language development; it becomes increasingly modular with age and neural differentiation. The theory begins to specify more explicitly the cortical circuitry underlying an innate⁹ grammatical module, the "elaborately structured neural system" required by Fodor's definition of a cognitive module. In the model being advanced here, however, the circuitry does develop by adding more elementary cortical subprocesses with maturation. In this respect, it fails to conform to one of Fodor's (1983) criteria of modularity. If the present account is correct, it follows that, from a developmental perspective, linguistic grammar never completely attains the status of a cognitive module, as defined by Fodor.

Evidence from present-day primates shows that a parallelism between combinatorial action structures and combinatorial symbol structures is also present. Research with monkeys indicates that this parallelism could also be developmentally homologous, based on a relatively undifferentiated Broca-like region. It is hypothesized that, in comparison with humans, the development of hierarchical organization in primate behavior involves less complexity and less differentiation between the domain of action and the domain of language, because of the more limited connectivity in primate brains.

More specifically, the lesser development of a cortical circuit for syntax linking the region containing Broca's area in the left prefrontal cortex with a more anterior area (see bottom of Figure 11) in macaque monkeys may be a major language-relevant difference between humans and nonhuman primates. As the expansion and differentiation of the prefrontal area progressed during hominid evolution, the syntax of language would have developed the hierarchical complexity characteristic of human language, with its embedded relative clauses, and so on. During the same process of prefrontal expansion, a resulting increase in the hierarchical complexity of manual

object combination would have been a critical factor in the emergence of the tool use, tool construction, and general constructional skills required for modern human technology. It is the linking of the behavioral commonalities between species to a homologous neural substrate that removes this scenario from the realm of recapitulationist fantasy and makes it an evolutionary hypothesis worthy of further investigation.

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"Language, tools and brain" is dedicated to the memory of Richard Cromer, classmate and pioneer in the study of language-cognition relations, their development, and their neural foundations.

NOTES

1. A more complete presentation of Fodor's modularity concept will be undertaken in section 6.

2. Unintentional combinations intentionally repeated and transitory combinations occur earlier in both object manipulation (Langer 1980; Piaget 1952) and linguistic babbling, but these are not relevant to present purposes.

3. For this reason, in section 5.3 unitary visual symbols in ape language will be considered to be structurally equivalent to a single phoneme.

4. Using manual problem-solving tasks, Bullock (1990) reports that the ability to represent the goal in a superordinate position relative to the means develops between age two and three. This growth in hierarchical complexity seems likely to relate to the increase in hierarchical complexity occurring in manual object combination in this same period of development.

5. Also involved in the developing language circuits of the frontal lobe of the left hemisphere are subcortical connections (Janowsky & Nass 1987; Lieberman 1990). These are not discussed further here because they are not known, not because they are unimportant.

6. This prediction assumes that *capacities* that lead to symbol learning and use in captivity are present in the wild, although they would not have been actualized in the same way. See later section for a discussion of how communicative capacities may be actualized in the wild.

7. As in our discussion of the human data, we emphasize

functional circuits involving the left ventral frontal region of the cortex, without trying to define one subarea as *the* Broca's homologue.

8. Boehm (1988) reports meaningful combinations of calls in wild chimpanzees observed at Jane Goodall's field site. They are not emphasized in this account, however, because the meaning relations of chimpanzee call combinations appear much farther from human language than the gesture combinations. The fact that bonobo chimpanzees can comprehend human speech (Savage-Rumbaugh et al. 1990), however, may make call comprehension and its associated neural circuitry most relevant to the evolution of language comprehension.

9. Innate has the dictionary meaning of "inborn." "Inborn" does not literally have to mean phenotypically present at birth, however. It can also mean genotypically present at birth; that is, a genetic program is present at birth that guides later development, in this case, cortical development.

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Making the best use of primate tool use?

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Constructing her arguments for a common neural substrate underlying the hierarchical nature of early language and object manipulation strategies, Greenfield shows commendable ingenuity in selecting, sorting, and tying together diverse strands of evidence. Given that the integrative exercise takes in data from many fields, including developmental psycholinguistics and psychology, neuropsychology, neurobiology, comparative psychology, and private behavioral ecology, it would be surprising if some aspects of this particular "combinatorial activity" were not less well-organized than others. My focus is on the author's use of the behavioral evidence in nonhuman primates, to point out what I see to be some problematic interpretations and some overlooked but potentially relevant information.

One source of dissatisfaction with the target article is the way the array of complex behaviors collectively referred to as tool use is treated merely as a certain type of object combination. (Indeed, tool use is never defined in the target article.) There appears to be at least two important differences, however, between primates' use of tools and children's early object manipulation as exemplified by the nesting cups paradigm. First, tool use is highly goal-oriented. Second, and more important in the present context, there is usually only one way to achieve the desired result with a given tool. For example, a chimpanzee using a termite-fishing tool can *only* insert it into the termite mound to obtain the prey. In other words, the chimpanzee is limited by the very nature of the task, rather than by the level of cognitive organization, to using only one strategy. In contrast, human infants combining nesting cups have a greater degree of freedom in terms of possible strategies and possible final constructions, although these are assigned the same value in terms of hypothesized complexity (see Figure 1, target article). For Greenfield, the chimpanzee's behavior during termite fishing, recalls the simple pairing strategy of the

youngest human infants combining nesting cups. I argue below that this view undervalues the combinatorial strategies involved in tool use by primates.

As an illustration of tool use by nonhuman primates other than chimpanzees, I use a study by Anderson (1985), on *Macaca tonkeana*. This serves as a reminder that many instances of tool use have been described in nonpongoid primates (see Beck, 1980, for review), but this type of study is all the more pertinent given that the best nonhuman primate data on Broca's area homologues and prefrontal connections come from macaques (sect. 5.5). Two adolescent male macaques spontaneously learned to use a metal rod to obtain food that was out of direct reach. When engaging in tool use, the monkey pushed the rod out from the cage and oriented it so that the distal end dropped into a plate of honey. The rod was then pulled back into the cage, and the honey was removed by hand or directly by the mouth. To paraphrase Greenfield's description of spoon use by human infants (sect. 3.3): The macaque combines rod with honey (one subject used side-to-side motions, like children) and then brings the subassembly of rod plus honey to the mouth, the final object. Westergaard's (1988) observations of lion-tailed macaques is using probes to obtain syrup and termite fishing by chimpanzees can be viewed the same way. A growing literature on tool use by capuchin monkeys (*Cebus* sp.) also deserves closer inspection from Greenfield's perspective. Tool use by capuchins includes nut-cracking using objects as hammers (Anderson 1990), probing sticks into narrow tubes to dislodge food (Visalberghi & Trinca 1989), and using containers as cups (Westergaard & Frigaszy 1985; or see Visalberghi, 1990b, for a review).

Greenfield seems reluctant to accept the idea that nonhuman primates, even chimpanzees, might be able to perform object combinations based on subassembly strategies. A few "borderline" cases are acknowledged, but others are missed, including Köhler's (1925) account of a chimpanzee fitting two sticks together to form a sufficiently long raking tool. This example illustrates the active construction of a tool subassembly, which is then used on a third object.

I have suggested above that some examples of tool use by nonhuman primates meet the criteria for a subassembly combination strategy at least as complex as that involved in spoon use by humans. If tool use by macaques can be said to reflect the operation of subassembly strategies, a logical next step would be to look for evidence of hierarchically organized "linguistic" behavior in these primates. At present, such evidence is lacking. As pointed out by Greenfield (sect. 5.5), macaques show left hemisphere dominance for processing species-specific vocalizations, but to my knowledge all of the relevant research – based on the effects of cortical lesions – has focused on receptive rather than productive aspects of vocalization. The absence of evidence for syntactic vocal communication in monkeys, along with the potential for hierarchically organized object manipulation strategies, suggests that the neural substrate underlying the homologous ontogenesis of these behaviors in humans (and pongids) may in fact have an evolutionarily older role, being implicated in manual specialization during complex object manipulation. In this context, some of the most interesting data are likely to come from studies in which monkeys and apes are given tasks requiring sequential bimanual coordination (e.g. Fagot & Vauclair, 1988a; 1988b). [See also MacNeilage et al.: "Primate Handedness Reconsidered" *BBS* 10(2) 1987.]

To conclude this discussion of object manipulation and tool-use in nonhuman primates: Greenfield's position is that nonhuman primates are at best limited to borderline subassembly strategies during tool use. My feeling is that the literature review leading to this position is incomplete, but the author has done a service in revealing the need for better developmental data in this domain.

Finally, if sound combinations rather than word combinations are the most useful for illustrating synchrony in the develop-

mental timing of language and manual object combination in human infants (sect. 3.2.2), it can be asked whether the lexigrams used by the chimpanzee and bonobo subjects of Savage-Rumbaugh et al. (1990) are the best medium for revealing a similar parallelism in nonhuman primates. I agree with Greenfield (sect. 5.3) that symbol combination might be the only appropriate level of analysis for those particular apes, but I suggest that sign language-using apes might be a better source of data. Individual signs of American Sign Language can be described in terms of subunits, termed cheremes. B. Gardner et al. (1989) have carefully recorded and described developmental sequences of the place, configuration, and movement of manual gestures in cross-fostered chimpanzees using sign language. Here is one type of productive linguistic ability that might be especially amenable to hierarchical analyses of the type central to Greenfield's hypothesis.

Syntax is not as simple as it seems

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Greenfield's fascinating but occasionally frustrating target article raises more issues than anyone could hope to deal with in the confined space of a *BBS* commentary. What I take to be its most significant contribution is the claim that Broca's area, which until about age two indifferently handles both language and motor activity, subsequently develops distinct if adjacent circuits for these, and that the circuit now devoted exclusively to the hierarchical processing of language receives input from somewhere else, most probably from the left anterior prefrontal area (sect. 4). Such claims must be substantiated by the neurological community. If confirmed, they provide striking support for the proposal (Bickerton 1990) that syntacticized language arose as a result of changes in the circuitry of the brain occurring during the speciation event that produced *Homo sapiens sapiens*. In turn, Greenfield's claim of a "spurt starting at age two in . . . neural connectivity" is supported by the finding of a similar spurt in syntactic capacity starting at or shortly after that age (Bickerton 1991). [See also Bickerton: "The Language Bioprogram Hypothesis" *BBS* 7(2) 1984.]

Because the material explored by Greenfield has such far-reaching implications, it is all the more important to get straight the linguistic facts that any global analysis must be faithful to. It is also important to get the formalisms straight, and some of these – for instance the tree for *bye tat* (Figure 9.III) in which *bye* is generated by conjoining *b* and *eye* and *tat* is generated by two arrows whizzing back and forth between *t* and *a* – did not really reflect properties of language. Granted that the child probably cannot vary consonants word-internally at the age in question; there are still three distinct articulations in *tat*, and hence more layers of structure than Greenfield seems to want to admit at this stage. Still more baffling is Figure 10, where a terminal (K) is attached to a nonterminal node, and where an inflected word (*mangio*, "(I) eat") is produced by first attaching the inflection to *half* the stem and then attaching this hybrid to the rest of the stem! All of us have always assumed that inflections attach to complete stems: If Greenfield is really claiming anything different, she must say what and why.

I had problems, too, with the pairing, pot, and subassembly methods seen by Greenfield as operative in the ontogeny of both manual activity and language. The article claims (sect. 3.2) that sound combination and object combination develop synchronously, with the subassembly method appearing around twenty months. However, according to Greenfield et al. (1972, Figure 3) the pot method remains the dominant strategy for

seven out of eight children from 20 to 32 months, and for five out of eight is still the dominant strategy at 36 months. By 36 months, at the most conservative estimate, a highly complex hierarchically structured syntax has completely taken over in all but the severely retarded, and indeed has probably been fully in place from 30 months onward if not considerably earlier (Bickerton, unpublished; Leopold 1939/49; Limber 1973). Thus, while the *emergence* of subassemblies may be synchronous, it seems that their subsequent development is not. Perhaps this datum follows from the Broca's area bifurcation that Greenfield envisages, but if so, it should have been spelled out more clearly.

Unfortunately, mistaken assumptions about syntax are common among psychologists, and they lead Greenfield to overestimate the linguistic achievements of bonobos. Kanzi, it is claimed, has syntax because he has regular word order in two-word (and even in a handful of three-word) utterances. But word order, in and of itself, does not constitute syntax or even necessarily the basis for syntax. Here psychologists get led astray by the simple fact that they know English, a strictly configurational language, but don't, in most cases, know any of the so-called "exotic" languages. In fact many human languages, including Latin (alas, no longer routinely accessible to the educated), have highly variable, almost-if-not-quite-free word order. Moreover, syntax includes things like knowing that in *John needs someone to work for*, *John* is the implicit subject of *work*, whereas in *John needs someone to work for him*, *someone* is the implicit subject. Between regularity of word order and knowledge like this lies a profound gulf, one that cannot be bridged in any way by the mere multiplication of subassemblies.

Then there is the issue of productivity. Not stated in the target article are the overall statistics for Kanzi's production over a five-month period: 13,691 utterances, of which 89.61% were single-element utterances, 9.45% were two-element utterances and only 0.94% were longer utterances (Greenfield & Savage-Rumbaugh 1990; 1991). Comparable figures for a typical child of 22 months are: single element, 43.21%; two-element, 38.27%; longer utterances, 18.52%. (Figures from Bickerton 1991: Two-word utterances outnumbered single-word utterances in the next and all subsequent months.) These contrasting patterns of distribution suggest that quite different things are going on in bonobos and children under two (and I would not even credit the latter with having true syntax!).

Greenfield claims that her position, though it has much in common with mine, is ultimately incompatible with it. I don't think this is necessarily correct. There is nothing in her paper inconsistent with the hypothesis that Kanzi and pre-two-year-olds have varying degrees of protolanguage, which consists of simply stringing words together or putting one pot into another. Around two or a little later in most normals, some additional brain mechanism, quite a recent development in evolutionary terms, kicks in rather suddenly and causes the child to start processing words in a quite different manner.

I certainly never claimed a "total discontinuity" between language and protolanguage (sect. 7.2; see pages 163, 181, 185, and 187 of Bickerton 1990). The relation of protolanguage to language is rather like that of the tadpole to the frog or of the caterpillar to the butterfly. Obviously, in all three cases, one thing has developed out of the other; equally obviously in each case, the emergent creature behaves in a manner radically different from that of its antecedent form.

The neurological data adduced by Greenfield point clearly toward research that may help us explain the linguistic metamorphosis, and this constitutes a significant step toward our understanding of how language can be produced by the human brain. However, no such goal can be achieved if we minimize the magnitude and subtlety of that metamorphosis. Syntax is nowhere near as simple as it looks. A stronger grasp of its complexity will avert interdisciplinary misunderstandings and accelerate progress toward our common goals.

What does language acquisition tell us about language evolution?

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Greenfield's target article covers a vast range of data and presents several interesting and provocative ideas about the evolution and development of language, but the central claim – that there is a developmental link between symbol combination and object combination – suffers from serious problems. She presents no real evidence for her position, there are several studies that refute it, and even if she were right, this sort of "developmental homology" would have few implications for a theory of the evolution of language. I discuss each of these issues in turn.

1. The argument. The case for a parallel between language development and object combination goes as follows: Greenfield describes different stages of development from the unpublished diary data from three children, notes how these seem to correspond to the stages of object combination she has found in separate studies, and gives interesting examples.

This clearly will not do. Because neonates are at rock bottom on both language production and object combination, it is not surprising that some parallels in development can be found. There are similar parallels between language production and the development of social skills, categorization behavior, and table manners. One reason for this is that the complexity of a young child's behavior is largely restricted by performance factors such as attention span, memory load, and so on. In the case of language, it is well known that children suffer from production limitations that operate independently of knowledge of language itself (see Bloom, 1990b, for a review). Given all of this, it is not particularly surprising that children who cannot utter long sentences also have difficulty constructing piles of objects. They have problems with any behavior that involves processing load – regardless of whether or not it is hierarchical.

Greenfield seems to be sensitive to the issue. At one point, she defends her analysis by stating that "it is not easy to find identical structural substages; nor is it easy to find a close correspondence in the timing of the substages." This may be true, but what reason is there to believe that there is actually a close correspondence in the timing of identical structural substages? All we are given is a series of anecdotes.

Greenfield concludes her presentation of the evidence by noting that it would be useful to do a study on this topic, to see whether there is any relationship between language development and object combination if factors such as age and memory load are factored out. Quite so.

2. Counterevidence. Greenfield argues that the "developmental homology" between symbol combination and object combination only lasts until about the age of two. Prior to this, children have "not yet developed the neural circuits for complex grammar." On the face of it, this position would seem to be refuted by several studies showing considerable knowledge of linguistic structure by children aged 17 months and younger (e.g., Golinkoff et al. 1987; Katz et al., 1974; see Gleitman et al., 1987, for a review.)

Greenfield dismisses this line of research as "neither relevant to the argument nor damaging to the chronology of structural development of speech production outlined earlier." This is apparently because these data are from comprehension, and her theory concerns only production. Thus, her claim that "language and tool use evolved together phylogenetically and develop together ontogenetically" presumably applies only to language *production*, not to language *per se*. This qualification has some odd consequences for her speculations about evolution and homology. Is she claiming that the capacity for language comprehension is *not* homologous with object combination? And that language comprehension evolved independently of

language production? (Although some asymmetry between comprehension and production is possible, it seems unlikely that our evolutionary ancestors ever had the innate ability to comprehend abstract syntactic structures that nobody could yet produce.) In general, it could clarify things if Greenfield devoted some of her reply to discussing how the putative independence of comprehension and production relates to her claims about evolution and homology.

Even with regard to production, there is evidence against Greenfield's position. She discusses the growth of sentential and inflectional complexity in English and Italian as if this developmental trend were universal. It is not. Children acquiring language such as Quiché (Pye 1983), Polish (Weist et al. 1984), and Turkish (Aksu-Koç & Slobin 1985) show very precocious morphological development relative to children acquiring languages such as English. For example, Aksu-Koç and Slobin (1985, p. 845) note that for children acquiring Turkish "the entire set of noun inflections and much of the verbal paradigm is mastered by 24 months of age or earlier. . . . Both noun and verb inflections are present in the one-word stage, and there is some evidence for productive use as young as 15 months." It is difficult to reconcile these facts about language development with Greenfield's position.

3. The relevance. Even if Greenfield's conjecture about symbol combination and object combination proved to be correct, it is unclear how it would relate to a theory of the evolution of language. Greenfield accepts a nativist model of language development (e.g., Chomsky 1980) and argues that the capacity for language is a unique human competence, one that emerges as a result of neural maturation. Unfortunately, the target article says little about this capacity; it concerns instead what Greenfield sees as a prelinguistic stage of development, one where the neural structures that underlie communicative behavior also control other abilities, such as object combination. Whether or not such a stage exists is of considerable importance for theories of cognitive development, but it has little to do with the evolution of language. In other words, if Greenfield is right that children younger than two have not yet started to use their innate language faculty then we are not going to learn much about language by looking at the behavior of these children.

Greenfield takes this stage of child development, along with evidence from chimpanzees, as suggesting that the capacity for language production (though presumably not the capacity for language comprehension) has evolved "out of" the more general ability to organize elements hierarchically. But, as Simon (1969) points out, virtually all complex systems exhibit hierarchical structure. The real mysteries about the evolution of language concern those properties that are not shared by other cognitive capacities: such phenomena as phonological representations, syntactic structure, constraints of anaphoric interpretation, inflectional morphology, thematic relations, and so on. It is properties like these that are absent from the behavior of trained chimpanzees and yet show up in the language of very young children. How did these evolve?

Evidence from language acquisition – and from the genetics of language acquisition (see Bever et al., in press; Gopnik 1990) – may help us solve these mysteries. To take just one example, developmental data can help us decide whether certain "functional" properties of adult language exist as a result of the child's desire to communicate or because of natural selection (see Newmeyer, 1990, for discussion). Greenfield's target article is a valuable contribution, then, because it is one of the few attempts to relate evidence from language development to the question of how language has evolved. Although I disagree with much of what Greenfield says about early child language, it is undeniable that she has embarked on an exciting and promising line of research.

Hierarchies and tool-using strategies

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In presenting a model of evolutionary homology Patricia Greenfield argues persuasively for the existence of a common neural substrate underlying the development of language production and manual object-combination during an infant's first two years. The three strategies for combining nesting cups (Figure 1) have their parallels with the development of children's sentence types (Figure 3) and tool use that she considers to be a special case of object combination programmes. On theoretical grounds, supported by "some suggestive observations" from Connolly and Dalgleish (1989), Greenfield predicts that an analogue of the "pot" will be found in the case of tool using. More specifically, she predicts an intermediate stage where the infant brings food to the mouth with the fingers of one hand and the spoon directly to the mouth with the other – the mouth serving as a pot for the two.

In an attempt to test Greenfield's prediction, we re-examined the data (though not the original tapes) from Connolly and Dalgleish (1989). In these observations the activities of the contralateral hand (the hand not holding the spoon) were recorded and classified into task-related and non-task-related sets. Task-related activities by the contralateral hand increased as the skill was acquired and by far the most common was steadying the dish. Occasionally, though rarely, the fingers of the contralateral hand were used to push food onto the spoon in the dish or to push the spoon into the infant's mouth. No observations of the kind predicted by Greenfield were recorded. In the interpretation of this information, two things must be borne in mind. First, mothers tend to discourage their infants from feeding by hand because it is messy; second, had the dish been fixed to the table the hand used for steadying may well have been freed for other purposes.

In a further study, Connolly and Dalgleish (in press) made observations on four infants at weekly intervals where the emphasis was on *individual* and not group patterns of change. Again, nothing resembling the "pot" strategy was observed and one of the infants over the period 12–18 months showed a decrease in task-related activities with the contralateral hand. In a further series of observations linked to changes in the consistency of the food and the physical properties of the spoon it was noted that the visual appearance of the food appeared to have an effect. If it was lumpy, the infants were more inclined to use their fingers. Food containing small objects, such as peas or small pieces of cereal, led to the infants' using a precision grasp (thumb-index finger opposition) to pick out bits from the dish. In one case a child was given a dish containing mashed banana and a small slice of bread and butter. One might expect this to have encouraged the use of both hands in the feeding process; one with a spoon, the other not. The child, however, attempted to place the bread and butter on the spoon and to transport it to the mouth.

Another observation that may relate to the "pot" strategy was recorded for one child at the age of 50 weeks. In the first week of observation this child used the spoon to transfer food from the dish to the mouth, typically producing two units of feeding in this fashion. She then used her hand to transfer food from dish to mouth switching back again to the spoon after one handful. Something of this spoon/spoon/hand/spoon/spoon/hand pattern was seen until around 13–14 months. The spoon was never put in the mouth when food was transferred by hand, however.

There is general agreement on the hierarchical nature of behavioural organisation in speech and manual object combination, of which tool using is a special case. But how do such hierarchies develop? A central problem for hierarchy theory is the relationship between the structural level (the neural substrate) and the descriptive level (the cognitive processes; Pattee

1973). It is difficult to draw a line between these two levels in biological systems because no necessary dominance of the descriptive over the structural level is implied as computer analogies normally lead one to believe. Indeed, a combination of both seems to be the main feature underlying the increasing differentiation and integration of skilled units into progressively more specific and complex actions (Connolly 1970).

Greenfield argues that in such a tool-using skill as eating with a spoon, young infants use combinatorial rules similar to the ones they would use to seriate and manipulate objects. She appears to have focused on only one feature of the hierarchy, however: its intentional aspects. Connolly and Dalglish (1989) point out that action has two aspects: the *intentional*, which is the purpose or goal that must be accomplished, and the *operational*, which is concerned with how the goal is to be attained (p. 896). These two features of the skill are like opposite faces of a coin, different but tied together. Intentional aspects are concerned with the emergence of an intention to produce a particular effect in the environment; they entail some knowledge of the properties of the tool as a means of effecting the desired result. Operational aspects relate to the performance strategies used to solve the problems attending the attainment of the desired outcome. So far as the development of skills is concerned, the intentional and operational aspects are dynamically interrelated, in the sense that the intentional level conditions rather than controls the operational level. Thus, some freedom of action is a necessary feature of the operational aspects of a skill. A similar principle can be identified in language production (McNeill 1971). The production of sentences presents regularities only on a macro level (encoding the structure of the sentence); on the micro level it dissolves into unpredictability, so great is the number of alternatives that can be introduced (nouns, verbs, adjectives, and adverbs) without altering the structure of the sentence.

Greenfield is mainly concerned with highlighting the parallels between action (tool use) and language; if we are to understand the development of the hierarchy, however, the process leading to macro level order needs to be scrutinised. To understand how hierarchical development occurs it is necessary to look at a system top-down and bottom-up; without this the nature of the evolving process may be misjudged (Mesarovic et al. 1970). In Connolly and Dalglish's (1989) study the young infants faced an ill-defined problem; they knew clearly neither the goals to be reached nor the means of doing so. The emergence of an intention has to do with the context in which the infant is embedded but the combinatorial rules inferred from the behaviour are also affected by the exploration of strategies on the operational level. The rules can certainly constrain the pattern of exploratory activity, but the way changes at the operational level influence the development and differentiation of intentions should not be overlooked. Consider also the examples from early syntax that Greenfield illustrates in Figure 2(b). The child wants to eat more crackers and so produces the verbal unit "more" or "more cracker." This utterance will no doubt be accompanied by such nonverbal units as orienting toward the packet, reaching with outstretched hand, opening the mouth, and so on, and together these are organised into a complex action that contrasts with the relatively rudimentary syntax observed at the verbal level. Such effects demonstrate that even at this elementary level the system is able to explore and develop strategies dynamically, although looking at the macro level alone one has only the impression of regularity in the solution of a problem. Eventually a comparison between rudimentary and sophisticated grammars of action will indicate that many initial strategies were subsequently dropped. The important feature of the transition to a more complex state may have been not so much the elimination of particular strategies but the *process* through which it is necessary to go to eliminate them.

Connolly and Dalglish (in press) studied patterns of change

in tool use by individual infants. They found that individuals tend to show patterns similar to those observed in their group data (Connolly & Dalglish 1989), revealing, in Greenfield's terms, the extent of a particular combinatorial rule. Observations on individual children, however, indicate that the pattern of an action is shaped in the individual-environment encounter. It is at this level that intentional and operational aspects are in dynamic interaction. The nature of this interaction between ends and means merits further consideration.

