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Commentary on **Patricia M. Greenfield (1991). Language, tools and brain: The ontogeny and phylogeny of hierarchically organized sequential behavior. BBS 14:531–595.**

Abstract of the original article: 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.

Toward a neurology of grammar

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Abstract: This commentary makes a case for a connection between the hierarchically organized skills emphasized in Greenfield's (1991t) target article and rhythmic skills utilized in music. It also links hierarchical organization with automated processing. Implicit is the notion that lower levels of a hierarchy become automatic, as they go under control of higher levels of organization.

Preamble. Greenfield's (1991t) paper is an important contribution to our understanding of two related aspects of the evolution of human language via some nonlinguistic (or prelinguistic) preadaptation:

(a) Ontogenetic: the early existence in the human child of prelinguistic brain-structures and cognitive-behavioral patterns that may serve as such preadaptations in the ontogenesis of human language.

(b) Phylogenetic: the possible existence of the same preadaptations in the direct ancestors of *Homo sapiens,* thus in the phylogenesis of human language.

I think Greenfield makes a very good case, in her survey of both the neurological and developmental literature, for Broca's area being of a crucial brain location for the ontogenetic development of complex rhythmic-hierarchic behavioral skills, skills that either pre-date language or are independent of it. The comparative primatology literature she cites, while less extensive, points in the direction of a similar phylogenetic development. If both developmental aspects hold up, one may have here another significant instance of recapitulation. The remarks I make below are meant to amplify and perhaps extend the import of Greenfield's (1991t) contribution.

The scope of Broca-dependent skills. Tool use is probably not the only nonlinguistic function that may depend on Broca's region. Many other complex, rhythmic-hierarchic, higher-cognitive behavioral skills may do so. Greenfield herself surveys her earlier work in Greenfield and Schneider (1977), where it is shown that the acquisition of grammar and complex visual hierarchic structure between 3 and 6 years of age go hand in hand in normal children. Grossman's (1980) finding has a similar import, showing that the same left-hemisphere lesions in Broca's area that impair grammar also impair the processing of hierarchic visual information. In addition, Robinson and Solomon (1974), Poeck and Huber (1977), and Ibbotson and Morton (1981) show that the processing of rhythmic information is left-hemisphere localized, and thus by inference perhaps Broca dependent.

And Lea (1980) shows that impaired rhythmic-musical abilities in school children go hand in hand with impaired language abilities. Similarly, Martin (1972) and Robinson (1977) review evidence for a shared neurological capacity for the processing of speech and rhythm. Music performance is indeed a complex rhythmic-hierarchic skill par excellence. It is perhaps the closest analog – thus potentially dependent on a neural homolog – of the syntactic structure of language (cf. Balzano 1982, 1986; Clynes 1982; Lerdah & Jackendoff 1984; Mitchell 1971; Sloboda 1977; Wolf 1976). Similarly, complex motion routines (cf. Schmidt 1975; 1980; Shapiro 1977; 1978; Shapiro & Schmidt 1980; Shapiro et al. 1980) are rhythmic-hierarchic skills that may also be Broca dependent. Tool use may thus be merely a subset of a large area of prelinguistic behavior that shared important structural features with human grammar, and may thus have been both its behavioral and neurological precursors.

Automaticity, grammar, and Broca's. Another important point implicit in Greenfield's paper is the connection between complex rhythmic-hierarchic behavioral skills and automated processing. In the ontogeny of language – both L1 and L2 – as well as in its likely phylogenetic evolution, the shift from the pregrammatical "pidgin" processing mode to grammatical-syntactic processing most likely involves the development of automated processing of discourse (cf. Blumstein & Milberg 1983; Givon 1979; 1989;

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Lieberman 1984; Schnitzer 1989). [See also Epstein et al.: "Second Language Acquisition" *BBS* 19(4) 1996; Bickerton: "The Language Bioprogram Hypothesis" *BBS* 7(2) 1984.] One may further argue that hierarchic structure and automated processing tend to go together (cf. Chase & Ericsson 1981; 1982; Ericsson 1985). Thus, it is only to be expected that other hierarchically organized complex skills are likewise Broca dependent.