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Anatomy of hierarchical information processing

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Greenfield's target article brings together comparative, developmental, and pathological evidence to support the hypothesis that the cognitive functions underlying the unique human competence for language evolved from more domain-general abilities for analyzing and manipulating hierarchical information. This is an insightful and persuasive argument, although there are many details that are vague or problematic, and some significant questions are begged that cannot yet be answered.

Dissecting language competence. Many linguists and cognitive scientists have claimed that the types of syntactic structures and the logical interdependencies among the grammatical rules preclude the possibility that language abilities evolved by the gradual elaboration of cognitive capacities possessed by apes (for recent statements see Bickerton 1990; Piattelli-Palmarini 1989). It is argued that modern language abilities originated as a result of a single remarkable mutation event producing a fundamental rewiring of the brain in just such a way that all types of syntactic operations were thereby prefigured. No more miraculous "hopeful monster" has ever been envisioned. The alternative presented here is that grammatical analyses do not depend on some unprecedented form of cognitive calculation but rather on one that was co-opted from some other related domain of cognition and subsequently developed to suit the demands of its new function.

The perennial arguments over the cognitive uniqueness of language are undercut by investigating one crucial component cognitive process, hierarchical analysis, rather than language competence taken as an indivisible whole. This yields insightful comparisons across both cognitive domains and species. Hierarchical analytic abilities and constructional behaviors seem to be a particularly promising starting point because many of the most unprecedented features of language structure derive from its hierarchical recursive character.

Although a number of authors have suggested parallels between the organization of complex goal directed actions and features of grammar (e.g., Deacon 1989; 1990a; Greenfield 1978; Kimura 1979; Lieberman 1984; 1991; Reynolds 1976; 1983), the account given here goes further toward specifying a precise class of behavioral strategies and their grammatical parallels. The argument differs from many earlier theories of this sort in its focus on what is more properly a cognitive task rather than a skilled motor task. More important, the focus on hierarchical processes as opposed to merely complex sequencing abilities addresses more directly the challenge of explaining the structure of grammars, not merely the ability to string words together rapidly in speech. This approach does beg a troubling semiotic question: Why is hierarchical-recursive logic so central to language and not to other forms of natural communication?

And it also poses a daunting theoretical challenge: demonstrating how these hierarchical calculational abilities can account for the various rule systems of grammar.

The argument for the functional homology between hierarchical and grammatical abilities does not rest solely on resemblances between grammar and other forms of cognition and action, but also on parallel maturational sequences for phonological and manipulative hierarchical abilities and the fact that brain damage that impairs both speech and grammatical analysis also often impairs hierarchical visual-manual abilities. This is supported by complementary anatomical correlations between differently localized linguistic functions (as demonstrated by brain stimulation and imaging studies), maturational trends linking these regions, and corroborative axonal tracer data from monkeys. The demonstration of minimally developed hierarchic reasoning capabilities in chimpanzees for both object and symbol manipulation shows that this particular homologous ability is unusually developed in humans.

Anatomy of hierarchic abilities. The argument for homology can be amplified by considering some more specific comparative anatomical data. The focus on chimpanzees as the only major nonhuman comparison is dictated by phylogenetic concerns and successes in teaching them language-like abilities. Unfortunately, there is even less detailed functional neuroanatomical information for chimpanzees than there is for humans because of ethical restrictions on invasive research. We must turn instead to studies of monkey brains for detailed physiological and neuroanatomical comparisons. Despite the greater phylogenetic distance separating us from monkeys, extensive neurophysiological, lesional, and axonal tracer studies provide a rich source of homological predictions.

Although to my knowledge identical hierarchic tasks have not been tested in monkeys, closely related paradigms have been used to investigate the function of specific frontal lobe regions. Damage to premotor cortex, located just in front of primary motor cortex, has been shown to disturb the ability to organize the sequential order of fixed multistep behaviors while leaving individual behaviors relatively intact. With more rostral damage, however, simple sequential organization is relatively unimpaired and more complex dependency relations are impaired. Petrides (1982; 1986) has shown that damage to a region around the arcuate sulcus, which stands at the interface between prefrontal and premotor cortex, specifically impairs conditional association learning. This involves stimuli and behaviors that have a logically nested relationship to one another, not just sequential organization. In this paradigm, the animal must use the results of one behavior in a sequence as information to determine which among the subsequent alternative behaviors should follow in which order. Finally, damage to the prefrontal area in front of the arcuate sulcus, particularly including the area of the principle sulcus, disturbs the learning of spatial delayed alternation tasks (Stuss & Benson 1986). In this paradigm, information about the success of a prior behavior must cue the animal to shift that same behavior to an alternate spatial position in the subsequent trial.

All three paradigms involve sequential dependence of behaviors on one another, however; as we consider the tasks sensitive for progressively more rostral frontal areas, different levels of hierarchic dependency emerge. In a simple sequence a subsequent behavior depends solely on the presence of the immediately preceding behavior. In a conditional sequence the performance of a behavior is not determined solely by the immediately preceding event; rather, the choice among subsequent behavioral "subassemblies" depends on the preceding behavior. Finally, in delayed alternation it is the spatial context of the performance of a behavioral subassembly that must be shifted with respect to its immediately previous spatial context. It is probably not coincidental that these kinds of tasks roughly parallel the developmental sequence described for hierarchic behavioral strategies in grouping cups and copying tree struc-

tures. It also parallels the set of tasks that Piaget used to define the last three stages of sensory-motor development in infancy (i.e., hidden object tasks). This also corroborates claims for a caudal/rostral developmental sequence, but suggests that the proposed two-stage circuit model probably needs expansion to include at least three steps (linking four areas: prefrontal, arcuate, premotor, motor).

Monkey connection data from tracer studies suggests a further complication of the model. Most of the behavioral studies summarized above have involved areas on the dorsolateral convexity of the prefrontal cortex, regions that have prominent corticocortical connections linking them with associational visual and somatosensory areas of the posterior cortex, including areas of hand representation (Barbas & Mesulam 1981; 1985; Cavada & Goldman-Rakic 1989). These connections are entirely consistent with the visual-manual nature of the tasks impaired by damage in these areas. Other tracer studies (Deacon 1984; 1989; in press) have shown that the premotor, arcuate, and prefrontal areas just ventral to these frontal visual-manual areas lack visual associative connections and instead have extensive auditory associative connections as well as somatosensory connections that are probably associated with oral-facial sensation. This topography suggests that the ventral region comprises a parallel but modally distinct set of areas in which hierarchic processing of aural-oral information predominates. The modality segregation appears to be relatively complete for visual versus auditory modalities in arcuate prefrontal areas (Deacon, in press), whereas there appears to be considerable overlap and interrelationship between hand, face, and mouth representation in the inferior premotor cortex (Rizzolatti 1987).

Thus, the correlations between hierarchic manual and phonetic development and the independence of visual-manual and grammatical development may correspond to this within-modality versus between-modality difference. In agreement with the primate anatomy outlined above, human data from cerebral blood flow (e.g., Ingvar 1983; Nishisawa et al. 1982; Roland 1985; 1987) and PET studies (e.g., Petersen et al. 1988; Posner et al. 1988) demonstrate a highly active area quite ventral in the prefrontal lobe and rostrally separated from the classically defined Broca's area during tasks involving silent reading, identifying objects based on verbal description, active listening to speech, generating word lists, and producing spontaneous word associations. Figure 11 (though intentionally ambiguous) probably misrepresents the relative position of the prefrontal language circuit. It should originate much lower. Monkey anatomy lends little support to the idea of a single supramodal hierarchic processor, as suggested by Grossman (1980).

Recapitulationism. The notion that young children's mental capacities are to be compared with those of the adult ape and that later childhood development can be understood as the addition of more advanced stages beyond this ape level is based on a seductive but fallacious view of evolution and the brain. Greenfield's target article only narrowly avoids such assumptions in numerous places. Like the adult ape, the very young child is incapable not only of language but presumably also of many other modes of thought that are correlates of language ability. The ape remains incapable, but the child eventually passes through ever more sophisticated stages of language ability. Yet ape minds are not arrested human minds, and older children do not simply extend some developmental trajectory that apes only begin. Human and ape mental development begin at birth at different levels of maturity, proceed at slightly different maturational rates, and diverge increasingly as development continues (Antinucci 1989). Adult apes can perform mental calculations of which prelinguistic children are incapable, and human infants engage in behaviors that are impossible for adult apes. Linguistic specializations do not wait to appear until after the human child has passed beyond the last ape developmental stage. Unique human language adaptations ap-

pear at very early developmental stages. Notable among these is the human infant's unprecedented capacity and tendency to babble, beginning early in the first year.

The idea of developmental differentiation is used to compare the brain of the prelinguistic child and the chimpanzee, assuming that the relatively undifferentiated state of the child's brain is comparable to the brain of an adult ape to the extent that there is less differentiation and independence of the manual and symbolic sequencing systems. Both the anatomical and behavioral evidence are doubtful on this point, however. Both species' brains pass through similar developmental stages, and there is no evidence that the human brain carries this process farther. Kanzi manipulates lexigrams manually and so the manual/symbolic distinction in this case is questionable, and is certainly not directly comparable to speech. Apparent correspondences between children's abilities and those of adult apes in these domains are probably only superficial similarities, not homologies. Structural and functional homologies can be demonstrated without drawing on this questionable evidence.

A comparative view of object combination and tool use: Moving ahead

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Greenfield has provided a thought-provoking interpretation of the behavioral and neuroanatomical link among manipulation, tool use, and language. I focus my comments on the value of Greenfield's analysis of the structural components of object manipulation for comparative research, in line with my interests in manipulation and tool use in nonhuman primates (Frigaszy & Adams-Curtis, in press; Frigaszy & Visalberghi 1989). The combination of objects in manipulation is presented as structurally homologous to the combination of words in early language. Tool use is designated as a special case of object combination that involves the same structural features as other forms of object combination. Structural features of tool use and other forms of manipulation are seen as dependent on a single overarching source of grammar for action in the brain.

Greenfield's analysis of object manipulation and tool use suggests a specific direction for research on the developmental and phylogenetic bases of tool using skills. It leads to hypotheses linking tool use with spontaneous manipulation outside of tool-using tasks: Those individuals (and species) exhibiting certain combinatorial forms of action grammars (such as pairing and "pot" combinations) in spontaneous manipulation should exhibit simple forms of tool use (those in which a tool is paired with one other object, for example). Those species exhibiting "sub-assembly" in manipulation, in addition to the two simpler combinatorial forms, should also exhibit tool use in which a sequence of actions links one object with another, which are then used as a single tool on a third object. Analysis of combinatorial structures during manipulation can proceed directly on the basis of observed behavior, without involving inferences about intention or representational capacity – an advantage over the neoPiagetian program to examine the same behavioral capacities (cf. Chevalier-Skolnikoff 1989).

Testing Greenfield's ideas requires assessing (1) spontaneous manipulation in terms of action grammars (i.e., hierarchical organization of action) and (2) tool use in tasks requiring specified elements of action grammar. Hierarchical combinatorial strategies present in normative manipulation should also be available in tool-using tasks. Developmentally, one should expect the appearance of combinatorial actions in routine manipulation to allow tool use that is dependent on those forms of combination. Thus there should be temporal synchrony in the appearance of elements of action grammars across tasks. The

availability of a combinatorial strategy does not make its use inevitable, however, and determining the factors influencing the implementation of similar strategies across tasks takes on central importance at this juncture (Fischer 1980). We should expect, even if there is a single "grammar-production center" in the brain, to observe unevenness in the expression of strategies across domains and individuals. These are clearly issues that comparative and developmental researchers can and should address.

Comparative work to date on tool use in nonhuman primates, my own included, has not dealt systematically with the task requirements in tool-using tasks. Rather, we have been largely preoccupied with documenting the occurrence of tool use and trying to understand its relation to phylogenetic, social, and ecological factors. We do have a fair amount of descriptive data, on object manipulation across nonhuman primate species, however, that address the occurrence of combinations one way or another (e.g., Antinucci 1989; Torigoe 1985); and we have a fair idea of the relative frequency of tool-using behaviors across species. Object manipulation is well developed in virtually all primate taxa, yet tool use is commonly observed only in a few (notably chimpanzees and capuchins). The available comparative data, although they do not provide a direct test of the hypothesis, suggest that species of nonhuman primates differ significantly in the frequency with which they combine objects with substrates or with each other during routine manipulation, and that these differences correlate positively with the propensity to display tool use. Combinatorial actions among nonhuman primates are most likely to involve the pairing strategy or the pot strategy, in Greenfield's terms; higher levels of hierarchical organization are probably rare. We do not know at present whether nonhuman species differ in the number of structural variations for actions combining objects present in their repertoire. It will be important for advancing our understanding of comparative cognition to determine whether such differences exist.

Tool use requires combinatorial actions, but it does not always require actions more complicated than simple pairing. For example, from my reading of Greenfield, using a stick to "fish" for termites (by placing a stick into a termite mound, and then bringing stick to mouth) involves two sets of pairing actions, but no subassembly. Do species that rarely or never exhibit tool use differ from common tool users in their failure to pair behaviors? This is an empirical question. An alternative possibility is that the sheer frequency with which simple combinatorial actions (such as pairing) are performed, or the variety of simple forms that are produced, may be sufficient to account for species differences in the frequency and variety of observed use of tools.

We have developed the latter idea from our work with capuchins, which devotes enormous amounts of time and energy to manipulation, much of it involving actions combining objects with a substrate (Frigaszy & Adams-Curtis, in press), and which are anomalous among nonhuman primates for their frequency and variety of tool use (Visalberghi 1990b). We have used the concept of "generativity" to refer to the dimensions of variety in capuchins' manipulative activity that we view as supportive of the appearance of tool-using behavior. Tool use in capuchins mainly involves using an object against a substrate (such as a stick to penetrate into narrow openings), which is structurally parallel to the forms of manipulation they exhibit with great frequency outside tool-using tasks. If capuchins differ from other species of monkeys in the frequency of combinatorial actions during routine manipulation but not in the possession of action grammars producing these combinations, their uniqueness in tool using can be attributed to organizational features outside the action grammar system.

Up and down the frontal hierarchies; whither Broca's area?

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Few will quibble with the essential principles and assumptions of Greenfield's work, for they are supported by an enormous body of empirical evidence. Speech and skeletal movement are indeed hierarchially organized and develop in parallel, ontogenetically, and probably also phylogenetically. In the human primate, both have their substrate in the cortex of the frontal lobes. Whether and when the two develop in synchrony, however, is very difficult to ascertain. There are simply no reliable criteria of equivalent hierarchical ranking for the two domains of behavior, no clear isomorphic relationship between them. Therein lies the main obstacle to following Greenfield's reasoning in attributing to them an early common substrate and arguing for the split of that substrate in the course of ontogeny. In making Broca's area that primordial substrate, the problem becomes especially difficult, because the identity of this area in nonhuman primates is debatable – and so is animal language by whatever definition.

With these basic methodological questions unresolved, Greenfield's tantalizing hypotheses remain plausible but undemonstrated. Even if we assume that her homologies between speech and manual behavior are approximately correct, we still have to contend with thorny problems of neuropsychology, electrophysiology, and microscopic anatomy. Clearcut double dissociations are extremely rare, especially in the human brain, because nature simply does not plan experiments the way we do. Cortical lesions and their consequences vary widely even within the best selected clinical samples; neither their size nor their remote effects conform exactly to hypothetical boundaries in the mind of the neuropsychologist. [See *BBS* multiple book review of Shallice: *From Neuropsychology to Mental Structure*, *BBS* 14(3) 1991.] Nonetheless, the frontal dissociations adduced by the author are highly suggestive of the division of neural labor that she hypothesizes for speech and tool use beyond a certain age; for further support one would like to see more right-frontal controls. As to the electrophysiological data, the coactivation of different cortical areas is certainly indicative of mutual connectivity and even participation in the same cortical network, but it is not conclusive evidence of commonality of function. Dendritic branching, as Greenfield admits, tells us little about specific ontogenic trends of cortical connectivity. [See also Goldberg: "Supplementary Motor Area Structure and Function: Review and Hypothesis" *BBS* 8(4) 1985.]

Be all that as it may, much of the evidence used by Greenfield to make her case could be used for a somewhat different view of things that appears to me more plausible and still in many respects compatible with hers. In fact, Greenfield's views come close to mine (Fuster 1989) if we strip Broca's area of primordial status and place it where I believe it belongs: in the role and position of a specialized and late developing prefrontal area that sits high but not necessarily at the top of the hierarchy of command for spoken language and that, like language, is part of the exclusive patrimony of our species. Its intermediate hierarchical and ontogenetic position is fully compatible with (a) species specificity and (b) a functional role within a hierarchy for representing and processing action that transcends the human species.

Frontal areas develop ontogenetically, and presumably also phylogenetically, along the maturational gradients indicated by myelogenesis: first primary motor cortex, then premotor cortex, and finally prefrontal cortex. Those stages of morphological development would parallel and support the formation, from the bottom up, of a neural hierarchy for representing action.

Ascending representations of "efferent copies" of motor action in a corticofrontal hierarchy, would form progressively higher classes of "motor memory," from the more concrete aspects of muscular movement to the more abstract representations of global action (schemata). In the mature organism, the processing of action would engage the hierarchy in the opposite direction, from the top down – in other words, from the cortical schemata of goal-directed action sequences to the "microgenesis of the action" in primary motor cortex.

Broca's area would mediate the praxis of speech by controlling and pacing the oropharyngeal cortex (yes, its internal "tool"). Premotor cortex (including the supplementary motor area, SMA) would perform a comparable role for general musculature. Thus, in the developmental and physiological scheme of things, Broca's area would lie somewhere between the ancestral and commanding position in which Greenfield puts it and that of a derivative output structure that others attribute to it. None of this is incompatible with the supreme specialization of the entire prefrontal cortex, including Broca's area, in the brain of the human.

NOTE

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Absence of evidence and evidence of absence

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The modest, inquiring approach of such modern psycholinguists as Patricia Greenfield is a refreshing advance over the aggressive dogmatism of the Chomskian period. Where Chomsky presented "an instantaneous model of language acquisition" (Chomsky 1967, pp. 441–42), Greenfield finds significant patterns of development. Where Chomsky separated the language function from all other cognitive functions (Chomsky 1979), Greenfield relates linguistic development to the rest of cognitive development, including object manipulation. Where Chomsky denied any relation between human language and the rest of animal behavior (Chomsky 1979), Greenfield sees parallel patterns of cognitive and linguistic development in some other species, at least in early stages.

Less modern is the theme of a *scala natura* in this target article. Since Aristotle, natural philosophers have proposed criteria for ordering the animal kingdom along a scale that would place humanity firmly and scientifically somewhere above the brutes, yet lower than the angels. Like college instructors looking for a gap in the distribution of examination scores that justifies awarding A's to one group of students and B's or less to the rest, natural philosophers look for a gap that separates human from brute. Time after time these gaps in nature have turned out to be temporary gaps in human knowledge, soon filled in and obliterated by the next wave of research.

Greenfield's proposal is based solidly on the argument that a gap in the evidence must represent a gap in the *scala natura*. In her view, as in the view of so many gap seekers before her, absence of evidence is evidence of absence. In her case, however, the absent evidence is only absent from her review. A wealth of well-known and well-documented evidence already exists to fill in the gaps in her review. We only sample this evidence here.

The unbridgable chasm in this target article begins with the ability to pick up food in a spoon and bring it to one's mouth. Greenfield failed to find a single example of nonhuman tool

making or tool use that was significantly superior to the act of eating with a spoon. A chimpanzee could achieve a higher level in Greenfield's view if one could "combine two objects into a tool subassembly that can then act on a third object outside the chimpanzee's own body" (sect. 5.2, para. 7). The arbitrary clause, "outside the chimpanzee's own body" was apparently inserted here to rule out the case of spoon-plus-food-to-mouth.

In Goodall's (1986) detailed and thoroughly documented observations, chimpanzees gather stems of grass and strip off the leaves to make termite fishing poles. It takes skill to fish for termites. At Gombe we, two human hominids, "hooked" very few termites between us in an hour of trying, even after watching many chimpanzee demonstrations. Gombe chimpanzees also crumple leaves to make sponges, which they then use to extract drinking water out of shallow puddles. The termite soldiers that bite the modified stems as invaders in their nests, and the water that is absorbed by the crumpled leaves, are in both cases third objects entirely outside the chimpanzees. That the stems or the leaves are next used to convey termites or water to a fourth object, a chimpanzee mouth, indicates that these chimpanzee-made tools are dual purpose ones.

In Kohler's (1925/1959, pp. 113–19) detailed and thoroughly documented observations, a young chimpanzee, Sultan, spontaneously inserted the end of a thin bamboo stick into the hollow end of a second length of bamboo, thus making a longer stick. Sultan then ran with this new object to rake in food that was out of reach with either of the short sticks. Sultan extended this skill to a situation in which the food was so far away that he had to join three sticks together. The three-stick tool was awkward to handle so Sultan would shorten the tool by disconnecting sections of bamboo as he raked the food closer. Greenfield cites Kohler (1925) and Goodall (1986), but her descriptions of non-human tool making are so selective and incomplete that they distort the published record.

Meanwhile, many significant studies of tool making are never even mentioned. Captive chimpanzees, gorillas, and orangutans are skillful painters. When painting, these nonhuman primates dip the brush into a paint pot and then bring the assembled tool to the paper (e.g., Lenain 1990; Morris 1962, p. 48). Cross-fostered chimpanzees have access to a much wider variety of objects and offer a richer set of observations. There are additional examples of tool making in our unpublished films of cross-fostered chimpanzees, but we confine ourselves here to published work. Hayes and Hayes (1954) published a film illustrating a variety of examples of tool use and tool making by the cross-fostered chimpanzee, Viki. For example, Viki could thread a needle and then use the newly created object to sew cloth. Viki could also strike a match and then use the lighted match to make a small bonfire of scrap paper in an ash tray. The Hayes film is well-known and readily available to all.

With so much evidence of complex tool use and tool making by 1972, the remaining question was whether a nonhuman could use a tool to make a tool. Wright (1972) described how he taught the orangutan, Abang, to strike a flint core with a crude stone hammer to create a flake. Abang then used the sharp edge of his newly made tool to open a box by cutting a cord. The box contained food, but all of Wright's teaching was by demonstration. Once he had mastered the process, Abang proceeded on his own, without demonstrating or encouragement. He also invented variations, such as using his teeth to hold a flake that was too small to operate with his fingers.

Natural philosophers who base their theories on gaps in the evidence must assume that all significant discoveries in their domain have already been made. Working scientists generally assume that the best is yet to come, and future research will yield one surprise after another. Two notable exceptions among working scientists are Premack (1986) and Terrace (1979), who claim that their investigations have revealed the outer limit of chimpanzee intelligence once and for all. [See also Premack: "The Codes of Man and Beasts" *BBS* 6(1) 1983; and Premack &

Woodruff: "Does the Chimpanzee Have a Theory of Mind?" *BBS* 1(4) 1978.]

Greenfield takes the list of two and three unit combinations in a five-month sample of the output of one juvenile bonobo, Kanzi, as the ultimate limit of intelligent communication for nonhumankind. This would contradict her own thesis about parallels with genetic relations and manipulative skills. Genetically and physiologically bonobos are further removed than chimpanzees from the main hominid line, though not so far removed as gorillas and orangutans (Stanyon et al. 1986). Also, both captive and wild bonobos are far less inclined to engage in object manipulation and tool making than chimpanzees (Heltne & Marquart 1989).

Kanzi communicated with humans by pointing at abstract forms displayed on a large response panel. Savage-Rumbaugh and her associates sometimes call these forms "lexigrams" and sometimes "words." When Kanzi pointed at someone in view, his action was transcribed as if he had pointed at a lexigram for "person." Nearly all Kanzi's lexigrams are for different kinds of foods, drinks, destinations, or services (Savage-Rumbaugh et al. 1986, Table 1). "He usually used his combinations to communicate about actions that he was planning or objects he wanted" (sect. 5.3, para. 5). His response panel can be described as a wish list. By contrast, sign language studies of chimpanzees yield vocabularies and conversations that are rich and varied (B. Gardner et al. 1989, Table 3.1, 3.2). Washoe, Moja, Tatu, and Dar often invented combinations of signs to name objects, such as COLD BOX for the refrigerator, DIRTY GOOD for the toilet, METAL HOT for a cigarette lighter, and LISTEN DRINK for Alka-Seltzer (see also Fouts 1975).

When Kanzi moved from place to place, the response panel had to be folded for portability and set up again at the next location so that he could resume his dialogues. By contrast, Washoe, Moja, Pili, Tatu, and Dar usually initiated conversations. They signed to themselves about pictures in books, and they even climbed high up in a tree to sign to themselves in privacy (R. Gardner & Gardner 1989, pp. 23–24). Special samples and tests were the only kinds of signed interaction in which the humans initiated all initiated all signed exchanges. In the Fouts laboratory, Washoe actively taught signs to her adopted son, Loulis (Fouts et al. 1989); Washoe, Moja, Tatu, Dar, and Loulis commonly carried on conversations among themselves when no human was present, and all their signing had to be recorded on video tape (Fouts & Fouts 1989).

Savage-Rumbaugh et al. (1986, p. 217) report that in a typical videotaped record Kanzi produced 10.2 utterances per hour. In a comparable videotaped record made at about the same age (40–49 months), Dar's output was 479 signed utterances per hour, of which 49% were combinations of two or more signs (Rimpau et al. 1989). The list that Greenfield and Savage-Rumbaugh (1990) analyzed was extracted from a sample in which 10.4% of the utterances were combinations of two or more lexigrams. This explains why it took 5 months of constant recording to obtain a list of 723 combinations suitable for analysis. It also explains why it is so easy for Savage-Rumbaugh and her associates to record all of Kanzi's output, and so impractical to record more than representative samples of the output of human children and cross-fostered chimpanzees.

When human children combine phonemes to create and modify words, Greenfield (Figure 9) credits them with a level of construction comparable to spoon-plus-food-to-mouth. They can achieve a higher Greenfield level by combining two such created words, and a still higher level by combining three words. Consequently, Kanzi starts one level behind any child because the complexity of each lexigram in the Rumbaugh & Savage-Rumbaugh system is a fixed unit. He can reach the spoon-plus-food-to-mouth level only when he combines two pointing acts, and he can get above that level only when he combines three points in certain special ways. The only creative device available to Kanzi was the sequence of his points.

The signs of natural human sign languages have a structure that parallels the phonemic structure of spoken languages (Stokoe et al. 1965). They are constructed by combining and recombining smaller gestural units. They have what Hockett (1978) called "duality of patterning." Table 3.2 in B. Gardner et al. (1989) illustrates in detail the way that the signs in the vocabularies of Washoe, Moja, Tatu, and Dar combined and recombined the phonemes of ASL. Moreover, their errors (as confirmed by two independent observers who could not see what the chimpanzees were naming) also depended on the phonemic structure of ASL (R. Gardner & B. Gardner 1984).

Grammar in natural sign languages depends on inflection rather than on sign order. Sign language inflections are modifications that depend on the phonemic structure of signs. Rimpau et al. (1989) present a detailed analysis of Dar's constructive use of signed inflection to specify person, place, and instrument of actions. These are particularly interesting because in this sample Dar used childish variants of adult inflection that were certainly not modeled for him by his adult humans. For example, he used one of the sign language inflections to specify that he wanted to be tickled with a favorite hard rubber toy. His inflections were both productive and original.

To support their claim that Nim had reached the ultimate limit for chimpanzees, Terrace and his associates (Seidenberg & Pettito 1979; Terrace 1979) claimed that all other sign language studies of chimpanzees suffer from certain experimental errors. These claims are contrary to fact and contrary to the published record, as explained in detail in Van Cantfort and Rimpau (1982) and R. Gardner et al. (1989). In Greenfield and Savage-Rumbaugh (1990), Terrace and his associates are cited as the last word on the subject without any mention of the subsequent replies or subsequent research with Moja, Pili, Tatu, Dar, and Loulis. For nonpartisan comments that consider both sides of this argument see Byrne (1990). [See also Whiten & Byrne: "Tactical Deception in Primates" *BBS* 11(2) 1988.] In omitting sign language studies of chimpanzees as in omitting so many significant records of tool use and tool making, the gap that the target article specifies between brute and human unfortunately seems to arise from gaps in scholarship.

Continuity versus discontinuity theories of the evolution of human and animal minds

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At one time sharp, qualitative gaps seemed to separate ape and human minds. Specifically, humans were thought to be the only animals capable of symbolism, syntax, tool making, culture, and self-recognition. Each passing year, however, astonishes us with new findings of ape abilities, and we now know that apes possess the rudiments of all human behaviors. Evidently, ape-human cognitive distinctions are matters of degree, rather than of kind (Gibson 1991). In particular, increased levels of information processing capacity and hierarchical mental constructional skills may be the primary distinctions between human and ape language, social, and tool-using behaviors (Gibson 1983; 1988; 1990a; 1990c; Reynolds 1983). Greenfield's analysis of manual and communicative development provides an elegant demonstration of these hierarchically organized behaviors.

Similarly, investigators have often postulated that the human brain contains such unique neural structures as the hippocampus minor, Broca's area and the inferior parietal association area, but we now know that the rudiments of these structures can be found in the brains of nonhuman primates. The major neuroanatomical distinctions between ape and human are, like the cognitive distinctions, quantitative in nature. Thus, the

human brain and some of its component neural areas and tracts are larger than those of apes, and the ratio of neural connections to neurons is greater. Although these quantitative changes may well provide the neurological foundations for increased information processing and mental constructional skills (Gibson 1990a; 1990c), the notion survives that qualitative differences in brain function separate human and ape. In particular, investigators now postulate unique, genetically determined, neural modules for language, reciprocal altruism, and a host of other behaviors (Barkow 1989). [See also multiple book review of Barkow: *Darwin, Sex and Status: Biological Approaches to Mind and Culture*, *BBS* 14(2) 1991.]

How can the brain contain unique functional modules if it has no unique anatomical structures? How can human cognition depend on unique functional modules if the rudiments of human behaviors are present in the apes? Greenfield's target article provides a major clue: Modularity develops with experience. Much ongoing work supports her claim. Although ample evidence exists for distinct functional specializations in adult brains, no data indicate a genetic basis of neural modularity. Quite to the contrary, older theories that behavior is plastic but brain function is strictly biologically determined, are demonstrably wrong. Developmental interactions between brain and behavior occur in both directions (Gibson & Petersen 1991), and many variations in adult brain function are environmentally rather than genetically determined (Plomin & Ho 1991). Thus, similar mental modules may occupy different anatomical locations in human adults, and cortical areas specialized for auditory functions in hearing humans may develop visual functions in the congenitally deaf (Neville 1991). Moreover, individual neurons in the motor and somatosensory cortices may exhibit functional plasticity on a week-by-week basis depending on experience (Merzenich & Kaas 1982). If modularity reflects experiential input rather than predetermined anatomical specificity, then no conflict exists between modular theory and current evidence of behavioral and neural continuity from apes to humans.

Consequently, I believe that Greenfield's formulation provides a major breakthrough in our methods of approaching the evolution of the human brain. The concepts of hierarchical mental construction and developing neural modularity can also be applied to analyses of animal behavior. For example, much debate has occurred in this journal on the subject of animal intelligence. Are all species equally intelligent (Macphail 1987), or are some more intelligent than others (Parker & Gibson, 1979)? [See multiple book review of Jensen: *Bias in Mental Testing*, *BBS* 3(3) 1980; and Jensen: "The Nature of the Black-White Difference on Various Psychometric Tests: Spearman's Hypothesis" *BBS* 8(2) 1985.]

When phrased in terms of the much debated word "intelligence," the question may be unanswerable. When phrased in terms of hierarchical mental construction, however, the question is readily approachable, and distinct species differences are easily detected (Gibson 1990a; 1990b). In particular, such large brained species as dolphins, great apes, and Cebus monkeys possess greater hierarchical mental constructional skills than do those with smaller brains. Perhaps the cerebral cortex of all species is basically designed for mental constructional skills: The larger the cortex, the greater the skills? Hierarchical organization, however, can be applied to diverse behaviors. The precise application of these skills in diverse species may vary according to experience and cortical input from peripheral sensory and motor organs. [See also Glezer et al.: "Implications of the 'Initial Brain' Concept for Brain Evolution in Cetacea" *BBS* 11(1) 1988; and Falk: "Brain Evolution in *Homo*: The 'Radiator' Theory" *BBS* 13(2) 1990.]

Have your module and eat it too!

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Does human language emerge from more generalized cognitive processes or is it the product of a special neural module that largely determines its shape and development? This question creates the great divide between researchers who focus on language development and language processing. In positing that language and object manipulation have a common, undifferentiated origin in Broca's area and then differentiate into separate and distinct neural tracts, Greenfield has, if she is right, formed a rapprochement between two camps that speculate about language and its biological origins (see Pinker & Bloom 1990).

On the one hand are those theorists (Fodor, Chomsky) who believe that language is an impenetrable module of the mind whose contents are distinct from collateral cognitive skills or from functional communicative considerations. [See also Chomsky: "Rules and Representations" *BBS* 3(1) 1980; and multiple book review of Fodor: *The Modularity of Mind*, *BBS* 8(1) 1985.] On the other hand, theorists like Lieberman (1984; 1989a), Bates (1979), and Greenfield look for developmental homologies between other behaviors and emergent linguistic capacity. [See also Parker & Gibson: "A Developmental Model for the Evolution of Language and Intelligence in Early Hominids" *BBS* 3(3) 1979; and Chevalier-Skolnikoff: "Spontaneous Tool Use and Sensorimotor Intelligence in *Cebus* Compared with Other Monkeys and Apes" *BBS* 12(3) 1989.] Where Greenfield unites these two positions is in suggesting that there is a *shift during ontogenesis* from common neural bases for language and cognition to a unique linguistic module. This position presupposes, then, that a module for language exists. It reduces the difference between Greenfield and Fodor/Chomsky to the question of the origin of the module.

If language is the product of natural selection (see Pinker & Bloom 1990), the outline of Greenfield's argument is at least logically correct. That is, from an ontogenetic perspective, there is every reason to suspect that components of language may have neurological bases common to other behaviors not originally designed to serve language *per se*. Furthermore, common neural substrates would be expected to appear early rather than late in ontogeny as Greenfield has argued. This is because, as Darwin (1859/1964) pointed out, radical changes early in development would have too many effects on other developing bodily structures. Changes that occurred relatively later in evolution – such as a module for language – are seen later in ontogeny, as well (Studdert-Kennedy 1990). From a phylogenetic standpoint, although not even our closest infrahuman biological relatives appear to have or are able to learn a communication system with anything approaching the complexity of human language, work by Savage-Rumbaugh (Savage-Rumbaugh 1987) on a pygmy chimp who can comprehend *strings of oral* language is promising.

The question remains, however, whether the particular homology between hierarchical object structures and sound combinations that Greenfield wishes to argue for is correct. Whether these abilities are truly homologous depends on two things: the neuroanatomical data and cases of potential dissociation between object manipulation and language. We deal with each point in turn below.

First, the neuroanatomical evidence for Greenfield's theory must be found, as she is aware. A troublesome point, for example, where neuroanatomical data is essential, concerns the role of language comprehension. Greenfield's story on homologous origins for language and object manipulation rests on

language production, in particular, sound, and then word, combination. If Greenfield's is to be more than a theory of how the child combines sounds to produce *speech*, then she must show that data on precocious language comprehension can be incorporated in it. Evidence on precocious syntactic ability in language comprehension from our labs (see Hirsh-Pasek & Golinkoff 1991; e.g., sensitivity to word order by 17 months), forces Greenfield to posit an escape hatch to allow her explanation to go through. She claims separate (although overlapping) neural components for the processing as opposed to the production of speech (see sect. 6). No data concerning such a division exist yet in humans; if none are forthcoming, the theory reduces to homologous roots for the harnessing of motor programs that allow the child to produce speech as well as complex motor acts. In other words, the current theory could not stand.

Second, we must ask what positing a homology between language and hierarchical structures buys us, if, as Pinker and Bloom (1990) have pointed out, many human systems are hierarchical in nature. In addition, even if we accept the homology argument, Greenfield must seek out test cases where dissociations between object manipulation and language could be found and are not. (She attempts to explain such dissociations in sect. 2.3.). In any event, correlational arguments can never provide the smoking gun for arguments of homology, as Greenfield realizes. That is, one can find all sorts of physical and neuroanatomical correlates (such as increases in both shoe size and dendritic density) that are in no way homologous to language. One test case may be provided by the children with Williams syndrome studied by Bellugi et al. (1990). Williams children suffer from a poorly understood metabolic disorder that, among other things, causes them to be seriously retarded. Nonetheless, these children produce complex, syntactically advanced speech that far exceeds what would be predicted given their low IQs. If Greenfield is right, these children's object manipulation and sound combination skills should be correlated in the first three years of life. A dissociation between object manipulation and language would disconfirm her hypothesis. In addition, if Greenfield is right that language *becomes* a module after its initial flowering, the dramatic dissociation between language and intelligence in these children becomes understandable. Although their language module is "off and running," their serious retardation predicts that object manipulation skills should fail to develop adequately.

Whether Greenfield's particular search for homologies turns out to be correct is almost less important than the fact that the search for homologies has begun. An ironic parallel comes to mind: Skinner's view resulted in an impenetrable black-box psychology, treating speculations about the human mind as irrelevant. [See *BBS* special issue on the work of B. F. Skinner, *BBS* 7(4) 1984.] The Chomsky-Fodor species-specific language module has had a similar effect: Speculation about homologies to language – either phylogenetic or ontogenetic – were also often treated as irrelevant. Greenfield's target article (and Pinker & Bloom, 1990, and prior work on speech perception) help open up the module, just as Chomsky helped unwrap (and ultimately discard) the black box. Only syntax, the *sine qua non* of language, remains in that module now. On Greenfield's argument, the module is not present from the outset of ontogenesis but is itself the result of neural differentiation. Before we can "have our module and eat it too," however, Greenfield's hypotheses must be subjected to a number of critical tests.

Gestures, persons and communication: Sociocognitive factors in the development and evolution of linguistic abilities

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Despite the multiplicity of facts taken into account by Greenfield's hypothesis, some aspects of development that could be relevant to the topic at hand seem to be missing from her remarkable account. One of them has to do with early social cognition and communication.

Several authors have identified an important turning point in infant development that seems to occur at around 9 months of age: an ability to combine *things*. It consists of the appearance of the infant's ability to coordinate objects and people in the same behavioral act (Schaffer 1984). Prior to 9 months of age, human infants *either* manipulate objects *or* engage in interactions with adults; only after 9 months are they able to introduce objects into their social interactions (e.g., handing an object to the mother or requesting a toy by means of pointing [Schaffer 1984]). This ability has been related to Stage V Piagetian sensorimotor development and therefore to the emergence of simple tool use (Bates & Snyder 1987). It has also been suggested that the interactive and communicative skills related to this transition might be a basis for early language development (see Bruner, 1975, for a classical statement of this hypothesis). A similar transition has been reported in gorillas (Gómez 1990) and chimpanzees (Gómez 1988). All these facts and hypotheses make it worth considering the phenomenon in relation to the theory proposed by Greenfield.

1. Object-person combinations. The emergent ability of infants to coordinate persons and objects at about 9 months has been identified and interpreted by several authors. Trevarthen and Hubley (1978) call it "secondary intersubjectivity" and suggest that it reflects an endogenous developmental process related to socioemotional development. Bates et al. (1975) consider it a mark of the emergence of prelinguistic "intentional communication." Sugarman (1973) speaks of the coordination of previously separated person and object schemes. Schaffer (1984) summarizes the various findings establishing an "object-person integration" stage of sociointeractive development that occurs from 8 months on. Many of the above authors have tried to relate it to later developments, notably the emergence of language and other symbolic skills (Bates & Snyder 1987). They have also tried to relate it to concurrent cognitive developments, among them tool-using abilities.

2. Tool use and intentional communication before 12 months. Some authors have noted a certain chronological coincidence between the emergence of tool-using abilities¹ and the earliest gestures addressed to other people to communicate about objects (Bates et al. 1975; Harding 1982; Sugarman 1973). There has also seemed to be a logical parallelism between "getting an object with another object" and "getting an object through a person." It has been suggested that both actions could perhaps be based on a partially common sensorimotor understanding of the world. The idea of homology (qualified as "local homology") is even used by Bates (1979) to refer to the relations between intentional communication and other prelinguistic abilities. Case (1985) considers object-person combinations to reflect a more general pattern of cognitive growth, also reflected in object manipulations. He establishes parallel developmental stages between "control structures for interacting with inanimate objects" and "control structures for interacting with other human beings." Object-person coordinations correspond to a developmental substage of *bifocal coordination*, which, according to Case, appears at 8–12 months both in the inanimate and animate domains. Case's hypothesized parallelisms of development extend to vocal and verbal development.

Empirical studies have shown, however, that the purported relationship between early tool-using and communicative abilities is problematic when actually measured: Some studies found no relationship at all (Sarriá 1989; Martinsen & von Tetzchner 1988); the relationship found by others turned out to be rather weak in correlational terms (Bates et al. 1979). Furthermore, early communicative acts seem to correlate best with such sociocognitive abilities as imitation or symbolic play (Bates et al. 1979; Sarriá 1989). So, although there is a "logical" similarity and a rough chronological coincidence between early tool use and prelinguistic communication, it is uncertain to what extent they can be said to share the same underlying cognitive abilities. A similar conclusion is suggested by Gómez's data on gorilla development (Gómez 1990): At around 10 months, his subject (a hand-reared gorilla baby) developed the ability to combine objects in simple tool-using behaviors (such as dragging a box under a goal); soon she exhibited completely isomorphic behaviors on human adults (e.g., dragging a human with full force under a goal bar and climbing on him to reach it); but it took her about 6 additional months to develop a strategy with humans that showed the features of prelinguistic communication (e.g., requesting a human to go under the goal).

These results suggest that even if prelinguistic communication shares some basic abilities with tool use (perhaps what Case, 1985, calls "bifocal coordination"), it also requires other abilities that seem to be specific to social interaction. To develop communicative gestures, it is not enough to be able to coordinate two things; these require something else (perhaps a "person concept" [Gómez 1991] or some other social-cognitive ability). Indeed, the correlational studies that failed to find important connections between communication and tool use did find high correlations with such social variables as imitation or symbolic play (Bates et al. 1979; Sarriá 1989). What does the logical and chronological parallelism between early tool use and communication reflect then? Is there a general ability (say, "bifocal coordination") that acts as a prerequisite for different capacities (e.g., tool use and intentional communication) whose developmental profiles, however, remain specific (uncorrelated)? Case (1985) does speak of a *parallel but independent* development of the skills involved in social and inanimate domains. Does the bifocal structuring of object manipulations and social interactions at 9 months reflect a partially common underlying machinery applied to different domains (homology) or similar responses from different machineries to similar constraints (analogy)? The problem with respect to prelinguistic behaviors at 9 months seems similar to the one addressed by Greenfield in the target article concerning protolinguistic behaviors after 12 months. But the phenomena analysed in the target article are related to the *syntactic* aspects of language, whereas those pointed out by us have rather to do with the *pragmatic* aspects of language.

3. The Machiavellian intelligence hypothesis. Greenfield (sect. 7.1) discards one of the three possible evolutionary scenarios – protolinguistic combinations' neural substrate as a preadaptation for manual object combinations – arguing that no theorist espouses it and that there is evidence against it. If one states this hypothesis in *prelinguistic* rather than *protolinguistic* terms (i.e., taking into account the early communicative behaviors to which we have been referring), then one can find some putative proponents of it. Roughly summarized, the Machiavellian intelligence hypothesis (Byrne & Whiten 1988; Humphrey 1976) states that in phylogeny the ability for means-end coordinations in the social domain (manipulating other people's behavior) may have preceded means-ends manipulations of objects; in its strong version it would even claim that manipulative intelligence grew out of social intelligence. Concerning supportive evidence for this hypothesis, let me point out as an example the phenotypic mystery of why captive gorillas appear to use tools in much the same way chimpanzees do (Gómez, personal observations), whereas they show no evidence of tool use in the wild. A

hypothesis of Machiavellian inspiration would propose that intelligent abilities evolved to deal with complex social interactions but then give rise to intelligent manipulations of objects in the environment of captivity, where the latter happen to be adaptive (cf. Humphrey 1976). As Greenfield points out, there is also counterevidence: My own results with a gorilla point to earlier ontogenetic development of manipulation compared to communication (Gómez 1990). All in all, available theories and evidence seem to make the third scenario worth discussing further.

NOTE

1. These abilities range from the "support behavior" (pulling a cloth on which the desired object is lying) to the use of a stick to attract an object. Some authors also refer to Piagetian Stage-V understanding of objective causality (Bates 1976; Harding 1982; Sugarman 1973). This might explain the apparent contradiction between the presence of tool-using behaviors at 9 months and Greenfield's reference to 11–12 months as the age at which earliest tool-use occurs (sect. 3.3). The support behavior appears earlier than more complex instances of tool use, such as the use of a spoon as an intermediary (Piaget 1936).

Planning and the brain

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The focus of Greenfield's target article is the relationship between language and object use. She tries to demonstrate that these functions are merely specific examples of all organized sequential behaviors that probably arise from a common neurological source. The one she identifies is the area of the inferior and posterior prefrontal cortex. We concentrate our remarks on her brief mention of the relationship of plans to organize such sequential knowledge as syntax and the role of prefrontal cortex as the storage site of plans and similar knowledge.

A few preliminary comments about Greenfield's notion of linguistic processes are necessary before we address the issue of planning. Greenfield makes certain assumptions about language representation that ignore the current Zeitgeist regarding the complexity and number of computational processes in linguistic processing (Caplan 1987). She simplifies the cognitive picture to make a general point about Broca's and adjacent cortical areas in the frontal lobes. Mapping specific representations onto cortical regions is risky unless the level of description is commensurate (Olson & Caramazza 1991). In this context, it is unclear what level of homology will be adequate; for example, why shouldn't we focus on homologous origins at the level of the gene? No level of homology is absolute; all are just relative.

Greenfield assumes that plans are stored in the prefrontal cortex and are an outgrowth of, or correspond to, simpler linguistic and praxic mechanisms. She asserts this without providing any evidence for the architecture of plans as representations, that is, memories. For example, some authors would claim that plans are not represented in the prefrontal cortex; rather the prefrontal cortex is involved in working memory processes that maintain knowledge stored in posterior cortex in an active state (Goldman-Rakic 1987a; Moscovitch 1982). If this hypothesis is supported, then the idea that plans are analogous to Greenfield's syntactic or praxic knowledge can be rejected purely on the grounds of differences in representational architecture. On the other hand, if plans were hierarchical and componential and perhaps categorically represented, then that architecture might reinforce Greenfield's notion of increasing sequential representation in the prefrontal cortex. Even then,

however, the reasons for the evolution of sequential representational knowledge may differ from the one she offered.

Both Grafman (1989) and Hendler (1987) have developed models of hierarchical representations for encoding planning knowledge (semantic and episodic) that have implications for the role of planning in cognitive functions. Grafman coined the term "managerial knowledge units (MKUs)" for his representation; Hendler used the more traditional artificial intelligence (AI) terminology of "frames." In both models, this level of representational knowledge was thought to emerge in combination from more primitive representational levels linking the planning knowledge to "lower-level" representations including rules, procedures, and perceptual operations.

One critical aspect of the models we have proposed concerns the role of time in the ability to reason about the world. The planning knowledge must be available during the activity it guides. Thus, a high-level planning template would need to be activated during such an activity as eating at a restaurant. On the other hand, the knowledge that guides the recognition of a shape or controls a motor action such as lifting a menu may be activated for only milliseconds. In this view, it is not sequential activity per se that determines the nature of certain representations and their neurological origins, but the time required to perform the activity, whether it be relatively simple (form recognition) or complex (eating at a restaurant).

This view of the centrality of temporal factors and the longlasting activation of planning knowledge has important consequences for our view of human planning (and the processing in the human prefrontal cortex). In particular, we believe that the duration of representational activation has an effect not only on representational architectures but also on attention, and hence on conscious awareness (cf. Marcel & Bisiach 1988). Working memory allows for a temporal window of 10 seconds or less within which only certain representational knowledge may completely "fit" (Baddeley & Wilson 1988). As the duration of a representational activity exceeds the window of working memory (e.g., in the case of eating at a restaurant, which may take an hour or more), it represents simultaneously the present (eating a main course), the past (looking at a menu), and the future (paying the bill). Thus, an ability to maintain the activation of the higher level memory units translates, roughly speaking, into an ability to reason causally about future events.

The ability to project the results of current actions into the future and to reason about the time course and duration of the resulting events is not a minor development – it is crucial for long term planning. Thus, this ability to represent in the same memory unit an activity and its many consequences and precursors may indeed be a major evolutionary step. In addition, increasing the role of time in memory representation greatly increases the repertoire of actions that can be represented in the brain. In this view, the ability to communicate consequence and precedence through language or gesture merely perfected the communication of the details of action.

Patients with prefrontal or subcortical lesions often perform normally on tests of intellectual achievement, immediate memory, perception, and spatial cognition (Anderson et al. 1988). Anecdotal observations suggest that there are at least two major categorical deficits in these patients: impairment of social-cognitive activity and impairment of abstract and concrete plans and the carrying out of mental sets (Stuss & Benson 1984). We consider the essential element in both cases to be the planning knowledge units discussed previously. Unlike other theorists who suggest that the prefrontal region is primarily involved in keeping representational knowledge stored in other regions of cortex active over a delay (Goldman-Rakic 1987a), we believe that planning knowledge is stored in the prefrontal cortex in the form of a relational and categorical architecture.

We have recently begun a joint project aimed at developing a model of such an architecture. The model assumes that the relations between planning knowledge stored in categories is

similar to that between lexical items stored in a category (Caramazza & Miceli 1991). The memory for plans is organized on the basis of centrality to the category, frequency of activation of that planning knowledge, association to other plans, and so on. The goal of the research is to develop this model carefully enough to generate predictions about how planning knowledge can be activated and what sorts of failures can be expected. Both psychological testing and computational simulation will be used to predict (and compare) behavior in normal patients and those with prefrontal lobe lesions. Such a testable model, even if it eventually fails, is preferable to the standard prefrontal deficit explanation that posits either an executive homunculus or a "type of working memory" (Stuss & Benson 1984).

Those who model psychological processes computationally (primarily the AI community) have argued for a parsimonious representational scheme for both natural language understanding and human planning behaviors (see Hendler, 1987, for a discussion of related work). In particular, it seems odd to argue that the knowledge for recognizing the plans being used by agents in stories is significantly different from the knowledge used in deciding one's own actions. The sort of long-term activation that is a dominant factor in planning also appears to be an important part of complex language understanding (e.g., the ability to read a novel). It accordingly makes more sense to expect a parallel emergence of behaviors concerned with action in, or knowledge of, complex routines, stimulated by different environmental and genetic demands (e.g., planning, language comprehension, tool use, and conveyance of consequences).

If you've got it, why not flaunt it? Monkeys with Broca's area but no syntactical structure to their vocal utterances

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In this commentary, I take issue with Greenfield's claim that data on tool use and sign language in chimpanzees can be used to buttress the hypothesis that the development of Broca's area can be traced to our common ancestors. The data accumulated by Greenfield concerning Broca's area strongly suggest that there are at least two distinct regions, one subserving object manipulation and the other speech production. There is no doubt in my mind that nonhuman primates in general (i.e., not just chimpanzees) are capable of complex object manipulation involving combinatorial manipulations. Much of the existing data, moreover, provide support for the MacNeilage et al. (1987) hypothesis that handedness and hemispheric laterality evolved from our primate relatives (e.g., Hauser et al. 1991). What must be seriously challenged, however, are the data on combinatorial arrangements of tokens in the "language" of nonhuman primates and the basis for such abilities. This challenge breaks down into four separate but closely related concerns.

First, since Broca's area has been primarily discussed in terms of its role in vocal production, is it appropriate to examine a nonvocal language, especially one that is not the species' natural language? In other words, why hasn't Greenfield discussed the effects of damage to Broca's area with respect to the deaf and their use of sign language, and in particular, the developmental timing of injury and the magnitude of the effect? And why has so little attention been paid to the natural vocal systems of nonhuman primates?