One minor clarification here concerns Greenfield's use of "planning" in the sense cited from Keenan-Ochs (1977). In both Keenan-Ochs (1977) and Givon (1979), it was observed that well-planned and edited written discourse is the most syntacticized – grammaticalized – mode of human communication. But the planning that is involved here is conscious. In contrast, grammar, as a well-habituated, routinized, automated language-processing mode – much like skilled piano playing, skilled bicycle riding, skilled dance routines, etc. – is an unattended, automated activity. The use of this skill is just as evident in spoken language as in the written register. Keenan-Och's "planned" may thus be a bit misleading in the context of Greenfield's discussion.

Hierarchic vs. automated. A final minor point concerns Greenfield's use of the term "hierarchic." Her examples of child sentences are indeed hierarchic, but nonetheless pregrammatical. One can find such "hierarchic" structures already in both child pidgin and second-language pidgin (cf. Givon 1990). Moreover, multi-propositional discourse, even when processed in the pregrammatical "pidgin" mode, is itself hierarchically structured (cf. Mandler 1978; Mandler & Johnson 1977). It is only the grammatical mode of language processing, however, that is clearly Broca dependent. In contrast, the comprehension and production of coherent multi-propositional discourse seems to depend more on Wernicke's area. This is true regardless of the hierarchic structure of both.

One may venture a guess that the acquisition of grammatical, automated processing of language kicks in when enough development of hierarchic discourse structure (perhaps also of hierarchic clause-structure) has taken place at the pregrammatical "pidgin" stage, so that the time-saving resulting from routinization and automaticity become a significant adaptive benefit. Frequent access and thus informational predictability are indeed the driving force of automaticity elsewhere (cf. Posner & Warren 1972; Posner & Snyder 1974; Schneider & Shiffrin 1977). Broca's area, one may perhaps conclude, is not at all involved in the processing of slow, laborious hierarchically structured behavior, of either pregrammatical clauses or pregrammatical discourse. Rather, it may kick in as a significant processing component only when the tasks are automated, routinized – that is, grammaticalized.

Greenfield on language, tools, and brain

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Abstract: Greenfield (1991t) fails in an attempt to defend her own original synthesis of cognitivist and nativist accounts of language development. The proposed synchronous stages of object and phoneme combination are not supported by the empirical data she presents. The functional specification of hypothetical neural circuits is almost entirely speculative. Nor is it likely that new data could save her model, since it is formulated in a simplistic information processing framework that is now of little more than historical interest.

1. Introduction. Greenfield (1991t) has made an ambitious attempt to provide a psychobiological account of some developmental patterns identified from observations of young children's object manipulation and early speech. The target article reflects a new interdisciplinary approach to the study of the development and historical origins of human language, an approach that is attracting a wide range of researchers: this is evident in recent important conference transactions, notably Wind et al. (1991) and Gibson and Ingold (1993; see also Parker & Gibson) as well as several books (Bickerton 1990; Corballis 1992; Liberman 1991a). [See also Bickerton: "The Language Bioprogram Hypothesis" *BBS* 7(2) 1984.] Greenfield exploits a great variety of previous research and her commentators draw on numerous additional publications; the result is a dense and difficult text in which details of data analysis tend to obscure the radical ideas. Indeed, in the conclusion to her response, Greenfield notes (p. 588) that in the commentaries "data-based challenges [are] especially significant (as compared to theoretical or logical ones)." In this contribution to continuing commentary, I shall try to redress the balance by focusing on theory and logic. The rest of this section contains a brief overview of Greenfield's main claims and the evidence she presents in their support; in subsequent sections I evaluate in detail the key parts of her argument.

In earlier work, Greenfield et al. (1972) investigated the development of complexity in young children's manual combination of various objects, especially sets of nesting cups. This led them to claim that a universal developmental sequence for this behavior is determined by an innate mechanism. They also noted the analogy between combining objects and combining spoken words and speculated that this might be a homology, that is, the result of a common neural mechanism programming both modalities. A related study by Greenfield and Schneider (1977) introduced the task of copying tree structures to study older children's developing mastery of hierarchical structure. The same task was used by Grossman (1980) to show that adult Broca's aphasics may also be impaired in their ability to reconstruct or copy tree structure: he suggested that there was a common supramodal hierarchical processor for both speech syntax and manual object combination tasks.