Second, data on the neuroanatomy of Broca's area in nonhumans comes from the genus *Macaca* and yet the discussion of tool use and language is primarily based on data from the genus

Pan. It seems to me that one must question the validity of the leap from *Macaca* to *Pan*.

Third, as every reader of *BBS* will know, the ape "language" studies have been criticized primarily because of the lack of evidence of syntax. [See *BBS* special issue on Cognition and Consciousness in Nonhuman Species, *BBS* 1(4) 1978.] The new data on bonobos (*Pan paniscus*) involving the combination of two tokens to produce structures with new meaning is intriguing, but comes after a long history of failed attempts. Because of the newness of this data, it has not yet met the kind of critical treatment showered on earlier work.

Fourth, data on the natural vocalizations of nonhuman primates have failed to reveal evidence of syntax, *sensu strictu*. Therefore, if Broca's area is present, why do nonhuman primates seem incapable of using it? I restrict my discussion to this last issue, because there are some data, not mentioned by Greenfield, that speak to the possibility of "protolinguistic combination" (sect. 7.1).

Ethologists have looked for evidence of syntax in the natural vocalizations of nonhuman animals but have generally failed to find convincing observations. Nonhuman primates and at least one avian species (Hailman et al. 1987), however, combine discrete utterances into sequences that appear to carry new meaning. These combinatorial manipulations of the signal system led Marler (1977) to make a distinction between "phonological" and "lexical syntax." In phonological syntax, discrete sounds are combined into new sequences in much the same way as phonemes are rearranged to form new words. Lexical syntax, in contrast, is analogous to a human sentence, where different sounds are combined into phrase-like structures and meaning emerges from the sum of the individual components. To date, there is no convincing evidence of lexical syntax, whereas there are several examples of phonological syntax. For example, Robinson (1984) has shown that capuchin monkeys (*Cebus olivaceus*) use rules to combine and recombine sequences of acoustically discrete utterances, in an apparently open-ended fashion. Although this system might satisfy the conditions for lexical syntax, we should take an agnostic position until careful studies of meaning and reference have been conducted on the discrete and phrase-like utterances produced (see Cheney & Seyfarth, 1990, for further discussion of this issue).

If nonhuman primates have Broca's area, why isn't it being used to generate syntactically structured vocal utterances? The first issue raised by this question is whether it is reasonable to use the label "Broca's area" when the function of this region in vocal communication seems remote at best. In support of this concern, I refer Greenfield to a series of results from lesioning studies in squirrel monkeys and macaques (reviewed in Jurgens 1990) that have shown that the removal of Broca's area has no detectable effect on call structure or call rate. In the species examined, the limbic system is the primary neuroanatomical structure involved in vocal production. Therefore, perhaps we should adopt a philosophical technique and call the human structure "Broca's area" and the nonhuman primate structure, located in a spatially analogous location "Broca's area*."

The second point I make accepts as its starting point Greenfield's conclusion that Broca's area is an all-purpose structure for combinatorial manipulations. Although we have no evidence of syntax in nonhuman primates, there is quite good evidence of complex tool use. Could it be, therefore, that Broca's area in nonhuman primates is dormant with respect to vocal production, but alive with respect to object manipulation? If this scenario is correct, then one would expect lesions of Broca's area to have an effect on object manipulation. Unfortunately, there are no available data to answer this question.

Last, if Broca's area is linked to combinatorial manipulations, what were the selective forces involved in creating a subdivided and potentially modular Broca's area? I certainly do not have any good answers to these questions, but would be very interested in what Greenfield has to say.

Neurobiology and language acquisition: Continuity and identity

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Greenfield's article is a logical extension of her own acquisition research and Pinker and Bloom's (1990) thoughtful (though somewhat theoretically biased) *BBS* target article, which proposed that "language" (specifically, universal grammar) evolved through natural selection as opposed to being a saltatory evolutionary anomaly. Although some acquisition researchers, especially those working within a formal theoretical linguistic framework, assert that human language is more appropriately studied in terms of neurobiology than in terms of culture or behavior (e.g., Chomsky 1986; Pinker 1989), far too often the same researchers are content to make neurobiological claims without neurobiological evidence. These default claims are perceived to be necessary, in part, because it is difficult to investigate the child's developing syntactic competence before the age of two and, perhaps more important, because the specific neurobiological consequences of the input (linguistic and contextual) a child receives during development is largely unknown. These obstacles have led many language acquisition researchers without a brain-based perspective to posit an innate language acquisition device. Although Greenfield addresses the question of innateness only indirectly in her treatment of modularity and homology, she does bring a much needed developmental and neurobiological perspective to language acquisition. In fact, she consistently (and, I believe, correctly) turns to the brain in order to support homology, an issue that cannot be resolved realistically by behavioral, intuitive, or philosophical arguments. One can indeed anticipate that most objections to her position will originate from researchers without a neuroscientific background. At the very least, she has woven considerable neurobiological evidence into several plausible hypotheses that provide a rich background for future research.

Although the incorporation of neurobiology is one of her target article's major strengths, some of the neurobiological research, especially Simonds and Scheibel (1989), requires qualification, clarification, and/or supplementation:

(1) Interindividual and gender variation. It is well accepted among neurobiologists, especially those working with human tissue, that the brain is characterized by tremendous interindividual variation. This is no less true for Simonds and Scheibel's quantitative dendritic study which, although it included 17 subjects, had only 2–4 individuals in each of 5 age groups spread over an age range of 6 years. Furthermore, Simonds and Scheibel were not able to control for possible gender differences in dendritic expression. Such interindividual differences were documented recently by Jacobs (1991) who, in a quantitative dendritic study of Wernicke's area in adults, found (1) a 40% difference between highest and lowest measures of total dendritic length, and (2) considerable and consistent gender differences in dendritic measures between males ($n = 10$) and females ($n = 10$). These possible shortcomings, fully acknowledged in the original Simonds and Scheibel article, should be considered here, and also in interpreting other neurobiological studies mentioned in the target article.

(2) Dendritic systems and connectivity. For clarification purposes, dendritic growth in Simonds and Scheibel was measured both in terms of dendritic length and segment number: the greater the overall length, the greater the receptive surface of the dendritic ensemble and, theoretically, the greater its computational capacity. Because more distal dendritic segments develop later ontogenetically, one can also roughly infer the relative maturational characteristics of different cortical areas. It should also be noted that more distal segments appear to be

preferentially sensitive to epigenetic factors. Although Greenfield notes that the dendritic studies do not provide specific information "on exactly where the other termini of the circuit or networks are located" (sect. 4), she claims that "as long-distance connections develop, connections with neighboring cells and areas decrease" (sect. 4). This appears to be an unsupported generalization. Also, it is not exactly clear what is intended by "short" versus "long" range connections; Thatcher et al. (1987) claimed that short-distance fibers were perhaps 6.65 cm in length (the average interelectrode spacing), which is, at best, a vague approximation.

The relative complexity of the dendritic systems reflects the cells' connectivity not only with "more distant regions of the brain" (sect. 4), as Greenfield claims, but also with extensive local intracortical circuits and subcortical connections (as mentioned in note 5). Similarly, she suggests that the dendritic measures in the 24–36 month group demonstrate that "Broca's area is now receiving more distant inputs" (sect. 4). It is unclear how one can know where the input originates without axonal tracing; apparent changes in intracortical connectivity may also be the result of increasing myelination with concomitant enhancement of neural efficiency. Finally, with regard to her statement that dendritic growth may "link up motor cortex with Broca's area" (sect. 4), it should be noted that dendritic growth itself does not form the connections; rather, it merely *reflects* the relative degree of connectivity.

Although these are important neurobiological issues, they do not detract significantly from Greenfield's overall hypotheses, which are supported by a considerable variety of neurobiological studies. Nevertheless, what each investigative technique reveals about the brain, and the degree to which these can be complementarily synthesized (as Greenfield has done) remains an open question.

On a different topic, much of the target article focuses on nonhuman primates and their productive abilities. Greenfield is sensitive to a variety of important issues here, ranging from the necessity of considering developmental data to the relevance of an animal's natural communicative system and comprehension abilities. The relationship between comprehension and production is an interesting one. This is especially true because dolphins (*Tursiops truncatus*), which diverged from the hominid line approximately 30 million years ago, have been shown to display remarkable abilities in interpreting both acoustical and gestural versions of highly abstract, rule-governed artificial languages with both linear and inverse grammars (Herman et al. 1984; Herman 1986; 1987). [See also Glezer et al.: "Implications of the 'Initial Brain' Concept for Brain Evolution in Cetacea" *BBS* 11(1) 1988.] This includes the ability to comprehend rather complex, lexically and structurally novel sentences, as well as conjoined sentences, relational sentences, and sentences in which different word orders produce changes in meaning, such as the following (from Herman 1986, p. 231):

LEFT HOOP LEFT BASKET IN (= Place the left hoop in the left basket).

This sentence certainly seems to be as hierarchically complex as those presented for the subassembly method. Future research on homology might indeed investigate the comprehension side of language acquisition.

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Constructivism without tears

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We feel encouraged by three interconnecting threads in Greenfield's target article: (1) the integration of biological considerations into developmental theorizing; (2) the focus on output systems rather than solely on input systems; and (3) the consideration of modularity as a product of development rather than as an innately specified structure.

Let us briefly consider each of these in turn. Recently, Johnson and Morton (1991) have proposed that it is essential to seek information from two biological sources for an adequate account of certain aspects of cognitive development: evidence from the postnatal growth of the brain, on the one hand, and evidence pertaining to other species and the external environment of the animal, on the other. This analysis was applied to the development of face recognition during infancy, whereas in the target article Greenfield applies a superficially similar analysis to language acquisition and tool use. Johnson and Morton, however, challenge the use of evidence from biology simply to bolster an existing cognitive account. Rather, the new cognitive-level constructs should be deeply rooted in the biological level from which they are derived. Indeed, the cognitive mechanisms that arise from such an analysis often look very different from those originally derived from purely cognitive concerns.

Turning now to the second issue, most recent theorising on cognitive development, especially in infancy, has focused more on input than on output systems. For example, with few exceptions, research on language acquisition in early infancy has focused on perception (e.g., Jusczyk 1986; Mehler et al. 1988) rather than on production (e.g., various parameters of babbling). This focus has resulted in the widespread acceptance that the infant mind is a series of prespecified input modules. Ethological and neural considerations, however, automatically lead one to shift the focus toward output systems, because there is a tighter mapping between output systems and adaptive behavior in the external environment. This shift in focus – suggested also by the target article – will, in our view, lead to a richer, epigenetic view of development that will encompass both modularity and a process of modularization.

This leads to the third point. Elsewhere we have argued that modularity should be seen as a product of development, that is, that the initial state of the infant mind gives rise to a gradual process of modularization and does not merely contain innately specified modules (Johnson & Karmiloff-Smith, in press; Karmiloff-Smith 1986; 1991). Moreover, even late-developing capacities can be shown to display properties of modular organization suggesting an ontogenetic process of progressive modularization rather than a steady state from the outset. For example, studies of both use and metalinguistic awareness of sentence-level and discourse-level properties of linguistic markers suggest that metalinguistic awareness of sentence-level properties increases with development. By contrast, awareness of discourse-level properties of the same linguistic markers seems to involve some limited access during middle childhood, after which it disappears by adulthood, suggesting that it has become encapsulated (Karmiloff-Smith et al. 1991).

Aside from these three general issues, which we welcome, certain details of Greenfield's arguments seem to us to be flawed. First, at the level of abstraction invoked by Greenfield, hierarchical structures are common across many aspects of cognitive and behavioural development. Indeed, it has been argued that this general pattern reflects the ontogeny of many aspects of vertebrate brain structure (Johnson 1990; 1991). Hence hierarchical organization will not allow us to group language and tool use and differentiate these from other aspects

of cognition and skill learning, because the latter can also be decomposed hierarchically. Second, it is essential to differentiate between hierarchical tree structures and hierarchies of control. We know, for instance, that children with Williams syndrome develop very elaborate linguistic structures in the face of otherwise serious cognitive deficit (Bellugi et al. 1988). Yet it is not at all clear that, outside their linguistic capacities, such subjects have the hierarchical structures invoked by Greenfield in tool use.

We are nonetheless heartened by the apparent renaissance of epigenetic views in cognitive development. Although Piaget's domain-neutral stage model of development has clearly proved inadequate, his general epigenetic constructivism is regaining scientific popularity (see discussion in Karmiloff-Smith 1991). We suspect that this shift in approach will continue as a matter of necessity as evidence from neural development becomes increasingly intertwined with cognitive-level theorising. Indeed, although aspects of brain development may follow a maturational time course (Johnson 1990; 1991), gene expression may be regulated not only by the internal biochemical environment, but also by interaction with the organism's external environment.

NOTE

It is a particular pleasure for us to reply to a target article dedicated to the memory of our colleague at the MRC Cognitive Development Unit, Richard Cromer.

Speech and brain evolution

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I am, perhaps predictably, quite pleased to see the data that Greenfield presents. The brain mechanisms for automatized speech motor control have always seemed to me to be among the key elements of human linguistic ability (Lieberman 1975, pp. 6–16; 1984, pp. 57–78). Humans are clearly unique in their ability to produce voluntary speech and to use complex syntax. Moreover, speech and syntax deficits are linked together in the classical neurophysiological literature on the deficits of Broca's aphasia. It was therefore reasonable to propose, as I did, that the brain mechanisms that regulate the complex motor control sequences necessary for human speech were the immediate preadaptive basis for syntax in the last stage of hominid evolution that yielded anatomically modern *Homo sapiens* (Lieberman 1989b; 1991). The behavioral data that Greenfield and her colleagues have gathered and the recent neurophysiological data she reviews clearly support this view.

The Darwinian mechanism of preadaptation is often misunderstood. As Darwin (1859/1964) noted, the preadaptive basis for the human lungs is the swim bladder. Our lungs are not swim bladders, however; Adaptations that facilitate respiration have modified the ancient swim bladder morphology, although the evolutionary history is apparent when one considers such factors as the role of the elastic recoil of the lungs during speech production (Lieberman 1967; 1984). The neurophysiological data Greenfield reviews are likewise consistent with preadaptation's playing a major role in the evolution of the aspects of Broca's area controlling speech and syntax. The developmental sequence and geometry of specialized areas probably reflects the morphology of the motor strip, which itself undoubtedly played a part in the evolution of this particular part of the brain for speech motor control. In other words, it is no accident that the parts of Broca's area that regulate syntax are adjacent to ones active in speech motor control.

A similar situation may exist for the subcortical circuits that

connect Broca's area to anterior prefrontal regions. Though Greenfield notes that circuits connecting Broca's area and prefrontal cortex play a part in regulating speech and syntax, she does not discuss subcortical pathways. (This is not to be construed as a "fault," because developmental neurophysiological data on subcortical pathways are scarce.) It is apparent that subcortical pathways connecting Broca's area to prefrontal cortex play a crucial role in regulating both speech and syntax. The traditional view of Broca's aphasia is that damage localized to this particular part of the neocortex will result in these deficits, whereas damage to any other part of the brain won't. This belief is reflected in the supposition of many linguists that humans have a specific, localized "language organ," "module," or "modules" (Chomsky 1975; 1976; 1980b; 1986; Fodor 1983). [See also Chomsky: "Rules and Representations" *BBS* 3(1) 1980; and multiple book review of Fodor: *The Modularity of Mind*, *BBS* 8(1) 1985.] That supposition, however, is incorrect. Permanent aphasia appears to occur only when the subcortical pathways connecting Broca's area to the prefrontal cortex are interrupted by massive tissue destruction (Stuss & Benson 1986). In fact, subcortical damage that disrupts the connections from Broca's area, *leaving it intact*, can result in aphasia (Alexander et al. 1978; Benson & Geschwind 1985, pp. 206–07; Metter et al. 1989; Naeser et al. 1982).

The damage pattern that yields Broca's aphasia clearly involves interruption of pathways to anterior prefrontal cortex. Metter et al. (1987; 1989), for example, note hypometabolism in prefrontal cortex for Broca's aphasia and conclude that the behavioral deficits of Broca's aphasia – general "difficulty in motor sequencing and executing motor speech tasks," as well as "language comprehension abnormalities," derive from damage to neural pathways to prefrontal cortex. CT scan data show damage to subcortical areas including putamen, caudate nucleus, and the internal capsule (Baum et al. 1990). Damage to the internal capsule may interrupt pathways from caudate nucleus to globus pallidus, which pass through the internal capsule, as well as pathways to cortex from thalamus (Parent 1986). These subcortical lesions could interrupt either or both of the independent "sensorimotoric" and associative basal ganglion pathways that connect cortex, putamen, globus pallidus, substantia nigra, thalamus, and cortex (DeLong et al. 1983; Parent 1986). The presence of two independent, physically proximate, subcortical pathways again derives from their common evolutionary history (Parent 1986). Impairment of these pathways may also explain the variable pattern of manual, speech, syntax, and cognitive deficits observed in Broca's aphasia.

Parkinson's disease (PD), which causes major damage to the basal ganglia while sparing cortex, can also result in deficits in speech production and syntax. PD subjects who have moderate motor deficits may also have difficulty producing (Illes et al. 1988) and comprehending (Lieberman et al. 1990) sentences that have complex syntax. Recent data (Lieberman 1991; Lieberman et al., submitted) show voice-onset-time, VOT, disruptions similar in kind to those noted for Broca's aphasics in PD subjects having moderate limbic motor deficits. In a study of 40 PD subjects the VOTs of 9 subjects overlapped 18.3% for "voiced" and "unvoiced" stop consonants in syllable initial position. At similar syllable durations (300–500 msec), 3.6% of VOTs overlap for normal subjects. The VOT overlap PD subjects had significantly longer response times and error rates in a syntax comprehension test. They also performed significantly worse than mild PD subjects on cognitive tests that involve concept shifting. Deficits in syntactic comprehension, however, did not always occur for subjects whose VOTs overlapped. Neurophysiological data again indicate that the speech production syntax and cognitive deficits of PD may result from damage to subcortical circuits to prefrontal cortex; positron emission tomography (PET) studies show prefrontal hypometabolism for PD (Metter et al. 1984; Metter et al. 1987).

This brings me to the question of why apes cannot talk, and to

evolutionary schemata for human language. Greenfield's developmental data show a linkage between speech production and the capacity to combine objects manually – in human children. Apes (including *Pan paniscus*) lacking functional neocortical vocal control, however, cannot produce the voluntary muscular maneuvers that are necessary to produce human speech (Sutton & Jurgens 1988). Apes also lack the specialized supralaryngeal vocal tract that is necessary to produce the full range of human speech sounds (Lieberman 1968; 1975; 1984), but it is impossible to train them to produce the many sounds that their anatomy could produce. As Goodall (1986) notes, they cannot produce voluntary vocalizations independently from affect. It is therefore apparent that the development of the neural mechanisms regulating voluntary speech was a necessary step in the evolutionary process that resulted in modern *Homo sapiens*. In all likelihood, specialized subcortical pathways were involved as well as cortical specializations.

The first stage of the evolutionary sequence I would propose (Lieberman 1989b; 1991), like the models proposed by Greenfield in the target article, Kimura (1979), and MacNeilage (1987), involves adaptation for lateralized manual motor control mechanisms. Right-handed people, who constitute about 90% of the population, consistently use their right hands when they have to perform precise manipulations. The dominant hemisphere of the brain also controls the production of speech. This view is not novel; it is developed in detail by Kimura (1979) who herself built on observations that date back to Liepmann's (1908) studies of the "akinesic" deficits in motor control that occur with damage to the brain. The second evolutionary stage would involve the voluntary control of speech. The initial contribution of vocal communication to biological fitness undoubtedly derives from the fact that it frees the hands. Because the earliest hominids could walk upright, their hands were free to use tools and carry burdens. Although manual sign language is an effective means of communication, it does involve using one's hands. Tool-using hominids would have enjoyed an advantage in most situations if they were capable of using vocal communication. Vocal communication is also effective when the viewer is not visible. A change in brain organization that allowed voluntary control of vocalization is the minimum condition for vocal communication. Broca's area allows humans to access the automatized motor subroutines that are necessary to produce the sounds of speech. The third stage would, as Greenfield proposes, be the development of syntactic ability.

The concept of "modularity," although intriguing, probably has more relevance to digital computers or radar than to the brains of living animals. The human brain is not different in its overall architecture from the brains of other hominids; it obviously has many specialized devices, but those devices are generally involved in different aspects of behavior. As Mesulam (1985) notes, a given neural structure may participate in very different activities involving different circuits. Darwinian evolution inherently involves new behaviors arising from the modification of brain mechanisms that initially functioned in some other activity. These mechanisms, though modified, however, may continue to work to effect their "old" as well as their new functions. Broca's area, in my view, is a multifunction organ adapted to the regulation of sequential activity in several different domains that reflect its evolutionary history. It appears to function as Kimura (1979) proposed, in precise sequential hand maneuvers. The lateralized brain mechanisms that were initially adopted for precise sequential manual control generalized to control the nearby orofacial motor control areas of the primate brain. A later change involved using these brain mechanisms (which accessed complex motor control "rules") for the sequential rules of syntax. These different functions all appear to depend on subcortical pathways linking Broca's area with prefrontal cortex.

And Broca's area appears to be useless in itself without the enlarged hominid prefrontal cortex (Deacon 1988; 1990c) and

connecting subcortical pathways. The prefrontal cortex is our general purpose fine motor control and thinking machine. It is at once our “think tank” and fine motor control sequencer. It enhances motor control, the learning of new tasks, the formation of abstract concepts (Markowitsch 1988; Stuss & Benson 1986). It enters into all these activities and language.

When did the modern human brain evolve? Greenfield’s data stressing the reorganization of the human brain for speech production raise the question of when this process occurred. Studies of the evolution of the human supralaryngeal vocal tract, that is, the upper part of the airway from the lungs, are relevant. As far as the specific properties of human speech are concerned, the important parts of the supralaryngeal vocal tract are the pharynx and oral and nasal cavities. The soft palate, which can close off the nose to the mouth, the tongue, lips, and larynx (which can move upward or downward) work together to change the shape of the supralaryngeal vocal tract. A series of changes have occurred in the last 200,000 years or so, that adapted the supralaryngeal vocal tract for speech production at the expense of vegetative functions like respiration and the prevention of choking to death on food (Lieberman 1968; 1975; 1984; 1989b; 1991). The presence of a modern supralaryngeal vocal tract is thus an index for the presence of a modern brain – one that allows the production of speech. The human supralaryngeal tract is maladapted for swallowing; it enhances the possibility of death from impacted molar teeth. Its only contribution to biological fitness is speech, but that is not possible without a modern brain. The presence of a modern vocal tract in fossil hominids like Jebel Qafzeh and Skhul V 100,000 years ago is hence an index for a modern brain (Lieberman 1991).

A modern brain probably had already occurred in earlier African fossil hominids like Broken Hill who may have been ancestral to these fossils, and had a vocal tract that was almost completely modern. The chronology will have to be established in the years to come in the fossil hominids who have yet to be unearthed, ancestral to the early specimens of anatomically modern *Homo sapiens*.

Linguistic and manual evolution

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Greenfield deserves our thanks for having the guts to raise, in this academic den of wolves, what, in my opinion, is one of the most important issues in the field of evolution of complex functions: What is the relationship between linguistic and manual evolution? Her answer is that until the hominids, manual and linguistic (communicative) functions shared a common core of hierarchical organization centering on lateral frontal cortex of the left hemisphere. While applauding the attempt, I have two rather major areas of disagreement. First, I present evidence that the author’s analysis of early phonological (sound) organization is not a viable one, and conclude, therefore, that the suggested homology between early phonology and concurrent manual capabilities does not exist. Second, I present an alternative view of some aspects of the evolutionary relationship between vocal and manual organization, suggesting convergent evolution rather than homology. This view is based on a more specific conception of hierarchical organization than the one presented by Greenfield – a conception involving “frame/content” modes of organization. In contrast to the author, I attempt to say *why* this mode of organization might be common to the two domains.

Early phonological organization. One of Greenfield’s main claims is that “sound combination and object combination develop synchronously in a structurally parallel sequence.” She suggests that there are five successive stages of sound combina-

tion during the period from “about 9 to 20 months of age.” I provide contrary evidence that all the forms in this putative sequence predate the beginning of speech acquisition. Moreover, they are typically present *throughout* the period of *pre-speech* babbling, which lasts a few months. With one exception, they do not develop in any order. In addition, it is now generally agreed that an infant’s first words are virtually identical to utterance patterns of concurrent babbling (Locke 1983; Vihman et al. 1985). Consequently, there is very little that is new about the phonological form of early words. Furthermore, there is virtually no evidence that any of these forms are produced by “combination,” because some of the units supposedly being combined do not exist in isolation, and there are strong restrictions on what apparent vowel and consonant units can co-occur, suggesting an absence of independent control.

Consider the consequences of these facts for some of Greenfield’s detailed claims. In describing the proposed first stage of sound combination she claims that “the earliest words are reduplicated consonant-vowel syllables such as dada and mama.” “In these sound combinations, a single consonant is combined repeatedly with the same vowel.” Reduplicated consonant-vowel syllables are the most common multisyllabic forms from the beginning of babbling (Oller 1980). Two points can be made concerning the claim that vowels are being combined with consonants in these forms. First, the consonants concerned do not otherwise occur alone, so there is no evidence that they are separate units. Second, there are strong co-occurrence constraints between particular types of consonants and particular types of vowels in babbling (MacNeilage & Davis 1991; Vihman 1991) and early speech (Davis & MacNeilage 1990; Vihman 1991). For example in a recent case study of prelinguistic babbling, the most strongly favored consonant-vowel sequences were [dæ] as in “dad” and [bʌ] as in “bud.” But although the infant produced hundreds of two syllable sequences, there was not a single recorded instance of a [bædʌ] or a [dʌbæ] sequence or even of a [dæbʌ] or a [bʌdæ] suggesting extreme limits on combinatorial possibilities, not only for vowels and consonants, but also for syllables. Thus, rather than reduplicated consonant-vowel sequences such as “mama” or “dada” involving repeated “combination” of a single consonant with a single vowel, what are probably being produced are “pure syllable frames” (MacNeilage & Davis 1990a) by means of mandibular oscillation (open for the vowel, closed for the consonant) with no syllable-internal modulation by articulators other than the mandible.

The same criticism applies to the proposed second stage “in which a single consonant can be combined with a single vowel to form a word (e.g., Nicky’s *na* for “no” –).” The term “combined” is not justified. In addition, monosyllables do not develop after multisyllabic utterances, even in babbling. If there is any developmental asynchrony in a particular child, it tends to be in the other direction, as a sequence of two syllables that seem to be produced by repeating one.

Greenfield’s third stage “is characterized by a process called consonant harmonization” in which “the first sound, a single consonant, remains constant as it is successively combined with two different vowels.” In a recent case study (MacNeilage & Davis 1991) such forms were typical from the beginning of babbling and constituted most of the approximately 40% of multisyllabic sequences that were nonreduplicative.

The fourth proposed stage involves vowel harmonization in multisyllabic forms: “The initial consonant varies while the second sound (a vowel) with which it combines, remains constant.” Two recent studies of a number of infants have shown that from the onset of babbling an average of about 40% of multisyllabic forms involve consonant variation (Mitchell & Kent 1990; Smith et al. 1989). And in the study of the babbling of five 13-month-old infants by Kent and Bauer (1985) in which vowels as well as consonants were tabulated, about 20% of nonreduplicated utterances involved consonant variegation alone.

The proposed fifth and final stage involves, in part, “combining already developed syllable subassemblies into higher order units. This can involve adding a consonant-vowel combination to a second consonant to form a phonologically more complex word (e.g., ball –).” Development of syllable-final consonants is slow during babbling, but when such consonants begin to be produced, some of the most favored sounds in this position such as fricatives and liquids (e.g., the [l] in “ball”), are sounds that, at that time, occur only rarely in other positions in words, and therefore may not have the independent status required by the concept of “combination.” A better hypothesis regarding this development may be the opposite of the one Greenfield favors. Rather than integration (combination) of separate units, there may be differentiation of vowel production movements in space and time to produce a consonantal constriction following the vowel (MacNeilage & Davis 1990a).

In summary, virtually all the vocalization forms that Greenfield considers to develop serially during the development of word production are typically simultaneously present in some quantity from the beginning of prespeech babbling. In addition, studies of the organization of these forms give little justification for the view that they involve combination of units at all. Consequently, the portion of Greenfield’s theory that asserts that there is the same sequence of steps in the development of hierarchical organization of word phonology and concurrent manual function cannot be retained.

An alternative conception of manual-vocal relationships. The failure of the attempt to demonstrate a homology between early phonological organization and concurrent manual capabilities raises the question of why we might have expected similarities in the first place. Greenfield was encouraged to look for parallels between these two domains by Philip Lieberman, and both authors apparently share a motor orientation toward these issues, judging by their common emphasis on Broca’s area as a crucial locus of evolution of hierarchically organized sequential behavior, both manual and vocal. Although she does not explicitly say so, Greenfield presumably shares Lieberman’s conviction that identical organizational principles for manual and vocal organization originally evolved for motor reasons. (Lieberman proposes “that the rules of syntax derive from a generalization of neural mechanisms that gradually evolved in the motor cortex to facilitate the automatization of motor activity.” Lieberman 1984, p. 67). The question of whether there might be common principles and why, can be better addressed if we look a little more closely at the relationship between motor action and cognition in manual and vocal domains. In phonology, the supposed units and their hierarchical organization both have relatively direct reflections at the motor level. Cognition, (if we can use the word here) and motor action are directly linked, perhaps even relatively isomorphic. In morphosyntax, the relationship is less direct. Particular movement complexes (e.g., “cat,” “tack,” “act”) stand for cognitive units (morphemes) and hierarchy is revealed only by an analysis of syntactic and semantic relations between morphological constituents, which is only partly a matter of sequential organization. The relationship between cognition and action is even less direct in the manual domain, at least for the phenomena Greenfield describes. Hierarchy is revealed in the way that *objects in the environment* are placed in a spatial relationship to each other. In many cases the same results of the organization process could be observed whether the movements are executed with the left hand, the right hand, both hands, the feet, the mouth, a crane, a servant, a computer simulation, and so on. When it is noted that there are quite different relationships between motor function and cognition in these subdomains, the attribution of common motor-based organizational principles to the three domains seems somewhat less reasonable. And Greenfield gives us no reason why there should be common organizational principles. The implication is that manual and vocal function started out with common organizational principles. But why?

I suggest an alternative view in which there is a partial similarity in the organization of phonology, syntax, and manual function because they responded in the same way to a common selection pressure. The view is based on a more specific conception of hierarchical organization than Greenfield has proposed, namely, a conception of a “frame/content” mode of organization. This is defined as “two independently controllable components of output organization, with a complementary lock-and-key relation manifest in the output” (MacNeilage 1987, p. 290). I suggest that this mode of organization has been repeatedly selected in the evolution of the manual and vocal systems (an example of convergent evolution) because it bestows the ability to produce a large number of output sequences through the use of a small number of organizational configurations (frames). I now outline the way this conception has been applied to the manual and vocal systems, and even to the interaction between the manual and oral apparatus. While providing an alternative to Greenfield’s approach, it simultaneously provides a perspective on it.

In phonology, the fact that in speech errors erroneously placed segments (consonants and vowels) are relocated in the same position in the syllable as the one they “came out of” requires the postulation of syllable “frames” containing “content” elements (MacNeilage et al. 1985; Shattuck-Hufnagel 1979). It has been suggested that the basic open-close mandibular cycle associated with the syllable first evolved as the “lipsmack,” a communicative gesture common in other higher terrestrial primates (MacNeilage & Davis 1990b). The main properties of babbling, discussed earlier, are considered to reflect the ontogeny of the frame component of speech before the ability to insert segmental content elements has developed (recall the earlier arguments against the capacity for consonant and vowel combinatorics). This involves a more comprehensive view of the comparative neurobiology of speech than Greenfield presents. It is known that a medial motor subsystem including the supplementary motor area plays a more important role in vocal communication in other primates than does lateral motor cortex, the only motor cortical region mentioned by Greenfield (Jurgens 1987). It is consistent with the evolutionary progression suggested here that the SMA is the only reported cortical site at which both electrical stimulation (e.g., Penfield & Roberts 1959) and irritative lesions (see a summary of studies in Jones 1981) evoke babbling-like rhythmic syllabic (frame) sequences in patients (see MacNeilage & Davis, 1990b, for more details).

At the grammatical level, adult serial ordering errors in which misplaced open class morphemes (content word stems consisting of nouns, most verbs and adjectives, and many adverbs) are placed only into positions appropriate for content words led to Garrett’s influential conception of language production according to which “open class elements are being inserted into a grammatical *frame* [emphasis mine] defined by closed class elements – ” (e.g., function words and grammatical affixes; Garrett 1975). It is surprising that Greenfield does not refer to the basic grammatical dichotomy between open and closed class morphemes, particularly as there is some agreement that it is only the closed class (grammatical morpheme) component that is the primary responsibility of frontal cortex, the open class component being more the concern of posterior cortex (e.g., Zurif 1987). Infants’ first word combinations, however, which are taken as crucial evidence for Greenfield’s conception of the role of frontal cortex, are considered to involve primarily *open* class forms (Gleitman 1984). Thus, as in the case of phonology, Greenfield’s sole focus on lateral frontal cortex seems to lead to considerable oversimplification of neurobiological aspects of linguistic phenomena. In fact, left hemisphere specialization for vocal communication has been shown in monkeys to involve posterior cortex (Heffner & Heffner 1984) but not, as yet, frontal cortex.

The frame-content mode is considered to be present in

manual organization in cases where a supporting hand (frame) holds an object that is manipulated by the preferred hand, which, metaphorically, provides content elements (MacNeilage 1987). This important evolutionary development accompanied the evolution of hand-internal control in higher primates. As I pointed out earlier, the hierarchically organized acts that Greenfield considered under the manual heading are not necessarily closely linked to specific manual movements as such. Consequently, the postulation of a frame/content mode of manual organization, cited here to argue for evolutionary similarities between manual and vocal systems, has no direct implications for the cognitive bases of the manual tasks considered by Greenfield. It seems here that the task should be to explore the cognitive *but not the motor* relations between the tasks Greenfield considers and grammar. Surely, in both evolutionary and developmental terms, such cognitive concepts as subordination or coordination or temporal sequence have common implications for actions either in grammar or in operations on objects in the external world. This commonality will not be found in motor homology, however, as is revealed by the fact that grammatical morphemes are signaled differently in manual sign language (typically by movements superimposed on a concurrent sign for an open class morpheme) and in vocal language (typically by temporally discrete movement complexes). Action, in motor terms, was probably a very important factor in the evolution of cognition, but cognition is not necessarily closely constrained by action today.

Frame/content modes of organization are not confined to manual and vocal systems operating alone. Other frame/content modes are the coordination of both hands with the mouth (as in squirrels) and the coordination of one hand with the mouth, which became possible with the evolution of the prehensile hand in early primates (MacNeilage 1991). These modes of interaction between the hand and the mouth highlight a further problem with Greenfield's position. Her evolutionary view is one of the development of homologous manual and vocal organizational states from a hitherto undifferentiated substrate. Thus, she interprets Rizzolatti's finding of neurons in lateral frontal cortex that discharge only when the hand touches the mouth as evidence of a lack of differentiation in nonhuman primate cortex. I believe she underestimates these animals. It is more likely that these neurons help to mediate the very elegant frame/content operations of hand-mouth interaction in feeding that have probably been important throughout primate evolution.

The view that organizational similarities between manual and vocal systems are to some degree a matter of convergent evolution of frame/content modes of organization does not necessarily imply that there is no homologous substrate for the two domains. Elsewhere, my colleagues and I have argued that there is a fundamental homology linking the two domains in the form of a left hemisphere postural control specialization, from which both manual (right hand) and communicative specializations may have evolved (MacNeilage 1991; MacNeilage et al. 1987; 1988).

Nesting cups and metatools in chimpanzees

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Greenfield's target article was very stimulating. Having studied the cognitive behavior of chimpanzees in captivity and in the wild, I would like to present two related findings about chimpanzees for further discussion from the viewpoint of a primatologist or a cognitive psychology. One is "the subassembly strategy to nest the seriated cups by captive chimpanzees" and the other is a metatool use in wild chimpanzee nut-cracking behavior using stone hammer and anvil.

I made systematic observations on nine chimpanzees from ages 2 to 26 playing with seriated nesting cups (Matsuzawa 1986a, Table 1). The procedures are the same as those of Greenfield et al. (1972). Seven chimpanzees aged 4 and younger failed to make the seriated structure of five cups and always used the "pot" strategy of putting cups into a "pot" cup. It was also interesting that the chimps were not satisfied with the nonseriated structure and spontaneously put back the cups, trying again and again to make the structure follow the pot strategy. Two adult chimpanzees who had intensive experience in language-like skills, however, behaved just as human children of more than three years old do.

A chimpanzee named Sarah made a five-cup seriated structure in the first trial. She was given five cups, A < B < C < D < E from small to large. Her performance was as follows: In the first step, she put B into C. In the second, she put D into E. Third, she put the subassembly of BC into DE. Finally, she put A into BCDE. Sarah used the most advanced "subassembly" strategy

Table 1 (Matsuzawa). Summary data for the manipulation of seriated cups by chimpanzees.

Name	Sex	Age	Test place	Number of cups given	Trials	Seriated?	Strategy
Pan	f	2	Japan	3	24	Yes/no	Pot
Reo	m	4	Japan	3	24	Yes/no	Pot
Popo	f	4	Japan	3	24	Yes/no	Pot
Whiskey	m	4	U.S.A.	5	10	No	Pot
Opal	f	4	U.S.A.	5	10	No	Pot
Liza	f	4	U.S.A.	5	10	No	Pot
Frieda	f	4	U.S.A.	5	10	No	Pot
Ai	f	13	Japan	5	10	Yes	Subassembly
				6	5	Yes	Subassembly
				9	1	Yes	Subassembly
				10	1	Yes	Subassembly
Sarah	f	26	U.S.A.	5	4	Yes/no	Subassembly
				6	1	Yes	Subassembly

Ai and Sarah are language-trained chimpanzees. Whiskey and Opal have some experience with plastic-sign language. Popo, Reo, and Pan have intensive experience on match-to-sample.

Source: Modified from Matsuzawa 1986.



Figure 1 (Matsuzawa). A wild chimpanzee of Bossou, Guinea, is cracking an oil-palm nut using a pair of stones as hammer and anvil in the "outdoor laboratory." This adult female chimpanzee is named "Jire." She is using her left hand to hold the hammer and her right hand to manipulate the nuts. The coordinated behavior of both hands is necessary in cracking nuts with the stone-tool. The chimpanzees showed perfect handedness at the individual level but no left/right bias at the population level. [See MacNeilage et al. "Primate Handedness Reconsidered" *BBS* 10(2) 1987.]

and made the seriated structure in the minimum necessary steps. She succeeded in making the seriated structure in two of the four test trials and used the subassembly strategy in all cases.

Another chimpanzee (named Ai) behaved just like Sarah. Ai never failed to make the seriated structure of five cups from the beginning. Without any training, in the first trial she succeeded in making a 10-cup seriated structure by nesting the cups following the strategy hypothesized to be the most advanced, the "subassembly" strategy. Eventually, Ai would put one subassembly into another and the resultant large subassembly into the other subassembly in the course of making 9- or 10-cup seriated structures.

It must be noted that Ai had intensive experience of visual symbols called "lexigrams" and "graphemes" used for a language-like system (Matsuzawa 1985a; 1985b; 1989; 1990a). She could combine "words (lexigrams)" into a "phrase" like "red/pencils/five" in her favorite word order (Matsuzawa 1985a)

and construct a "word" from the elements called graphemes (Matsuzawa 1989). Her cognitive skill in memorizing a complex geometrical figure presented for a brief duration and in reproducing the copy from its elemental figures is comparable to that of human adults (Fujita & Matsuzawa 1990). In these tasks, Ai showed the ability of constructing a whole image from scratch. Sarah had shown a similar ability in "putting a face together" (Premack 1975). In conclusion, the chimpanzees can construct copies of existing or imaginary figures by means such as assembling pieces of existing materials.

One can raise the question of whether Ai and Sarah are especially gifted chimpanzees. Did the intensive training induce something different from what happens with the ordinary chimpanzee? My answer is "no." They are not superchimpanzees. I think all chimpanzees are super. I have been in Africa three times to study the cognitive behavior in wild chimpanzees since 1986. I recently observed an interesting metatool use in a wild chimpanzee.

The chimpanzees at Bossou, Guinea, use a pair of natural stones as hammer and anvil to open oil-palm nut seeds (Figure 1). I constructed an "outdoor laboratory" in the chimp ranging area to analyze the nut-cracking behavior experimentally (Sakura & Matsuzawa 1991). Each of about 50 stones was marked and the stone use was observed and recorded. Nuts were also gathered and provided by the experimenter. On January 16, 1991, an old female named Kai appeared with the other seven members in the laboratory and began cracking nuts. Kai took a pair of stones for a hammer and anvil and spontaneously took the third stone to keep the surface of the anvil flat. Kai left the three-level tool there, a hammer on an anvil on an anvil-as-anvil. Such use of a tool for another tool must be described as "metatool" use.

The experimental analysis of stone tool use in wild chimpanzees revealed that they mastered the skill at the age of about four; the skill of a seven-year-old, however, was far from the refined level of adult chimpanzees. I did the same experiment with human children from 2 to 11 years old at Bossou and found that the children under three could not use a pair of stones for nut-cracking. They could manipulate stones but failed to find the three-term relationships: nut-hammer-anvil. Young chimpanzees and humans had a tendency to miss a part: striking a nut with a hammer without an anvil; striking a nut on an anvil by hand rather than by hammer; putting nuts again and again on an anvil, and so on. I observed an 11-year-old boy put a stone under an anvil to keep the surface flat as just as Kai the chimpanzee did.

What I would like to point out is the depth of cognitive hierarchical structure shown in the skills of chimpanzees in captivity and in the wild. The cognitive ability of chimpanzees is still underestimated. The genetic difference between *Pan troglodytes* and *Homo sapiens* is estimated to be 1.7 in a comparison of DNA sequences (Koop et al. 1986). I directly compared the cognitive development of chimpanzees with that of human children in a series of diagnostic tests of stacking blocks (Matsuzawa 1987), sorting objects into plates (Matsuzawa 1990b), manipulating seriated cups (Matsuzawa 1986a), and so on.

In my opinion, the developmental course of the two species is the same. On many occasions, chimpanzees showed the rudimentary form of the most advanced stage of cognitive development in each diagnostic test. The critical difference between the two species might be the depth of the hierarchical self-embedded structures in cognitive functions. So far as is known, no "language" trained chimpanzees have mastered such metalanguage as "noun" and "adjective." Although the chimpanzees in the wild have a long list of tool use – such as sticks for termite-fishing, leaf sponges for drinking water, and stone tools for nut cracking – there are few examples of metatool use and no reports of the meta-metatool use, such as a tool for a tool for a tool. Greenfield's approach to the analysis of the depth of cognitive hierarchical structures is stimulating and exciting.

Evolving remembrance of times past and future

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A capacity for hierarchically ordered vocal utterance depends on neural circuitry that controls sequences of articulatory subunits in exactly the way that other bodily activity undertaken in the execution of a task that recruits subtasks requires subunit sequence control. Thus, at the level of neural and muscular instantiations, we have no problem with Greenfield's argument that a special "module for language" is unnecessary, because "language" conceived in neural and muscular terms is simply part of the family of bodily choreographies.

The problems roll in when an "evolutionary scenario" is presented wherein "increasingly adaptive" tool use on the part of our hominid ancestors would enhance the "adaptive power of protolinguistic communication" in the course of adult-infant apprenticeship regimes. Protolinguistic communication is called up in view of the "simple syntax" observable among our primate relatives following the "language" experiments undergone by primates such as the bonobo named Kanzi.

Overlooked in this scenario is the fact that the capacities elicited under these experimental conditions entail (1) the considerable ingenuity of the likes of Greenfield and Savage-Rumbaugh [see Rumbaugh et al.: "Linguistically Mediated Tool Use and Exchange by Chimpanzees (*Pan troglodytes*)" *BBS* 1(4) 1978] – fully paid-up members of modern language-using communities – as well as (2) the ability of apes (and humans) to ape, an ability not present in monkeys (Visalberghi & Frigaszy 1990). No equivalent tutors were available 2 to 3 million years ago (nor 200,000 years ago) for the aping of even the "protolinguistic" abilities in question. Or is Greenfield actually taking it that language inevitably emerges from certain brain mutations (in which case the apprenticeship scenario may be unnecessary – but then see our objections to the brain idea: Noble & Davidson 1991)?

Supposing the scenario is indeed the hearth at which language gradually warmed, certain other consequences follow from the logic of Greenfield's argument, as she herself demonstrates and explicates. The moves from "pair" to "pot" to "subassembly" in the evolution of "grammars of action" among the hominid ancestors of modern human beings bring about "embedded relative clauses" and, as necessarily, "the emergence of the tool use, tool construction, and general constructional skills required for modern human technology."

Greenfield insists that "there is no fossil record of behaviour," yet these skills emerged prehistorically and left an archaeological record of their material manifestations. As she is aware from the discussions at Cascais, there is more than one view about the evidence from prehistoric tools (Davidson & Noble 1990; Toth & Schick 1990). It is generally agreed that there was little change in techniques for producing stone tools from 2 million to less than 100,000 years ago (see Davidson 1991; Wynn 1990). Where there is evidence for wooden tools (Oakley et al. 1977; Thieme & Veil 1985), these were not made as subassemblies but merely by cutting. The evidence for constructed shelters has always been contested (see Davidson & Solomon 1990; Noble & Davidson, in press; Villa 1983); the evidence for hafting is open to question (Holdaway 1989).

It is an unfortunate truth that early archaeological evidence has been interpreted by those concerned to show the "remarkable abilities" of our ancestors without regard to the implications of their use or nonuse of language. Tacitly, abilities that result from language have been attributed to these ancestors because their perceived products can be made as a consequence of

language-mediated decisions. But the products can also be accounted for without language (Wynn & McGrew 1989) – and this makes more sense of the later archaeological record (once language had emerged) from the time of, and including, the first colonisation of Australia.

Making statements about the evolution of human abilities while ignoring *all* evidence from the past is common enough. One consequence is "merely" a difference about timing, but another is a failure to understand the relation between anatomical and behavioural features and natural selection. "Adaptiveness of primitive language and tool use" may "drive further brain evolution," but we see no evidence of this in the record of tools. Rather, as brain expansion occurred through mutation and selection (of larger-brained individuals) for manual dexterity and other unknowable abilities, redundant neural circuitry could have been recruited for finer control of oral/laryngeal musculature – yielding improved vocal communication (Calvin 1982) but not the distinctive reflectivity of language (Davidson & Noble 1989; Noble & Davidson 1989; 1991). In evolutionary scenarios new variation occurs without being driven by anything – the driving takes place in selection.

We do not quarrel with the proposition that language and the forms of tools that can be made are linked to each other, but the link is not simply at the level of brain function. Language is a form of behaviour in which meaningful signs are used symbolically, that is, referentially, arbitrarily (yet conventionally), in the absence as well as the presence of what is referred to. Consequences of this are the capacities for propositional thought and planning. Archaeological evidence of such capacities resides in material remains whose form entails more than the unavoidable results of biomechanical and physical forces – it involves fashioning to achieve a *planned* result.

That meaningful signs can be used symbolically is a discovery young children (of standard modern human neurological character) make in the course of their socialization as language-users (Lock 1980). It is not a discovery they could make unassisted by language-using older members (or assisted by creatures whose material products reveal no evidence of planning).

Thus, part of the issue in theorizing about language origins involves considering how the discovery of the meaningfulness of meaningful signs could occur prehistorically. For meaningful signs are plentifully produced by all sorts of creatures (see, e.g., the vervet studies of Cheney & Seyfarth 1990) but there is no evidence that these creatures are self-consciously aware of the putative meaning(s) those signs provide – or are aware of their significance. Maybe the "discovery" takes the form of a mutation that connects one part of the brain with another hitherto unconnected part. We have speculated, however, that it arises as a perceptual consequence of a novel behaviour (e.g., Davidson & Noble 1989; Noble & Davidson 1991) involving visual representation of meaningful bodily signs. There is no doubt that appropriate neural circuitry is required to enable a creature to witness the significance of its own forms of behaviour. But the capacity to provide an external representation of its actions seems to us a necessary precursor to its being aware of them.

Goal directed behavior in the sensorimotor and language hierarchies

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What is it that distinguishes humans from other species? In the realm of intelligence, the frequent answers are "language use" and "tool use." So in our attempt to define human intelligence, we have arrived at two more concepts that require precise

definition and evaluation. But is there more of a relationship between these two completely different manifestations of intelligence than their being specific to humans? Are they really “completely” independent? Language is in a sense a tool! Is this just analogy? Or homology?

Greenfield’s research is significant for its examination of the development of these two capacities in tandem, drawing from linguistics, psychology, and neurology – and feeding back challenges and light to all three. Her dual aims are to expose the “ontogenesis of hierarchical organization” and to explore the “evolutionary roots of language,” but this commentary concentrates on the first of these, largely at the expense of the second.

The focus on ontogenesis of hierarchical organization is itself distinctive and courageous. Greenfield’s examples are language and behavior. The natural world is also hierarchically organized, however: land, sea, and sky; animal, vegetable, and mineral; head, body, arms, and legs. Even recursion seems to be natural – for example, snowflakes show recursive structure. Behavior is a counterpart of environment – reflective and reactive. Language is its own sensorimotor counterpart – and therefore in another sense unique. The language and behavioral hierarchies considered by Greenfield are thus two among many, and can themselves be characterized more finely into subsidiary hierarchies (e.g., language encompasses not only syntax and semantics, but also phonology and orthography).

Consider the behavior of a monkey that leaps into a tree, races across a branch, plucks and cracks a nut, and eats the kernel. This behavior requires recognition of hierarchical structures. The behavior itself requires a *decomposition* of this structure. This *recognition* and *decomposition* must also come into “ontogenesis of hierarchical organization” – probably as prerequisites to the *construction* and *production* behavior on which Greenfield focuses. Given that the *analytic* tasks can be accomplished, a reversibility propensity would permit *synthetic* planning.

The very fact that hierarchical organization seems so fundamental to network models of cognitive processes makes it surprising that one area of the brain, Broca’s area, should be basically responsible for hierarchical behavior. Is it better characterized as recursive behavior? What is significant is that somehow a whole conceptual substructure can be reused as a conceptual unit. The hypothesis that Broca’s area has differentiated during infancy is most plausible. The precise nature of the behavior concerned, however, cannot yet be regarded as definitively characterized.

Broca’s area is more closely associated with the motor cortex than with the sensory. Thus it seems reasonable to concentrate on motor behavior (such as speech production and construction tasks) when evaluating its function. Broca’s aphasia, however, is also characterized by deficits in the correct comprehension of relationships expressed by the closed class words they fail to produce (Geschwind 1979). Thus it would also be interesting to see how much of the behavioral deficiency reported here relates to problems in the analysis, as opposed to the synthesis, aspects of the tongue depressor copying tasks.

Let us turn now to Greenfield’s analysis of the behavior she wants to map into language behavior. Pairing, pot, and sub-assembly methods all depend on some idea of goal, which is clearly a prerequisite for a plan. In fact, Greenfield’s analysis makes assumptions, and omits possibilities, *inter alii* in relation to these questions of planning and motivation.

As far as a child is concerned, at a stage when he is just doing trial-and-error “pairing,” why should there be any difference between an atomic unit (a single cup) and a higher-order unit (a set of nested cups)?

Physical units can be grabbed equally easily, and combined pairwise, without reference to their complexity. This is the fundamental attribute of hierarchical structure: Knowledge of the substructure is irrelevant to the use of a given unit. The

more dexterously demanding task of building a tower with the cups is not considered – the three “nesting cup” possibilities exclude implicitly the possibility of inverting the cups and “tower building.” In this case, there would be more difficulty in manipulating a higher-order unit than an atomic unit.

Thus the pairing method for nesting cups should not only produce the behavior of Strategy 1, but also that of Strategy 2 or Strategy 3 (in Greenfield’s Figure 1). Unless there is a tendency to bias for or against the use of higher-order units in either the active or the passive role, the different types of outcomes should be present with a probability related to the proportion of complex units present in the environment. In fact, one might suspect that there is a tendency to destroy (decompose) the higher-order units, as the more interesting ones. The pairing method for tower building may lead to practically indistinguishable cases of accidental and deliberate destruction of a higher-level unit.

“Strategy” implies the existence of plan and goal. The observation of what can be achieved through the pairing of units of arbitrary complexity would suggest the possibility that the jump to either high-order strategy could proceed very quickly once the goal is present. If the goals are given by the experimenter in a new environment where the child has had no experience, direct progress to the higher level strategies would be expected of those with more likelihood of appropriate analogous experience. And if the goals are supposed to be set by the experimenter, what evidence is there that those goals are the ones the child understands and chooses to pursue?

The difference between Strategy 2 and Strategy 3 does not seem to be primarily one of hierarchy. Units of the same complexity are combined in both. The difference is whether it is the moving or the stationary unit that is complex, and perhaps whether there are other constraints in play – in this case the constraint that a specified component must be on the ground. This would seem to bias the strategy toward the pot method. If the constraint was, by contrast, that the nested cups should end up in the hand in such a way that all but the last were hidden and only the outermost visible, I suspect that Strategy 3 might be preferred at an earlier age. This is equivalent, I would think, to using Strategy 3 with the cups upside down. Slightly more dexterity may be required, however.

An alternative analysis, which would tend to support the analogy depicted in Figure 3, is focused not on hierarchy, but on role. Strategy 3 may be viewed as more complex on the basis that *cup b* changes role from object to subject (undergoer to actor). This would seem to be a deliberate step in contrast with the default behavior of retaining the same object in the hand, a tendency noted by Greenfield. Strategies 2 and 3 break this tendency. Strategy 3 further breaks the analogous tendency to retain the same fixed target object.

The relationships relating language use to spoon use in section 3.3 are intriguing. The problem of goal is still there to an extent (using the spoon to eat), but the child’s goal does at least have the same result (food in mouth). The child’s secondary goal may be more to imitate what he sees (and has seen so often) – analogous to the goals of the “cup” experiment. The “pot” strategy prediction for spoon use does seem to jell with casual observation, and it would seem to occur at the predicted time. Nevertheless, the stages are likely to be influenced by parameters other than pure brain development. And the same goes for language. In particular, there is a stage when everything seems to end up in the mouth, and the far more specific goal of imitating the observed (but possibly not correctly analysed) use of the spoon as a tool may not be so high in the child’s goal-priority ordering.

Language is itself clearly goal directed, so similar influences might be expected to be at work. Nonetheless, Greenfield’s argument for a correspondence with either the one-, two- and three-word sentence stages, or the single clause, conjoined

clause and relative clause grammars, fails to be convincing.

The neural predictions and their confirmation are quite compelling, however, although the extent to which the neural "growth spurt" of section 4 is genetically (as opposed to experientially) directed needs to be explored further. On the one hand, there seems to be too small an amount of genetic material to explain direction at this level, while on the other hand there is evidence of "shifting" and "sampling" phenomena associated with neural growth (Jacobson 1978, p. 399ff.). It is quite possible that the extent of the differentiation of Broca's area is influenced – permanently – by experience.

My own computer modelling work (Powers 1983; Powers & Turk 1989) has from the beginning been based on the assumption that (some of) the same mechanisms apply equally well to the different sensorimotor and linguistic hierarchies. Greenfield's sections 3.2 and 3.4 follow the finer hierarchies of language behavior, examining developmental parallels in phonological rules (Lieberman 1984; 1990), concluding that Broca's area plays a similar role here, too. This is further supported by my results on the retargeting of computer experiments from the syntactic hierarchy (Powers 1983) to the phonological and orthological hierarchies (Powers 1991).

In the earlier experiments, statistical and neural network techniques were used to discover automatically closed classes and associated rules in noun phrases and filtered clauses, and this process was hypothesized to be associated with Broca's area. In the later experiments, similar techniques were applied to orthographical data (the unix dictionary) with the prediction that the vowels would emerge as a closed class. This prediction was also confirmed – with certain diphthongs and semivowel combinations just missing out on membership, and "u" just scraping in. The self-organization techniques applied by Kohonen et al. (1984) are in some respects similar, and it seems quite probable that my results would carry over from orthography to phonology.

Thus, the evidence is suggestive of a commonality between grammatical and phonological complexity, which would be explicable by a unified process involving Broca's area. As mentioned above, the role of Broca's area in analytic functions would also be worth exploration.

Section 4 is particularly impressive because predictions made from *psychological* theory in a *linguistic* domain were substantiated with the discovery of totally new neurological data. There should be more of this. Unfortunately, in linguistics, psychology, neurology, and artificial intelligence, theories, hypotheses, and models are often pursued totally independently – making claims with implications for the other disciplines, but without going to the trouble of making testable interdisciplinary predictions.

Hierarchical organization in grammar

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"Hierarchical organization" has straightforward applications within the field of phylogeny. In it, there are levels of complexity that follow a phylogenetic sequence such that later developments dominate earlier ones. The mental processes for pattern-forming that produce the "templates" to motor-programme tool-use behaviour, for example, offer a similar analysis: increasing complexity, discrete levels of development, (indicated ontogenetically), and perhaps, a hierarchical, that is, dominant/subordinate organisation of the processes. Yet in all these mental activities a process of integration or "combination" is required. This commentary suggests how these notions might apply to the functions of a "grammar module," say Broca's area.

Systems of formal grammar require the premise that their syntax expresses logical relations. The task of the grammar is to make overt "semantic representations," so the logical relations must already be covert in these representations. In parsing, the logical relations can be presented hierarchically, as inverted tree diagrams, though some forms of parsing express the relations as an integrated series of nestings, for example,

s[np[det[The]n[boy]]vp[v[stood]pp[p[on]np[det[the]adj
[burning]n[deck]]]]] (s = sentence, np = noun phrase, det = determiner, n = noun, vp = verb phrase, v = verb, pp = preposition phrase, p = preposition, adj = adjective)

In this complex sequential pattern, apparently, hierarchy is replaced by scope. The notion of a hierarchy might be introduced by using different shaped brackets for various levels. The illustration is offered to indicate that there may be different formats for expressing given phenomena.

A functional analysis of semantic representations, however, offers a slightly different appraisal of their organisation. As a departure point, Fillmore's (1977) dictum may be cited: "Meanings are relativized to scenes." Leaving aside the pragmatic and discourse deixis content of sentences, their declarative content may be summarised as being basically graphic: descriptions of items or of situations, whether the latter are processes or are viewed as kinematic. As far as I can ascertain cross-linguistically, the logical relations within propositions turn out to be spatial relations or spatial orientation, whether concrete or metaphorical, (as in case relations) and a widespread application of spatial relativity is the linear relativity of temporal expressions.

Spatial observation requires spatial orientation: a focal or "data" point to which all items are related. The various items related may form their own focal points to which "sub-assemblies" may be related. In description, such orientation comes out as relative salience. Then hierarchical organisation, as found in syntax, perhaps derives from and expresses such underlying mental processes as distinguish the relative salience found in spatial representations. A primary level in grammatical hierarchy, subject and predicate, might be claimed to be based on "focal" salience. So-called "subjectless" sentences such as, "It's raining," offer a global description of the visuospatial field; salience and hierarchical organization are lacking.

In producing grammar, the language production module is integrative. The components – syntactic, pragmatic, discourse deictic, lexical, rhetorical, perhaps even phonological and intonational – are subject to an integrative process. To the extent that the components are so subjected, there is one level of hierarchical subordination. Integration – a combinatorial faculty to produce complex patterns – is the module's main task; most of the components are supplied from other sources. It may be noted that the special areas for dexterity and grammatical production lie, according to the generally accepted model of the brain's functional architecture, appropriately close to the highest integrational areas.

Then it may be that the various mental processes mentioned in Greenfield's target article ultimately derive from a neural substrate that is concerned with the abstract processing of spatial representations so as to form, indeed to invent, patterns. For spatial patterning, special neural areas develop out of this substrate. The mental processes that produce the behaviours under discussion may have all undertaken a further step, so that they have in common a mental process of hierarchical organisation, however that notion is to be defined: One might need to expand the notion to include integration and salience. Greenfield's thesis offers a sound argument that that phylogenetic step, with its ontological consequences, was taken.

Are rhythms of human cerebral development “traveling waves”?

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Greenfield does an effective job of addressing some of the environmental and epigenetic factors involved in human cerebral development. The mechanisms of neural circuit differentiation are currently unknown, although new models and theories promise to aid our understanding of this area. My work with colleagues (referred to by Greenfield) is on a nonlinear dynamic model inspired by computational ecology (Thatcher 1989; 1991a; 1991b).

This model is based on convergent evidence of oscillatory patterns in the postnatal maturation of cortico-cortical connections. For example, Rabinovitch (1979), Blinkov and Glezer (1968) and Conel (1955; 1959; 1963; 1967) have demonstrated rhythmic oscillations of such specific anatomical features as developmental patterns in the thickness of cortical gray matter, the packing density of cortical neurons and in cortical volume. Recent analyses of relative power EEG in 473 normal children (Matousek & Petersen 1973) by Hudspeth (1985) and Hudspeth and Pribram (1990) also reveal rhythmic patterns of cerebral maturation. The patterns of rhythmic maturation in these studies show considerable temporal concordance with the patterns independently observed in recent EEG coherence studies (Thatcher et al. 1987; Thatcher 1991a). Since EEG coherence reflects the magnitude of cortico-cortical connections (Nunez 1981; Thatcher et al. 1986; 1987) it is consistent with convergent evidence to conclude that there are periodic increases and decreases in the magnitude or strength of cortico-cortical coupling during postnatal cerebral maturation.

An important question is: What do these rhythms or “waves” of maturation represent? The answer seems to be that the cerebral maturational waves behave as “traveling waves,” beginning near conception and extending throughout the human life span. The mechanics of the wave processes reflect dynamic equilibria between competing and cooperative neuronal networks (Thatcher 1989; 1991a; 1991b). The magnitude of EEG phase maturation (on the order of 40 milliseconds) is too great to result primarily from white matter myelination (see Thatcher et al. 1987; 1991b). Moreover, there is no evidence of oscillations in the development of white matter density and maturation, whereas there is clear evidence of postnatal oscillations in the development of various features of grey matter cytoarchitecture (e.g., Blinkov & Glezer 1968; Thatcher et al. 1989; 1991a). For these reasons, the maturational waves of EEG coherence and relative power appear to reflect dynamic kinetics and dynamic competition for the growth of dendritic synaptic contacts.

The period emphasized by Greenfield, from birth to age 5, is marked by competition for dendritic synaptic sites which occurs during periods of neuronal loss and while skull volume is increasing (Blinkov & Glezer 1968; Goldman-Rakic 1987b; Goldman-Rakic & Schwartz 1982; Rakic 1985; 1988). This results in an overall decrease in neuronal packing density and an increase in dendritic length and thus an increase in the dendritic surface area available for synaptic contact (Blinkov & Glezer 1968; Thatcher 1989; 1991a; 1991b). The oscillatory modes may result from delays between the activation and termination of trophic growth mechanisms that are signaled by biochemical mechanisms activated by spatial limits (Thatcher 1989; 1991a). Each oscillatory mode involves the production of a surplus of synaptic contacts followed by a pruning away of unused or inessential synaptic contacts. A computational ecological model has been used to explain the stages of human cognitive maturation in terms of nonlinear dynamics. Each cognitive stage is marked by extended periods of equilibrium between competing and cooperative neural networks punctuated by brief periods of

dis-equilibrium (Thatcher 1989; 1991a). The development of certain selected cortico-cortical connections that strongly shift the competition and dynamics further mark each cognitive stage. Among the most dominant cortico-cortical connections are those that develop between different regions of the frontal cortex and posterior intracortical regions (Thatcher 1989; 1991a; 1991b; Thatcher et al. 1987).

Objects are analogous to words, not phonemes or grammatical categories

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The vast majority of work on the neural bases of language focuses on the medium not the message – on speech rather than on symbols and grammar as communicative devices. Greenfield’s attempt is to explore the neural bases of language as a communicative device, and that is what makes it so important (cf. also Lieberman 1984). Using the comparative method *à la* Werner and Piaget, her focus is on hierarchical organization in (1) object manipulation and (2) language/communication.

The major question is what is the proper level at which to look for analogies and homologies in these two domains. Greenfield cites data to the effect that in their object manipulations human infants first use a pairing strategy, then a pot strategy, and then a subassembly strategy (Figure 3). She cites data showing that same thing for language in terms of the manipulation of grammatical categories. Thus, when the child places a cup in or on another cup, this is said to be analogous to a sentence in which the child indicates an actor (subject) acting on (verb) an object (direct object). The problem is that these analogous behaviors do not emerge at the same time in human ontogeny – children act on objects with a particular strategy well before they produce grammatical constructions exhibiting that same strategy. Greenfield thus proposes a change in the level of analysis from grammar to phonology. This will allow her to consider analogies and homologies between the manipulation of phonemes and the manipulation of objects – which are a priori more promising candidates for developmental synchrony.

Although she is able to show some synchronies in development at this level of analysis, in my opinion the analogy using phonemes as objects simply does not work. I have a very difficult time seeing why “baby” (supposedly a pairing strategy) requires a level of organization different from “stinky” (supposedly pot strategy). In the former case the consonant stays constant while the vowel varies, and in the latter case the vowel stays constant while the consonant varies. The fact that the consonant is the initial sound in both of these words does not seem to me to be a crucial factor for hierarchical organization. The crucial factor is supposed to be that the actions in the pot method are related to one another in a way that the actions in the pairing method are not; this difference would not seem to be characteristic of the phonemes in the words “baby” and “stinky.” Moreover, the phonological skill supposed to correspond to the subassembly method emerges at 15 months in both reported subjects, that is, during the pot stage and a full 5 months before the subassembly strategy is used in object manipulation.

I believe that the proper level of analysis for the analogies Greenfield wants to make is neither in terms of grammatical categories nor in terms of phonemes, but rather in terms of *words* and how they are related to one another in utterances. The pairing, pot, and subassembly strategies emerge at 12, 16, and 20 months, respectively. What is going on in language acquisition at these times? At 12 months many children have begun to use their first words to attain concrete ends. As Bates (1979) argues, they use a single word at this stage as a simple

tool, an object for achieving an end – much as they use a cup to bang on other cups or put a spoon in a bowl. In none of these cases do they actually *manipulate* more than one object/word; any other objects involved are merely substrates, background to the action. This is a one object/word strategy (I therefore do not like the term “pairing” strategy for these behaviors). At 16 months of age many children have begun combining words into word combinations. They have not yet learned productive syntactic devices for indicating the different roles played by the different words in the utterance, and that is why the utterances are word combinations (not “sentences”) devoid of grammatical categories. Simply put, children at this stage are merely dumping the words into one intonation contour the same way they dump objects into a pot. At 20 months of age many children begin producing multi-word utterances with a hierarchical example, at around 19 months of age my daughter began producing such three-word sentences as, “See Daddy(s) car.” A detailed analysis of the diary data found that in almost every case she had previously produced a two-word utterances that was identical, or almost identical, with two of the adjacent words in the longer utterance (and had produced the other word as either a single word or a two-word combination). In the above example, she had already produced utterances of the “See car” variety and the exact utterance, “Daddy(s) car.” This would seem to be a rather direct use of the subassembly method at the level of the word (see Tomasello, in press a, for other similar examples). In my opinion, this very simple and straightforward analogy – which, by the way, is the one Greenfield uses when looking at the chimpanzee Kanzi who has no phonology – works much better than either of the others that Greenfield suggests.

Following Bates (1979), it is important to emphasize that an analogy or even a homology between two domains does not guarantee simultaneous emergence in development. Many children develop skills with objects that are not manifest in their language for months, and, although it seems less likely, some children may show the opposite pattern. This is because the instantiation of the underlying structure (e.g., hierarchy) into a domain often requires other skills, with their own developmental histories and individual differences. Thus, hierarchical organization in language must await the acquisition of symbols, which requires vocal-auditory and social learning skills and is facilitated by some environments; object manipulation requires visual-manual coordination, at the very least. (It may be that during the sensorimotor period action has a special status relative to symbols, which are not totally differentiated from action at this point. But after 18 months or so, structures may emerge first at either the action or the symbolic levels; see Tomasello & Farrar, 1984 and 1986, for discussions).

Finally, Greenfield proposes several possible phylogenetic scenarios for the evolution of the hierarchical organization of behavior. She opts for one in which the neural substrate for hierarchy evolves in the adaptive contexts of both object manipulation and language. I prefer to view language as more derivative because it involves other things (cf. Bates 1979). Hierarchical organization most probably evolved in the context of object manipulation and tool use. At the same time – and this is the aspect of the scenario that Greenfield ignores – humans were evolving a special social intelligence and special social learning abilities (Tomasello 1990). The grammatical *relations* in language – such things as agents, patients, instruments, benefactors – come from the human conceptualization of real world experiences in terms of “events” with particular conceptual roles over and above simple hierarchy. And these are most likely to come from the propensity of human social intelligence to focus on the “narrative structure” of experience (Bruner 1990). It was not until hierarchical organization and event structure came together (perhaps not until *Homo sapiens sapiens* 30,000 years ago; see White, 1989, and Tomasello, in press b) that the fully modern form of human grammatical organization could arise.

The comparative simplicity of tool-use and its implications for human evolution

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I would like to argue in support of Greenfield's second evolutionary scenario – the one she does not favor. To do this I need to build on her discussions of object manipulation and tool use. Greenfield presents a persuasive case for the existence of a common neural base for language and object manipulation early in development. She further argues that language later acquires complex hierarchical features that have no parallels in object manipulation. Few would disagree here. She also suggests, however, that object manipulation and tool use develop complex features of their own. It is this argument for “separate but equal” complexity in later ontogenetic and phylogenetic development that I find unsupported.

Compared to language, adult tool use is not very complex. Unfortunately, tool use is a relatively neglected field in cognitive science, making comparison to language difficult. Greenfield's discussion of early tool use, based on the work of Connelly and Dagleish (1989), is an essential component of her basic hypothesis. Her discussion of tool use later in development, however, is not well developed. It is far from obvious, for example, what relevance the copying of diagrams made from tongue depressors has to tool use, yet this is her central piece of evidence for complex hierarchies in post-infant tool behavior. When we turn to some of the recent literature in cognitive anthropology, a picture of tool use emerges that is not dependent on hierarchical models. Day-to-day tool use relies heavily on rote sequences learned by repetition and practice (Gatewood 1985). The chaining of action into sequences, even by adults, is very like the sensorimotor schemes of infancy (Wynn 1990). The plans of action characteristic of tool using have at best simple hierarchies of routines and subroutines linked together at the time of use or memorized as recipes (Dougherty & Keller 1982). Nowhere does day-to-day tool use present the kind of organizational complexity characteristic of the day-to-day language used by all normal adults. More to the point, language has well-recognized spontaneous productive power. All normal speakers can generate new sentences *without practicing them*. New tool using sequences, on the other hand, must be learned by repetition; even master artisans must practice new routines. This difference in productive power indicates that tool using relies on organizational features simpler than those of language.

The comparative simplicity of tool using behavior suggests that it may have been the older adaptation. Greenfield has documented the comparative simplicity of object combination in chimpanzees, suggesting quite reasonably that this is homologous with human abilities. If we extend this interspecific comparison, something interesting emerges. Human adult tool use is much more like the combinatory behavior of chimpanzees than it is like the linguistic behavior of human adults. Greenfield herself emphasizes the apprenticeship of the Tai Forest chimpanzees when they learn to use stone hammers. The chimpanzees learn by observation, practice, and “scaffolding” by the mother. This kind of learning does not, in fact, require any sophisticated developments in communication. It is also the way humans still learn tool behavior.

One of the remarkable features of human apprenticeship is that it is largely a nonlinguistic process. Artisans are notoriously inarticulate when it comes to their work because so little of it concerns words (Dougherty & Keller 1982). Tool use is still learned in a relatively primitive manner. Greenfield makes a strong case for the homology of object combinatory behavior. These are similar in humans and chimpanzees, far more similar than the communicative behavior of the two species. Because

language is the more complex of the two, and because tool use has changed least when compared to the behavior of chimpanzees, it seems reasonable to conclude that language, and its neural base, is a more recent acquisition.

The archaeological record corroborates this. (Greenfield is wrong on one minor point. We do have a fossil record of behavior in the form of prehistoric artifacts and refuse of various kinds.) The earliest known stone tools (about 2 million years old) closely resemble chimpanzee tools in all respects pertinent to cognition (Wynn & McGrew 1989). From this early point we have a relatively complete record of tools spanning the entire 2 million years of human history. Nowhere in this record is there a dramatic technological development requiring that we posit a complete reorganization in the way our ancestors thought about tools. This is not to say that human tool using behavior (and its cognitive basis) have not evolved. Clearly our tasks require greater memory than those of chimpanzees and, occasionally at least, greater problem solving ability. But the style of thinking is still very similar. It is a comparatively primitive kind of thinking that has clear roots in our hominoid past. The same is not so obviously true of language.

Greenfield's hypothesis allows us to examine these questions of evolution; this is one of its great strengths. In demonstrating that there are some similarities between early language and tool use, and in making effective use of the comparative evidence, Greenfield has made a strong case for the homology of certain cognitive abilities. The later ontogenetic divergence of language and tool use, and the lack of divergence in chimpanzees, indicates that a divergence must have occurred in human phylogeny, as well. It appears that one of these behaviors – language – acquired sophisticated organizational features. The other, however, did not.

Author's Response

From hand to mouth

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As a group, the commentaries were extremely stimulating, even, in one case (Connolly & Manoel) leading me to go out and make additional observations.

A number of commentators were pleased with general characteristics of the approach in the target article, notably (1) relating biology and neurobiology to development in general (Karmiloff-Smith & Johnson) and to language development in particular (Jacobs); (2) investigating the neural and developmental basis for modularity, thus removing modules from their "black box" status (Golinkoff, Hirsh-Pasek & Reeves; Karmiloff-Smith & Johnson); (3) using neural evidence, rather than behavioral, intuitive, or philosophical arguments to investigate homology (Jacobs); (4) investigating output mechanisms, in contrast to recent emphasis in cognitive development on input processing (Karmiloff-Smith & Johnson). Probably most important to the theory itself, Powers appreciated that the theory was strong enough to make testable neural predictions.

The model presented in the target article has two main

components, the ontogenetic and the phylogenetic. Each component is in turn composed of three major elements: language, manual object combination/tool use, and neural substrates. Following a section in which new data concerning the predictive power of the model is reported, these subdivisions organize the outline of my response.

R1. The predictive power of the model

A crucial test of the validity and power of any theory is its ability to make new predictions beyond the specific data and general phenomena from which it was generated. Two of the commentaries provided or stimulated me to provide new observations in which the validity of the theory could be further tested.

R1.1. The "pot" strategy: The predicted missing stage in the ontogeny of human tool use

For the model of the ontogenetic relations among cortex, language, and object combination skills to apply to the phylogeny of language, tools, and brain, it was necessary to demonstrate that tool use and its development is a special case of the more general skill of object combination and its development. Using the study of Connolly and Dalgleish (1989) concerning the ontogeny of spoon use to demonstrate this point, parallels were drawn between the early stages of object combination strategies (the pairing, "pot," and subassembly strategies first described by Greenfield et al., 1972, and illustrated in Figure 1 of the target article) and the strategic development of spoon use, the Euro-American child's first tool.

There was evidence of the pairing and subassembly stages of tool use in Connolly and Dalgleish's published observations, but no sign of the intermediate "pot" strategy. Because Connolly and Dalgleish had not oriented their data analysis at all in terms of my theory, the theory was used to predict that the "pot" strategy would appear at an intermediate chronological position between pairing and subassembly in the ontogeny of spoon use. It was therefore gratifying that Connolly & Manoel took the trouble and care to go back to the coded spoon use data of Connolly & Dalgleish to search for the predicted stage.

It was disappointing that Connolly & Manoel did not find any sign of this strategy in their data set, although their commentary does report some ambiguous evidence of the pot strategy in a subsequent study (Connolly & Dalgleish, in press). An interpretative comment made by C&M, however, suggested that environmental conditions had been such as to prevent the phenotypic expression of the intermediate "pot" strategy in their English sample; they observed that mothers generally discouraged their infants from feeding themselves because handfeeding is so messy.

It seemed that, because of differences in cultural values, mothers in the United States would tend to encourage self-feeding at an earlier age, in the interest of independence and exploration, and that such early encouragement of self-feeding would be even more strongly manifest in a group care setting where one caregiver would often have to feed more than one baby at a time. I therefore spent two lunchtimes observing tool use at the

Infant Development Program in our Psychology Department at UCLA.

R1.1.1. Sample. Seven children ranging in age from 12 to 29 months of age were chosen for pilot observation. They were of mixed ethnic background: one African-American, one Mexican-American, one Asian-American, and four Euro-American babies. All had at least one parent who studied or worked at UCLA, most in the Psychology Department. The age range covered the sequential appearance of the three strategies in the original nesting cup study (see Figure R1). Five of the children were observed at one lunchtime each; two were observed twice, one week apart.

R1.1.2. Results. The sequence of strategies was exactly as predicted by the model, including the existence of the “pot” strategy and its appearance at a chronological point intermediate between pairing and subassembly. For the 12-month-old baby, the pairing strategy was dominant. The four oldest children (19, 19, 26, and 29 months of age) used only the subassembly method with the utensil (one child used a fork rather than a spoon). An even more complex subassembly strategy was shown by one of these children: a boy first created a subassembly unit by spearing pieces of meat with a fork, then he dipped the meat-on-fork into ketchup, finally he used the fork to bring meat plus ketchup to his mouth.

Most critical to validating the proposed model, the 16-month-old was observed to use only the pot strategy. She did so in a sequence in which she first used her right hand to put food directly into her mouth (while holding the spoon in her left hand). She then switched her spoon into her right hand and put it into her mouth. Next she switched the spoon to her left hand and started putting food in her mouth with her right hand. In this sequence, the mouth serves as the common “pot” for two different objects, spoon and food; the objects are never combined into a single subassembly, however. This sequence is exactly how the target article predicted the pot strategy would manifest itself. It is particularly interesting in the light of the neural model that posits unilateral cortical involvement for tool and language that this child used the same hand for both objects (food and tool), even when she had to switch hands to do so. [See also MacNeilage et al.: “Primate Handedness Reconsidered” *BBS* 10(2) 1987.]

Note that the chronological age for each strategy is thus far very much in accord with the observed ages for dominant strategies in the nesting cup study (Figure R1). In that study, pairing was most frequently the dominant strategy at 12 months, the pot strategy at 16 months, and subassembly made its first appearance as a dominant strategy at 20 months of age. The 18-month-old child in the spoon observation sample used only the pairing strategy, showing behavior somewhat less typical of her age group; even this child, however, was in the range shown in Figure R1. Although no 18-month-old children participated in that earlier study, three out of eight 16-month-old children used pairing as their dominant strategy. This child may have had less spoon practice than the other children as well; she was the only one being fed by her mother at the infant care center.

Finally, a second, unpredicted form of the pot strategy also appeared. The 12-month-old child first repeatedly

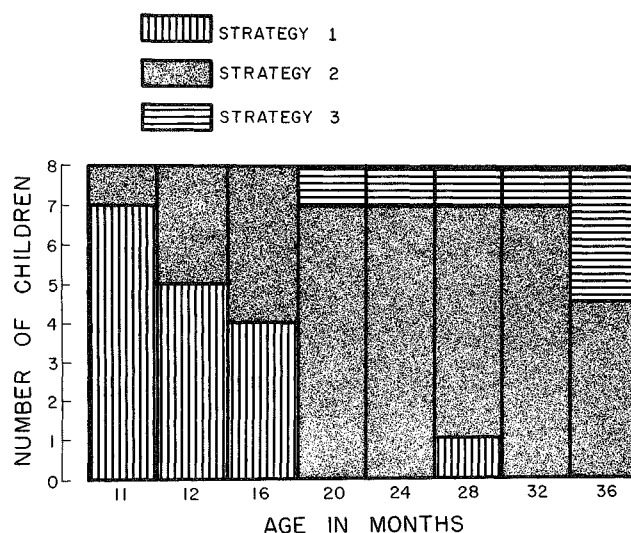


Figure R1. Frequency of dominant cup strategies at different ages (Greenfield et al. 1972).

hit the bowl with her spoon, then put food from the table into the bowl. In this instance of the strategy, bowl rather than mouth very literally serves as the “pot” or common object. Although pairing was the dominant strategy for this child, it was not unusual for the pot strategy to manifest itself at this age in the original nesting cup study (see Figure R1).

The progression from pairing to pot to subassembly is not simply a function of increased motor coordination. For example, the individual manual acts comprising the pairing and pot strategies of spoon use are the same and therefore have identical motor requirements. With nesting cups, although not spoon use, the exact same motor acts are also used in the subassembly strategy. It is therefore primarily the organization of the acts that differentiates the three strategies.

In conclusion, in an environment in which most children are allowed to self-feed as early as possible, the pot strategy appeared, as predicted. Because this stage was a prediction from the theory presented in the target article, these observations provide further support for its validity. It appears that Connolly & Manoel were right in attributing the absence of the pot stage in the original Connolly & Dalgleish data to the discouragement of messy self-feeding by English mothers.

R1.2. The missing phylogenetic link: Chimpanzees playing with nesting cups

An important link between the ontogenetic and phylogenetic evidence has been supplied by Matsuzawa in his very useful commentary. Although the model presented in the target article posits parallels between the development of object combination strategies in young children and chimpanzees, the evidence was not exactly comparable. For the most part, tasks and materials were not alike. The task from which the basic sequence of combinatorial strategies was derived, the nesting cup situation, was not reported with chimpanzees. In addition, the information on object combination and tool use in chimpanzees was not developmental, whereas the data on children were.

Matsuzawa has filled this gap with his cross-sectional developmental study of nesting cup behavior in nine chimpanzees ranging in age from 2 to 26 years of age, previously published only in Japanese (Matsuzawa 1986b). Matsuzawa reports the most complex strategy that each chimpanzee used when faced with a set of seriated cups disassembled by the researcher (Matsuzawa, personal communication, October, 1991).

The target article concludes that chimpanzees are very capable of using the pot strategy in their tool use and object combination, that their use of the subassembly strategy in the wild is borderline, but that, in captive symbol-sophisticated chimpanzees, the subassembly strategy, as manifest in spoon use, does unambiguously occur. Matsuzawa's data support these conclusions concerning the maximum level of hierarchical complexity developed by chimpanzees.

In younger chimpanzees (ages 2 to 4), the pot strategy was the maximum accomplished. In the older chimpanzees (ages 13 and 26), the subassembly was carried out. Both the 4-year-olds and the older chimpanzees had been trained in the use of humanly devised visual symbol systems (Matsuzawa 1986b).

The data have interesting developmental implications. The 2-year-old chimpanzee is quite similar in rate of development to the 2-year-old child, most of whom do not attain the subassembly strategy (Greenfield et al. 1972). The 4-year-old chimpanzees seem a bit slower than children in the United States in developing the subassembly strategy (Figure R1), but they are quite on a par with Zinacanteco Mayan children, who have much less object manipulation experience than children in the United States (Greenfield et al. 1989). In essence, the sequence of strategies for combining nesting cups is the same in children and chimpanzees, although the final stage may occur later in chimpanzees. Chimpanzee subjects older than 4, but younger than 13, would have to be tested to ascertain this.

R2. The ontogenetic model

R2.1. Sound combination, word combination and syntax

The target article proposes a hierarchical theory of the development of the computational aspects of language. According to this model, there is not, contrary to received wisdom in linguistics (Bickerton, personal communication, March 13, 1991), an independence of levels, in this case the phonological and morphosyntactic. Rather, in the tradition of Bryan and Harter (1899) in psychology, the lower levels are conceived as components of the higher levels, elements that must be automatized before they can become components of higher order processes (Bruner 1968). From the structural perspective of hierarchical organization, inflections are not differentiated from other morphemes, except that they provide marking for particular branches of the hierarchical organization (e.g., agreement of subject and verb). With development, the number of hierarchical levels grows.

The lower level or levels of the hierarchy involve phonological combination. The upper levels involve morphological combination. There is a trade-off between phonological and morphological complexity, however.

That is, the total hierarchical complexity can be increased either by adding more complex phonological structure to a constant number of words or by adding more words while keeping phonological structure constant. Figure 10 in the target article makes this point: The one-word utterance *ball* is at the same level of hierarchical complexity as the two-word utterance *bye tat [cat]* because of its more complex sound structure. According to the theory, the neural support for the construction of these structures starts out being provided by a circuit from Broca's area to the orofacial motor cortex; in this circuit, Broca's area programs the combinations by providing input to the orofacial motor area. Somewhere in the second year of life, a circuit from an anterior prefrontal area begins to develop; as this progresses, Broca's area begins to receive input from a more anterior region, and the levels of the circuit expand (Figure 11, target article). As this occurs, the hierarchical complexity of the language structures also grows. The maximum number of levels before this expansion seems to be about two (see Figure 10). Inflections can be used to mark the different parts of the hierarchically organized tree structure (e.g., last two examples of Figure 10). Future research is required to test this theory. For example, it predicts a trade-off between phonological and syntactic complexity, keeping overall hierarchical complexity constant: Phonologically more complex words will occur in shorter sentences at early stages of language development. In different ways, both Bloom and Bickerton take issue with this model.

Bloom had, in a referee's report on an earlier draft of the target article, usefully pointed out that my model needed to take account of the more precocious syntax in such inflected languages as Italian. I therefore used Hyams's (1986) work on the acquisition of Italian morphology to revise my model, yielding the sequence of examples in Figure 10 of the target article. In his commentary, however, Bloom now lumps English and Italian together and contrasts them to such languages as Quiché Mayan, Polish, and Turkish, whose morphology he states is (presumably *really*) precocious. Bloom seems to feel that such extremely precocious development would challenge my theory because it would involve even earlier syntactic marking. I accordingly read Bloom's references to Quiché (Pye 1983), Polish (Weist et al. 1984), and Turkish (Aksu-Koc & Slobin 1985). As I have done developmental research in Tzotzil, another Mayan language, Quiché Mayan was the easiest for me to achieve an analytic understanding of what children were doing at what age. It turned out that the acquisition of Quiché Mayan not only failed to support Bloom's criticism, but it provided quite dramatic (and unexpected) support for my model.

First of all, the youngest age at which any example in Quiché was given by Pye (1983) was 26 months; hence there was no possibility of demonstrating with these data that syntax would be too complex too early for the model. The main difference between the morphology of Quiché and Italian, however, was that a part of a syllable could function as a grammatical morpheme, whereas in Italian, it is always a whole syllable. What Pye found was that *the syllable, not the morpheme was the child's unit of production*. Thus, 2-year-old children were observed to reduce a complex, multisyllabic, multimorphemic verb to a single syllable.

In the light of a criticism raised by **Bickerton**, it is particularly interesting that this single syllable could contain the final consonant of the verb root plus an inflectional ending (e.g., *loh*, a 26-month-old's reduction of *kinwiloh*, I like it.). Bickerton criticizes the formalism that shows an inflected Italian verb *mangio* to be produced by first attaching the inflection (*io*) to half the stem (*g*) and then attaching this "hybrid" to the rest of the stem (*man*). The Quiché example, however, shows that what may be a hybrid to a linguist can be a psychologically real unit for a child at the early stages of language acquisition: *Loh* parallels the structure of *gio*; its first sound (*l*) is the end of the stem *wil*, whereas the rest of the syllable (*oh*) is inflection. The fact that the preceding syllable (*wi*) containing the first part of the verb root is left unrealized demonstrates the psychological reality of the syllable as a production unit in a way that the two-syllable production of *mangio* could not.

The primacy of the syllable as the unit, stressed by Pye's (1983) data on the acquisition of Quiché, provides evidence that the level of syllable formation in my model has psychological reality in terms of the child's own processing. As Pye (1983) points out, a number of researchers on the acquisition of English phonology have pointed to the syllable as the important unit (e.g., Ingram 1978; Macken 1977; Menyuk 1976; Moscowitz 1970; Waterson 1971).

At the same time, the existence of child language utterances in which a single phoneme (e.g., *l* in *loh*) is a morphological unit that is combined with another morphological unit (e.g., *oh* in *loh*) to make a single syllable (these also exist in English) implies that the syllable formation levels of the tree structures in Figure 10 must have psychological reality as well, while illustrating the interdependence of word formation (phonology) and morphological combination (grammar). For example, the lowest level of the hierarchy may in some cases be phonemes, in other cases a phoneme and morpheme. This interdependence of phonological and grammatical combination is intrinsic to the model of structural development being proposed.

Aksu-Koç and Slobin's (1985) data for the acquisition of Turkish pose more of a challenge to the model, but, in the end, they also highlight and strengthen its basic assumptions. In Turkish, productive noun and verb inflections are present in the one-word stage, as young as 15 months of age. As Aksu-Koç and Slobin (1985) point out, however, this is because stems are often monosyllabic and inflections are stressed suffixes. The examples provided for a 15-month-old (*daba*, *dabagi*, *bebeki*) would be diagrammed in my model as two-level structures, in which consonant vowel pairs are formed on the lowest level, then combined with each other on the second level. These also occur in my examples at 15 months of age (*ball*, *bye tat*, top of Figure 10). Examples of productive morphology at 15 months of age do lead me to modify one aspect of the model: The critical factor in hierarchical complexity is not the nature of the unit (i.e., an inflectional morpheme vs. a free-standing morpheme), it is simply the organization of the units. Although this point is implicit in the model of the development of language structure as increasing hierarchical complexity (Figure 10, target article), the earlier inflectional productivity

seen in Turkish led to making this point an explicit part of the model rather than associating inflections with the later differentiation of grammatical structure and object combination structure (sect. 3.5, target article).

Similar comments could be made concerning the Polish examples presented by Weist et al. (1984): In the case of their data, productive inflections occur in two-word speech. The only difference vis-à-vis my model is that there are some examples of the level of complexity that I have pegged at 21 months (Figure 10) occurring at 19 months in Polish (e.g., *lecial samolot* "was flying plane" = "The plane was flying"). But this does not seem to pose any particular difficulty for the model. The neuroanatomical data identify 12 to 15 months of age as the period in which the connections from Broca's to the orofacial cortex, the first-stage speech circuit, is occurring. In the light of this chronology, one might expect to see structures of the complexity of *more cookie* (Figure 10) or *lecial samolot* any time after 15 months of age.

In summary, **Bloom's** commentary has enriched the model by forcing it to incorporate acquisition findings from a variety of language types. The data from Quiché Maya simply strengthened the model, whereas the data from Turkish and Polish have led to a revision of the place of inflectional morphemes in the developmental model of language structure.

Many of **Bickerton's** comments also focus on the model of structural development in speech production. He raises a number of questions about the way I conceptualize the hierarchical organization of sound combination. For example, I made *tat* a one-level structure like *bye*, rather than a two-level structure like *more* because *tat* was viewed as resulting from a pairing strategy in which two phonemes *t* and *a* are combined. The final *t* was further viewed as abbreviated reduplication, rather than another level. This approach was consistent with the pairing analysis presented in section 3.2.2 of the target article for the early reduplicated words (e.g., *dada*, *mama*). Because such reduplicated words occur widely before children can construct a word with two different consonants, this view seems to capture the developmental/psychological reality, even if it leads to a linguistically unorthodox formalism. Indeed, Bickerton himself grants that a child of that age probably cannot vary consonants word-internally. The difference would seem to be that I am trying to capture that fact in my diagram (target article Figure 10), making my formalism linguistically unorthodox.

The same logic applies to *cookie* where **Bickerton** finds it baffling that a terminal *k* is attached to a nonterminal node. Here I was again trying to represent the fact that *kuki* (cookie) also involves a pairing strategy in which a common element (*k*) is successively attached to two different vowels. Had this word been represented in a more conventional way, one could not have captured the fact that such a structure is at a lesser level of hierarchical and developmental complexity, according to the model and data presented in the target article, than a word such as "coffee" in which two different consonant phonemes combine with two different vowel phonemes. The formalisms were designed to depict the psychological reality of development; they were not designed to accord with linguistic convention.

R2.2. Questions concerning the nature and significance of parallel developmental timing and stages in linguistic combination and object combination

The question of parallel developmental timing in manual activity and language is raised by **Bickerton**, who points out that, although the subassembly appears in object combination around 20 months of age, its subsequent development seems slower than that of the structurally parallel linguistic structures. According to the model of neural development presented in the target article, however, one would expect this divergence between linguistic and object combination beginning at this age because of the differentiation of the cortical circuitry in the two domains that begins at about this time (bottom of Figure 11).

Indeed, further exploration of Thatcher's cross-sectional EEG coherence data revealed that a more dorsal circuit going from anterior prefrontal cortex to Broca's area begins to increase its connectivity a few months after the more ventral circuit, which increases its coherence starting just after two years of age. This more dorsal prefrontal network could possibly reflect the left side of Circuit 1, the object combination circuit at the bottom of Figure 11. If so, the evidence would indicate that the development of the prefrontal manual circuit could lag a few months behind that of the prefrontal grammar circuit, thus providing a neural reason for Bickerton's observation of developmental decalage.

Several developmental psychologists (**Bloom, Golinkoff et al.**, and **Karmiloff-Smith & Johnson**) raise the question of the significance of such parallels, stating that there is hierarchy everywhere in human behavior. But hierarchies do not everywhere follow the same qualitative sequence and timing in their development. And a neural basis for the linking of the hierarchies turns analogy to homology. Moreover, insofar as different parts of the brain have common principles of organization, one would expect to find common principles of organization in different domains of behavior. In the theory presented in the target article, however, the development and timing of *specific* neural circuits was linked to the development and timing of *specific* behavioral developments in the domains of language and manual object combination. Such specificity is important to the theory; it is correspondingly crucial to clear up the misunderstanding expressed by **Grafman & Hendler**, who mistakenly say that I try to demonstrate that *all* organized sequential behaviors arise from a common neurological source.

Wynn approaches the problem of language/tool-use parallels from the perspective of an archaeologist, noting that human tools are simpler than human language. This may have been so in the period of evolutionary adaptiveness; it is no longer so, however. If we include symbolic tools like computers or occupations like rocket designer, it is clear that the hierarchical organization of these tools has the potential to exceed language in its complexity. Note, too, that we really do not know how complex language was at the period of its early evolution. In any case, the model presented in the target article would predict divergent not parallel development of the two domains as the anterior prefrontal circuitry underwent its further evolutionary development subsequent to the split between the hominid and chimpanzee line.

R2.3. Have I chosen the correct linguistic units for homology?

Two commentators are critical of the linguistic units that I have selected for homology: **Tomasello** and **MacNeilage**.

MacNeilage criticizes the word formation or phonological level I have selected; his objection is that there is not enough ability to combine and recombine separable units in early phonological development. The model, however, *predicts* just such developmental *constraints* in the combinatorial possibilities at the earliest stages; it predicts a sequence by which these constraints will be lifted at subsequent stages. **MacNeilage** also expresses dissatisfaction with the fact that the motor level is more constrained in speech than in object combination. Early object combination is surprisingly constrained, however, at least from a cognitive point of view. Nonetheless, **MacNeilage** may be right that there is a more direct translation of motor programs to motor action in sound combination than in object combination. This may be because the hands, unlike the mouth, deal with objects in the outside environment that are unpredictable in size and location, therefore being extremely variable in their requirements for action. If so, **MacNeilage's** point would in no way constitute counterevidence for the model.

A more serious criticism is that the various word formation patterns described occur in earlier babbling, where they do not emerge in any particular order. It is stated in my target article, however, that the model deals with the formation of meaningful words, not with babbling. Most telling was the fact that, according to **MacNeilage**, the cortical control of infant babbling is not located in Broca's area, but is instead in the supplementary motor area, the area that is important in nonhuman primate vocal communication. [See **Goldberg**: "Supplementary Motor Area Structure and Function: Review and Hypotheses" *BBS* 8(4) 1985.] This suggests the interesting possibility that babbling is homologous with the vocalizations of primates, while differing in its cortical control from meaningful speech.

I therefore conclude that differences in the developmental patterns for babbling and early words do not undermine my theoretical model because babbling is controlled mainly by the supplementary motor area, a cortical area with functions and structures different from Broca's area, the key cortical area in my model of word formation and its development. In sum, while the supplementary motor area may well be important in the initiation of speech, the phonological processes of word formation receive their organizational control at the level of Broca's area; it is this area that would then be responsible for the developmental sequence of word formation that is proposed in the target article.

MacNeilage suggests switching to his unit of analysis, frame and content. Having long admired **MacNeilage's** work, I find his unit of analysis highly appropriate to his investigation of interhemispheric differentiation. My model, however, concerns differentiation within a single hemisphere. The frame/content notion does not seem likely to illuminate the behavioral manifestations of this within-hemisphere structuration.

Tomasello recommends words rather than sounds as the potentially homologous units. His most cogent crit-

icism is that the temporal synchrony of the object combination and word formation stages is not perfect for the subassembly stage where my diary observations of the onset of subassembly in word formation/combination and the onset of subassembly in the experimental study of object combination are five months apart.

Tomasello therefore suggests switching the unit of analysis to the word. At first glance his suggestion seems to work. He must force the single word into representing the pairing strategy by relating it to a nonverbal element in the communicative situation, however. Then the “pot” becomes the intonational envelope for two-word utterances. Finally, two-word phrases become subassemblies in three-word sentences. This conceptualization has the problem that its units are apples and oranges – and also that the nature of the combinatorial units changes from stage to stage. The original units presented in the target article accordingly seem preferable. As for the five month gap, individual differences in rate of development may have created this *decalage*, given that the language data point was provided by a single child. In any case, differential environmental stimulation and other factors can also create a certain degree of asynchrony in homologous behaviors (Fischer et al. 1990). Another possibility is that the manual action circuit may develop a few months behind the corresponding grammar circuit, as discussed above.

R2.4. Where does human sign language fit into the theory?

This question is raised from a neuroanatomical perspective by **Hauser**. Data from Poizner et al. (1983) indicate that massive damage to Broca’s area and the left anterior frontal lobe leads to agrammatism in sign language as in spoken language. Lieberman (1991) notes that there is a disagreement between Poizner et al. (1987) and Kimura (1988) concerning the relationship between sign aphasia and disturbance in manual motor behavior. Whereas Poizner et al. claim that the manual behavior of the aphasic subjects was undisturbed, Kimura subsequently tested one of the same subjects and found difficulties in complex hand movements that paralleled the difficulties of this patient with syntax. She states that the discrepancy comes from the fact that Poizner et al. (1987) used very simple hand movement tasks not requiring complex combinations of movement.

Apart from the neuropsychology of sign, data on early stages of sign language development offer a further line of evidence for the validity of the model, while increasing its scope. At the same time, the theoretical model resolves some ongoing controversies concerning the interpretation of comparative data on the acquisition of sign and speech.

The model of the behavioral origins of hierarchical organization in speech begins with the process of combining sounds into words. The first stage parallels the pairing strategy in object combination and occurs at roughly the same time. The prediction concerning sign language would be that its earliest units would also be formed with a pairing strategy. New findings published in *Science* by Petitto and Marentette (1991a) indicate that this is indeed the case. In both babbling and the first meaningful signs, deaf children make a syllable by combining two different

handshapes or two different movements (holding the other variable constant). They also produced reduplicative versions of the pairing strategy, as hearing children do.

Other comparisons between the development of sign and speech in the same children (hearing children bilingual in sign and spoken English whose parents are deaf) actually support the model of cortical development posited to underlie the development of speech and manual object combination. There has been a controversy concerning whether first words or first signs develop earliest. The study of Petitto and Marentette (1991b) addresses this issue in a way that controls for individual differences. Applying the model of cortical development presented in the target article to sign language, one would predict that, at the earliest stage of circuit development, sign language would appropriate Circuit 1 at the top of Figure 11 in the target article, the manual circuit going from classical Broca’s area to the manual motor cortex. Although both Circuit 1 (manual object combination circuit) and Circuit 2 (grammar circuit) develop in the period from approximately 12 to 16 months of age (Figure 11, target article), the data from Simonds and Scheibel (1989) and from Thatcher (unpublished data, 1991) cited in the target article indicate that the connections from Broca’s to the orofacial motor cortex develop at 12 to 15 months of age (Simonds and Scheibel’s neuroanatomical data), whereas the connections from Broca’s area to the manual motor cortex may lag a month or two behind (Thatcher’s EEG coherence data). Although the two data points are established by different methods, they do suggest a *decalage* in favor of the spoken word. And indeed this is exactly what Petitto and Marentette (1991b) find: The first word emerged from two weeks to a month before the first sign in all three hearing children of deaf parents studied by the researchers. Given that the children were receiving much less stimulation in spoken language than in sign because sign was used exclusively by their deaf parents, this gap in favor of speech could well be larger still with equal stimulation of the two modalities of language.

What is most interesting is that the theory of the ontogeny of cortical circuitry developed in the target article would predict word formation in speech to be slightly in advance of word formation in sign language. The theory could not only make this prediction, it could also provide a neurodevelopmental explanation of why this would be the case. One hopes that the existence of such an explanation will help to resolve the controversy concerning the relative precocity of the two modalities of language acquisition. At the same time, the fact that changes in timing are frequent sources of evolutionary change (Gould 1977) makes one refrain from seeing the implications of a small developmental asynchrony between word and sign as evidence for the gestural theory of language evolution (Hewes 1976).

R2.5. Comprehension versus production

The claim that the neural circuitry for language comprehension differs from that of production is questioned by Golinkoff, Hirsh-Pasek & Reeves, as well as Bloom. There exists abundant supporting evidence, however, beginning with the specification of Wernicke’s area and

continuing to the cortical localization work of Ojemann (e.g., his *BBS* target article, 1983).

In response to **Bloom's** question about the separate evolution of comprehension, no such claim is being made. Rather, because common neural foundations are the ultimate test of homology, we must look separately at the evolution and development of capacities subserved by different neural circuitry. Because of an interest in the grammar of manipulative object combination, I began with production rather than comprehension. Bonobo evidence (Savage-Rumbaugh 1991) indicates that the prehuman roots of speech comprehension may be much more firmly established than those of speech and even gestural language production. One hopes that the target article will stimulate future research and thinking along these lines.

Indeed, **Jacobs** recommends comprehension research in his commentary. His own quantitative dendritic analysis of Wernicke's area (Jacobs 1991) would make a good starting point for future investigation. One would expect both symmetries and asymmetries in the evolution of comprehension and production and in their relations to tools. Tools, like language, must be comprehended as well as used. Note too that what was claimed was not total neural separation of production and comprehension but overlapping circuits. Such an overlap would be expected to be most salient at the programming level. This is in accord with findings, for example, that agrammatics neither comprehend nor produce complex syntax (e.g., Caplan et al. 1985).

R2.6. Specification of the neural model

The neural model is enriched by **Lieberman's** discussion of subcortical connections from Broca's area to more anterior prefrontal cortex. By way of background for the use of his developmental neurophysiological data, **Thatcher** provides the reader with a technical description of his EEG approach to measuring neural connectivity and its implications for neural development. **Grafman & Hender** rightly remind us of the importance of time in the organization of all complex, sequential behavior. This has more face validity to it than **Rolfe's** suggestion that the neural substrate in question is specialized for spatial, rather than temporal processing.

Deacon makes the suggestion that the prefrontal origin of the language circuit has been placed too high. Based on monkey evidence, he sees the origin as approximating Brodmann areas 45 and 47 (Deacon, personal communication, April and October, 1991). Based on Ojemann's human brain stimulation data, however, I intended the origin of the circuit to be the higher region of Brodmann Area 46, the approximate area pinpointed by Ojemann's grammar task. Although I have a preference for using human rather than monkey data where possible, I recognize that the Ojemann grammar data are based on few subjects (3) and an imperfect task (grammatical errors in reading aloud, a partly visual task). Particularly in the light of large individual differences between brains, more and better data are needed to resolve the contradictory indications from the human and monkey evidence concerning the most accurate location of the left prefrontal inputs to the grammar circuit. But whatever this location (or locations) turns out to be, note the agreement be-

tween Deacon and myself that prefrontal inputs do indeed play an important role in grammatical production. Indeed, given the varying characteristics of the different neural research techniques used and the wide range of individual differences in brains and their functional maps, the general location, configuration, and developmental trajectory of the proposed circuits are much more meaningful than exact locations.

Jacobs questions the notion that, as long distance connections develop, connections with neighboring cells and areas decrease. Both myelination (strengthening of existing connections) and the proliferation/pruning of synapses (creation of new connections) are important in the development of longer distance intracortical circuits. Currently there is disagreement in the field as to the relative contribution of each process. As noted in his commentary, **Thatcher** finds evidence in his EEG data that synaptogenesis/elimination is more important than myelination. For him, therefore, the developmental pruning of synapses is a process by which shorter-distance links (involving axon collaterals) tend to lose out in a competition for dendritic connections to longer-distance links (involving the main trunk of the axons) (O'Leary 1987).

Jacobs also asks how one can know where inputs start without axonal tracing. These were hypotheses based on neuroanatomical evidence of a growth spurt in the input mechanism (dendrites) at a certain cortical location, along with information concerning a growth spurt in EEG coherence between that location and a second location. It was then inferred that the dendrites in the first neural location were receiving input from the second location. **Fuster** casts doubt on the significance of this conclusion by his statement that, although coactivation indicates participation in a common network, it is not conclusive evidence of common function. It seems, however, that **Fuster** may have a more restrictive interpretation of function than I intend. I do not take coactivation as indicating a single common function in the narrow sense, but rather as indicative of component functions cooperating to carry out a particular task.

Fuster wants to place Broca's area in an intermediate control position between motor cortex and the anterior prefrontal cortex. Although he characterizes my position as one of Broca supremacy, my neural model in fact posits an intermediate position for Broca's area after age two (see bottom of Figure 11 in the target article). According to the model, however, Broca's is the highest level programming area between about 1 and 2 years of age (see top of Figure 11 in target article).

R2.7. Additional neuropsychological data

Cases of Williams syndrome, with their seemingly intact language capacity but poor performance on cognitive tasks, offer an interesting test, as suggested by **Colinkoff et al.** **Karmiloff-Smith & Johnson** refer to research from their laboratory that indicates a possible gap between the hierarchical organization of speech by Williams syndrome children and their hierarchical organization in other domains. In fact, the drawing of Williams syndrome children lacks the subassembly level of hierarchical organization (Bellugi et al. 1990): They are not able to integrate parts into a higher order whole. Although drawing

tasks do not involve object combination, such tasks are sequential, manual, and can be hierarchically organized; therefore, in the absence of evidence to the contrary, we will categorize them with object combination tasks for the sake of the present argument.

It is important to remember, however, that the neural model in the target article predicts the possibility of some dissociations in combinatorial skills from some time in the second year of life and, indeed, it was developed to explain such dissociations in the data of Curtiss et al. (1979; Curtiss & Yamada 1981). The children with Williams syndrome were tested from 10 to 17 years of age, however. Hence it is possible that they have developed a selective problem with the manual part of the differentiated circuits (Circuit 1 at the bottom of Figure 11) but that they have the grammar circuit, Circuit 2, relatively unimpaired. The model of neural development presented in the target article would lead one to predict greater association between manual and grammatical programming earlier in development, before the underlying circuits have expanded and differentiated. There is some indication that this is the case, because children with Williams' syndrome are delayed in the development of their language production in the first five years of life (Bellugi et al. 1990). In other words, early, before the differentiation of the neural underpinnings of speech and manual programming, the Williams syndrome children presumably have difficulty with both domains. Later on, after the circuits have differentiated, the difficulty is selectively located in the manual circuit. The fact that the language production delay lasts until age 5 does not fit perfectly with the model, but information about the early development of these children is still sketchy.

R3. The phylogenetic model

Corroborating lines of evidence to support the phylogenetic model as a whole are provided by Gibson. Frigaszy sees comparative implications of my ontogenetic model, specifically the notion that, from the point of view of cognitive complexity, tool use is a special case of object combination. She suggests further research that would test this idea by comparing the hierarchical complexity of object combination and tool use in different primate species. Gómez & Sarriá suggest extending my phylogenetic approach to issues in the development of pragmatics. Anderson correctly notes that it would be fruitful to study the hierarchical development of chimpanzee signs, as each sign consists of components (place, configuration, movement) that are analogous to the phonemic level of speech. He, like Gardner & Gardner, points out that B. Gardner et al. (1989) have carefully recorded and described the development of these sub-morphological components (as has Matsuzawa [e.g., 1985b] using a graphic language with chimpanzees). It was particularly gratifying to learn that my theory was potentially generative of further comparative research. A number of commentators raised critical points concerning the phylogenetic model, however, and I now turn to these.

R3.1. Meaning of bonobo language data

According to Bickerton, I have overestimated the linguistic achievements of Kanzi, a bonobo chimpanzee. He attributes this problem to the fact that I have over-emphasized word order based on my bias as an English speaker. He (unintentionally) implies that word order is a less legitimate part of grammar than inflections, which become more complex in free-word-order languages. Such an implication would not be valid: An emphasis on word order as a grammatical device is an *alternative* to an emphasis on inflection in the languages of the world. As we wrote in Greenfield and Savage-Rumbaugh (1990) (which was sent to Bickerton at his request), Kanzi was limited to word order because of the nature of the lexigram symbol system he was given to use; inflection was not possible in that system.

Bickerton further implies that I, like most psychologists, have been led astray by knowledge of English, a configurational language, combined with a lack of knowledge of "exotic" languages or Latin, a relatively free-word-order language. As a matter of fact, my analysis of Kanzi's symbol use has been very much informed by knowledge of "exotic" languages, Latin, and such other free-word-order languages as Italian. I have done research in two "exotic" languages, Wolof (Senegal) and Tzotzil (a Mayan language), studied five years of Latin, speak fluent French, speak reasonable Spanish, and have the rudiments of Italian. Indeed, it was my knowledge of Italian, a free-word-order language, that enabled me to make a hierarchical analysis of Hyams's (1986) Italian examples presented in Table 10 of the target article.

Moreover, I was not biased by English in my analysis of Kanzi's language; Kanzi was. In analyzing the input he received from his caregivers, we found that they (not surprisingly) arranged their lexigram communications to Kanzi in English word order. A productive rule that Kanzi acquired from them hence (not surprisingly) followed English word order, just as an English-speaking child's would. We also found that Kanzi invented some of his own rules, however. When these were considered, it was concluded that Kanzi was using a partial ergative system, in contrast to the accusative system modeled by his caregivers (Greenfield & Savage-Rumbaugh 1990). I was attuned to the possibility of ergativity by the research of Goldin-Meadow and colleagues (e.g., Goldin-Meadow 1979) with deaf children of hearing parents who invent an ergative system, as well as by my familiarity with Tzotzil, an ergative language.

On the other hand, Bickerton (personal communication, May 1991) pointed out that ergativity may be an incorrect interpretation of Kanzi's system. In an ergative system, subjects of intransitive verbs have the same marking or placement as objects of transitive verbs. Kanzi's gestural intransitive agents, however, placed last as were his transitive objects, implied transitivity on the semantic level, even though the object was not lexically realized. For example, when he created the utterance CHASE (pointing to a geometric symbol) you (pointing to an agent), he generally wanted the agent to chase him or another person, even though no surface object was expressed. Full transitives, being small in number, had not been analyzed. Bickerton's suggestion that perhaps Kanzi

was creating a verb-first accusative system led me to analyze his corpus of full transitives.

Analysis showed that Kanzi sometimes created both a gestural agent and a gestural object, in such utterances as CHASE (pointing to a geometric symbol) you (pointing to an agent) her (pointing to another person he wanted to see chased). As originally observed by my collaborator, E. S. Savage-Rumbaugh, both agent and object generally followed the action symbol, in that order. Although the full transitives were infrequent ($N = 8$), statistical analysis of these utterances indicated that Kanzi followed the symbol order action-agent-object to a statistically significant degree ($P < .00001$, significance of a proportion test). Objects in such utterances could be inanimate as well as animate (e.g., SLAP [indicating a geometric lexigram symbol] you [gesturing to a person] BALL [indicating a geometric lexigram symbol]).

If we consider these newly analyzed full transitives in conjunction with the action-agent utterances, most of which have a transitive sense, it seems that Bickerton is right in suggesting that Kanzi may have created, not an incipient ergative system, but an incipient verb-first accusative system, something that also exists in the panoply of human language. In such a system, the marking of the subject stays constant whether the sentence is intransitive or transitive. Because Kanzi places agent and object next to each other in his transitive utterances and lacks means for inflectional marking, there is no possible way of deciding definitively between these two alternatives. Kanzi has an incipient grammatical system that, in the absence of inflections, is intrinsically neutral as to the distinction between accusative and ergative.

Whether accusative or ergative, however, the important point is that Kanzi created a system that was not modeled by his English-speaking human caregivers, although it is part of the range of human languages. For this reason, I believe that Noble & Davidson overestimate the contribution that humans have made to Kanzi's communication and language development.

Note that, in any case, the use of inflections versus word order do not affect the model. That was why it was possible to use inflected examples from Italian as well as configurational examples from English in the developmental sequence portrayed in Figure 10 of the target article. Sentences from either type of language can be similarly analyzed for their hierarchical organization.

Bickerton states that a gulf exists between what bonobos can do grammatically and mature human syntax. I would agree. Greenfield and Savage-Rumbaugh (1990) clearly indicated that the bonobo produced only the most primitive syntax, approximately at the level of a 2-year-old child. We were willing to term this level "protogrammar," but then insisted that we should also speak of the language structure of the 2-year-old human as protogrammar. No further claim is made in the target article.

Bickerton points out (from the chapter I sent him) that there are differences between human children and Kanzi, a bonobo, in the characteristics of their communicative corpi. Two-year-old children use word combinations more than single-word utterances; for Kanzi the balance goes heavily toward one-word utterances. Although this point was not mentioned in the target article, neither was it claimed nor implied there that Kanzi is exactly like 2-

year-old human children in his use of language. Indeed, such an identity relationship should not have been expected. In tracing phylogeny, one looks not for exact similarities but for related forms with possibly homologous origins. Other primates did not evolve language; only humans did. One would therefore expect chimpanzees to find human language more difficult, the more complex it became; they would accordingly tend to avoid its most difficult forms, the same way a human being might procrastinate in reading a difficult scholarly article. Kanzi seems to be close to his limits with two- and three-symbol utterances. Children are not. Just as even human adults avoid triply (or even doubly) center-embedded sentences, structures that strain our processing limits, one would expect chimpanzees to avoid two- and three-symbol utterances, structures that are near to or at their processing limits. In comparing children and chimpanzees, one is not seeking exact similarities, but clues to relevant capacities present in our common ancestor species four to seven million years ago.

On the subject of chimpanzee language capacities demonstrated in captivity, Hauser notes that our new data have not yet met the kind of critical treatment showered on earlier work. The reason may be that our research has used the earlier criticisms (Terrace et al. 1979) to avoid methodological pitfalls. Thus, for example, we have (1) eliminated all imitations from our data analysis of bonobo combinations (Greenfield & Savage-Rumbaugh 1990; 1991), (2) we have analyzed the structure of the input language that Kanzi was receiving from his human caregivers (Greenfield & Savage-Rumbaugh 1990; 1991), and (3) we have published our entire corpus of two-symbol combinations plus the order-governed three-symbol combinations (Greenfield & Savage-Rumbaugh 1991).

In short, although the target article merely summarized data and selected certain examples for illustration of key points in the argument, a full description of methods and findings is available for scrutiny by others. In "show-ering" such critical treatment on our research, one must be careful not to impose a double standard; one must apply the same critical standards to the evidence for grammar in human 2-year-olds. The fact that humans later develop full-blown syntax, whereas chimpanzees do not, should not be a reason to bias the comparison of the early stages. Again, the claim is not that all of human language evolved before the existence of *Homo sapiens*, but only that certain rudiments did, and that those rudiments then formed the foundation for further development during the evolution of *Homo sapiens* and *Homo sapiens sapiens*.

This point is also relevant to the laundry list of properties that Bloom feels are more relevant to human language than hierarchical structure. First, the central point of Chomsky, in his critique of Skinner's (1957) *Verbal behavior*, was that associative chaining could never explain human language because of the hierarchical organization implied by embedded structures. Since that time, hierarchical organization has been considered not simply one in a list of design features of human language, but a critical attribute.

Moreover, the *rudiments* of a number of the capacities mentioned by Bloom have, contrary to his assumption, been found in chimpanzee language: protosyntax and its

invention (Greenfield & Savage-Rumbaugh 1990; 1991), primitive signed inflections (Gardner & Gardner 1989), spontaneous thematic (i.e., semantic) relations in two- and three-symbol combinations (Greenfield & Savage-Rumbaugh 1990; 1991).

R3.2. Primate vocal communication

Why has so little attention been paid to the natural vocal systems of nonhuman primates, asks Hauser. These systems are difficult to study in the chimpanzee, although important attempts are being made (Boehm 1989; Goodall 1986). Although the pragmatic aspects of such communication have been placed in comparative perspective in a recent paper (Greenfield & Savage-Rumbaugh, in press), what is known so far suggests that a more promising avenue for structural origins lies in naturally occurring gestural communication, discussed in the target article. Evidence concerning the neural localization of sign, discussed above, along with the equipotentiality of human infants toward learning sign and spoken language (most recently argued by Pettit & Marentette 1991b), indicate that this may be a valid choice.

Another possibility is that we have not yet been able to decode vocal signals made by a creature with a very different vocal apparatus. The recordings brought back from Gombe by Boehm (1988) are extraordinarily difficult for the average (or even trained) human being to decipher, although sound spectrography certainly helps. In contrast, video tapes of chimpanzee gestural communication from Ivory Coast made by Boesch and Boesch (1990) are much more familiar and quite easy for a human being to understand. In terms of what is thus far known, the structural parallels to human language of chimpanzee gestural communication have seemed much more striking, although the pragmatic parallels between chimpanzee and human vocalization have also been clear.

Kanzi and the other bonobos in Savage-Rumbaugh's language project vocalize much more frequently than they use lexigrams (Savage-Rumbaugh, personal communication), and they appear to her to have an extraordinarily complex vocal system of communication. Some of their vocal signals are in fact used in the human-chimpanzee communication that goes on at the Language Research Center where they live; one example of the interpretation of a bonobo vocal signal by human beings is presented in Greenfield and Savage-Rumbaugh (in press). By and large, though, the study of these vocalizations awaits the development of analytic methods that can describe a vocal communication system that sounds very different from that of human beings. Until that is done, we have no way of knowing whether the basic structure of the system is also so different, or simply that the vocal realization of the structure is foreign to a creature with the human vocal and auditory apparatus.

R3.3. Where to look for neural homology between human and primate?

It is suggested by Hauser that Broca's area in nonhuman primates is used for object manipulation but not vocal production. The target article presented evidence for this point from the neural circuitry of macaques used for object manipulation (sect. 5.5). One can also agree with

Hauser's point that the Broca's homologue is not used to control vocalization in monkeys (e.g., Ploog 1988). Because evolution basically "tinkers" with the material already available, it is quite conceivable that the same neural structure would be used for different purposes at different times in evolutionary history. Nonetheless, negative evidence of the absence of homology in such distantly related species is difficult to interpret because such homology could still be present in more closely related species such as chimpanzees.

On the other hand, to respond to a related issue raised by Hauser concerning the leap from *Macaca* to *Pan*: Positive evidence of a common neuroanatomical structure in more distantly related species (macaques and humans in the present case) has a clear interpretation. That structure is almost certain to be present in members of two closer specimens: macaques and chimpanzees in the present instance.

R3.4. Has the hierarchical complexity of chimpanzee tool use and tool construction been underestimated in my account?

This is the claim made by Gardner & Gardner, as well as by Anderson. Underlying these criticisms seems to be a different definition of object combination and hierarchical complexity than the one developed in the target article. Gardner & Gardner use an example of subtractive tool construction – stripping off leaves from poles to make termite fishing poles – as an example of greater complexity than spoon use. This is certainly more complex than spoon use in some dimension, but, according to the analysis presented in the target article, it is not an example of object combination at all. Similarly, Gardner & Gardner cite the important case of an orangutan (Abang) taught to use a tool to make a tool: Abang learned to strike a flint core with a crude stone hammer to create a flake, then used his flake tool to cut a cord, opening a box. This, however, is also subtractive rather than additive (or combinatorial) tool construction. From my structural perspective, therefore, Abang's chain of actions is but a series of pairings. Note, nevertheless, the important quality of role reversal in this example: The stone flake shifts from passive object of the hammer's action to active instrument in relation to the cord. Role reversal is a structural complexity often associated with subassemblies (see Figure 3, subassembly method, in the target article).

As for termite fishing, the required motor skills are, according to my analysis, not directly relevant to the assessment of hierarchical complexity in object combination. In response to a question raised by Frigaszy, as well as by the Gardners, the analysis of termite fishing, like that of ant fishing (sect. 5.2), is that it is a borderline subassembly. That section also analyzed the leaf sponges described by Goodall (1986) as borderline subassemblies. In both cases, two objects are being combined (spoon plus food, leaf plus water) to form a subassembly, which is then combined with a third object, the mouth.

The term "borderline subassembly" is also used when there is uncertainty about the extent to which the subassembly is created by the chimpanzee's actions as compared with the actions of the termites or ants jumping on the stick or the automatic action of the water adhering to

the leaf. If the chimpanzee does not do anything to create or recognize the subassembly then **Fragaszy** is right that termite fishing would be a sequence of pairing actions. For this same reason, **Anderson's** example of the use of a stick to get honey, which automatically adheres to the stick, sounds like the use of the pairing strategy.

Even if we admit these examples as full subassemblies, however, they are still at the same level of hierarchical complexity in my theoretical scheme as spoon use. I therefore cannot agree with **Gardner & Gardner** that leaf sponges are at a higher level of hierarchical complexity than the simple subassembly posited as the chimpanzee limit. Based on my analysis of chimpanzee tool use, I would agree with **Anderson** that chimpanzees are capable of constructing full-blown subassemblies. Both **Anderson** and the **Gardners** provide a wonderful example of this from **Köhler (1925)**: A chimpanzee fits two sticks together to form a sufficiently long raking tool. **Gardner & Gardner** also point to the interesting filmed example of the cross-fostered chimpanzee **Viki**, who threaded a needle and then used the resultant subassembly to sew cloth. It is interesting that the subassembly strategy was clearest in chimpanzee tool use in captivity where the small-object environment would be richer than that of the African forest.

R3.4.1. Matsuzawa's new developmental tool use data from wild chimpanzees in Guinea. Unique comparative, developmental, and experimental data from wild chimpanzees and humans in Bossou, Guinea, are added by **Matsuzawa** to the discussion of the maximum level of hierarchical complexity achieved in chimpanzee tool use and tool construction. In a nutcracking situation, an older chimpanzee was observed to take a pair of stones for hammer and anvil and then a third stone to keep the surface of the anvil flat. A nut was then placed on the anvil and struck with the hammer stone. According to my analysis of the hierarchical organization of this object combination, this is a borderline subassembly: The chimpanzee combines two stones to construct an anvil, a subassembly that serves as the common object for nut and hammer. This subassembly is borderline because the complex anvil fails **Reynolds's** criterion of being able to be rotated in space (but see discussion on this point below).

Most important for the present model are **Matsuzawa's** developmental data on tool use and construction in wild chimpanzees, the first developmental data on chimpanzee tool use to be collected in the wild. Under a certain age, both chimpanzees and Guinean children were limited to a pairing strategy. In cracking palm-oil nuts, they would include only two objects out of the triad, anvil stone, nut, and hammer stone. For example, they would strike a nut with a hammer stone, but no anvil. Or they would strike a nut on the anvil by hand, without a hammer. They even used the pairing strategy of repeatedly combining the same two objects in a reduplicated sequence of repeatedly placing nut on anvil. At age 3 (human children) or 4 (chimpanzee), the nutcracking behavior reaches the "pot" stage: A nut is placed on an anvil stone (the "pot"); then a hammer stone strikes the nut and anvil. It was only an older female chimpanzee and an 11-year-old boy who constructed a complex anvil. These ages support the possibility that the complex anvil

is conceptually functioning as a subassembly. Without more details as to the construction process than are provided by **Matsuzawa**, it is hard to decide this point. In any case, the significance of these observations is that the same developmental sequence from pairing to pot and then to (borderline) subassembly occurs in chimpanzees and children. This commonality in developmental sequencing strengthens the phylogenetic part of my model in an important way.

R3.5. Relationship between ontogeny and phylogeny

Because, according to **Deacon**, the target article narrowly escaped recapitulationism, it is probably not necessary to express agreement with his arguments against this ideology. Sometimes the empirical facts conspire to give a recapitulationist look to the data, however. Thus, the prefrontal areas develop last in children and also have smaller relative size in chimpanzees and other primates. The use of the idea of differentiation, criticized by **Deacon** as recapitulationistic, was actually an attempt to avoid naive recapitulationism. Nor can one disagree with **Deacon's** point that human and chimpanzee brains pass through similar stages of differentiation.

Although the anatomical development may look similar on a qualitative level, however, there is a quantitative difference, as **Gibson (1990)** states, stemming from the difference in brain size: "Specifically, increasing numbers of neurons and increasing numbers of connections per neuron automatically result in increased differentiation of sensory and motor units and hence in increases in the numbers of discrete sensory and motor behaviors possessed" (**Gibson 1990**, pp. 99–100). The ontogenetic process of one neural area differentiating by forming more discrete long-distance connections to different parts of the brain may be quite parallel in the neural development of humans and other primates. The process cannot go as far in creatures with a smaller brain, however, because, as **Gibson** implies, there are a smaller number of neurons and connections doing the same work. There is hence a recapitulationistic "look" to the pattern: Neural differentiation is less in young than in mature human beings; neural differentiation is less in mature nonhuman primates than in mature human primates. Logic therefore tempts us to equate young humans with mature nonhuman primates, but **Deacon's** commentary reminds us that neural differentiation is also less in young nonhuman primates than in mature primates of the same species. It is this fact that keeps the recapitulationistic "look" of the data from becoming a recapitulationistic ideology and fallacy.

Deacon makes a second point against differentiation, saying that the issue of symbolic/manual differentiation is not relevant to the bonobo case because **Kanzi's** symbol system is also manual. I would disagree. To agree with that point would entail making the claim that the manual and symbolic domains of deaf signers were less differentiated than those of hearing speakers. **Reilly et al. (1985)** make the point that, in the course of ontogeny, the facial expressions of deaf signers, used syntactically in sign language, differentiate from the same facial expressions used for emotional expression in deaf as well as hearing people. The modality of the facial expressions is the same in both cases, but they develop into different functional

systems; each may well have its own neural underpinnings. One would expect the circuits shown in Figure 11 of the target article to develop similarly in deaf signers as in hearing speakers. Indeed, the theoretical postulation of these circuits could even explain why facial expression is used syntactically in sign language: because the connections from Broca's area to the orofacial motor cortex shown in Figure 11 could be epigenetically adapted to facial expression rather than to speech. In other words, speech would use the "oral" aspect of the orofacial area, whereas sign would use the "facial" aspect.

In addition, the manual motor cortex is next door to the orofacial motor cortex, which would facilitate connections from Broca's to this area for the manual aspects of sign language. The prediction would be that, in deaf signers, the functional area of the manual cortex expands to take over some of the space of the orofacial area in hearing people and that this appropriated space is used for the manual aspects of sign language. The development of inputs from contrasting areas of prefrontal cortex would lead to the same differentiation of Broca's area. What remains an open question is whether the two parts of Broca's area resulting from its differentiation send their input to a single homogenous, albeit larger, manual motor cortex or to a manual motor cortex that, in deaf signers only, has functionally differentiated into two areas, one for sign, the other for object manipulation. In this connection, it would be interesting to know whether the same part of the manual motor cortex is used for gesture and object manipulation in hearing people. The answer to this question might well provide the answer to the question about the differentiation of the manual motor cortex in deaf people.

Clearly, the above theory of cortical circuits in deaf sign would conflict with Deacon's suggestion that the question of neural differentiation of symbolic language and manual object combination is irrelevant to the bonobo case.

R4. Conclusion

In light of the commentaries it is exciting to see how much and how diverse a set of additional data is predicted or explained by the theoretical model presented in the target article. These data come from domains as diverse as human sign language development, Williams syndrome, the ontogeny of human spoon use, nesting cup behavior in captive chimpanzees, and tool use in wild chimpanzees.

On the other hand, the commentaries also present challenges to the theory, data-based challenges being especially significant (as compared to theoretical or logical ones). The most serious empirical challenges posed by the commentary concern (1) the possibility of significant asynchrony between the emergence of the subassembly stage of word formation and the subassembly stage of object combination (Tomasello) and (2) the simultaneous emergence of many sound combination patterns in early babbling before the corresponding object combination skills (MacNeilage).

Both these challenges should now be resolved by further research. The first requires more fine-grained neurophysiological methods that can determine if in fact language circuits develop a bit earlier than the corre-

sponding manual circuits. New EEG techniques with many more cortical leads may be extremely useful in answering this type of question. The second challenge requires neurophysiological techniques to determine whether Broca's area adds an additional level or type of neural control to the supplementary motor area just at that point where babbling begins to turn into words. More generally, it will be important to investigate other neural circuits that may be implicated in the language and object skills that have been discussed. The proposed circuits should not be construed as the only ones that are operative in either grammatical speech or manual object combination.

Together, the theory and the commentary on it suggest additional research in many directions. I am particularly looking forward to collaborating with neuroscientists to fill in missing pieces in the model of neural development and trying eventually to establish direct empirical links between neural development and the behavioral development of language and tools. I also hope that other investigators will be able to use my approach to neural development and its behavioral consequences to explore other areas of behavior, language comprehension *inter alia*.

In investigating the neural foundations of the development of language and tools, however, one must remember that language and tools are not merely biological phenomena. As the very foundations of human culture, language and tools are part of both the human environment and the human biological endowment. Each stage of neural development sets the stage for certain interactions with the sociocultural and physical environment, which, in turn, leave their marks on both brain and behavior in an epigenetic process. This must be as true for phylogeny as it is for ontogeny. Understanding the reciprocal influences of environment and neural development is an important goal for the next stage of theory and research.

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Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively.

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