It was Grossman's work that apparently inspired Greenfield to look for more neurological evidence for homology. She found various studies that have located motor sequencing for right manual and speech control close together in or around Broca's area (Brodmann's areas 44 and 45), but also one study (Curtiss et al. 1979) that showed a dissociation between the two skills. Using as a guide Deacon's (1988; 1992) account of neural circuit development in monkeys, Greenfield proposes that Broca's area starts out in human infants as an undifferentiated hierarchical programmer providing output via two circuits to the left hemisphere orofacial and manual motor regions. The resulting language, limited to 2-3 word combinations with minimal morphosyntax, would correspond to what Bickerton (1990) has called protolanguage. Late in the second year of life neural circuits develop that connect Broca's area forward to two distinct prefrontal cortex areas, one specialized for grammar (Brodmann 46) and the other for manual sequencing (Brodmann 9). In the central part of her target article, Greenfield seeks behavioral and neurobiological evidence for this developmental model. On the one hand, she tries to establish precise structural parallels between the development of children's object manipulation, exemplified by nested cups (Greenfield et al. 1972) and spoons (Connolly & Dalgleish 1989), and their early speech (her own unpublished diary data). On the other hand, she uses two studies of brain development in children (Thatcher 1991a and unpublished data, Simonds & Scheibel 1989) as evidence for the growth of the proposed circuits.

The foregoing account of individual development (ontogenesis) is, Greenfield argues, compatible with the following scenario for the evolutionary emergence (phylogenesis) of language in hominids. Tool use and gestural protolanguage originated in the nut-cracking behavior of a common ancestor of humans and other primates. This behavior would be transmitted from mother to infants by explicit pedagogy, as has been observed in wild chimpanzees (Boesch & Boesch 1990). Both the tool use and the gestures would have been supported by a bimodal hierarchical motor control system located in a homologue of Broca's area. In the hominid line, the tool use, pedagogy and gestural language would have then developed further through "mutually reinforced

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natural selection" (p. 549) and pulled human brain evolution in the direction of an expanded prefrontal cortex. This new cortex would contain the complex grammatical module that distinguishes human language from that of chimpanzees, who have remained at the prehominid evolutionary stage corresponding to the protolanguage stage in human children. We would thus expect to find cross-species structural equivalences, with the prediction that chimpanzees are unable to progress beyond protolanguage.

Although this speculative and nativist theory of language origin occupies a good deal of space in the target article and commentaries, Greenfield has no real evidence to support or falsify it. As Lewontin (1990) points out, short of time-travel we will probably never know the combination of factors that led to the emergence of higher cognitive functions in humans; the information we would need just is not there anymore. I will therefore limit my attention to Greenfield's account of ontogeny, since this is at least in principle susceptible to empirical investigation.

Like many other contributors to BBS, Greenfield is deeply influenced by mainstream cognitive science in the Fodor-Chomsky tradition, while making extensive use of data and ideas from disciplines that speak a very different scientific language. This leads to persistent problems of interpretation. Taken literally, her claims amount to an absurdly strong form of nativism according to which the development of children's speech and object combination is as genetically constrained as is the growth of their teeth. If, on the other hand, her claims are taken as a temporary conceptual framework to integrate work from many disciplines, or as a sketch of what a general theory might one day look like, then her paper is a more modest but certainly more useful contribution. The problem is that Greenfield adheres strongly to a literal interpretation, while her commentators take her more or less literally according to the point they want to make. For example, Bloom (1991, p. 553) writes that Greenfield "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." In contrast Gibson (1991, p. 560) believes that "Greenfield's formulation provides a major breakthrough in our methods of approaching the evolution of the human brain." In most of what follows I shall take Greenfield literally and confirm Bloom's negative reaction to her specific proposal.

2. Hierarchically organized sequential behavior in children. The description of sequential behavior as hierarchically organized has proved a fruitful tool in the analysis of natural and artificial systems. This led Dawkins (1976) to follow Simon (1969) and propose "hierarchical organisation" as a general principle for ethology; some researchers (e.g., Fentress 1981) have tried to operationalize the suggestion. Many linguists have followed Chomsky (1956) and taken hierarchical organization as the defining characteristic of natural language syntax, but there are problems both with elevating the intuitive concept to the status of general principle and with its use in specific empirical research. On the one hand, hierarchical organization can be found in (or perhaps read into) a very wide range of phenomena. On the other hand, other organizing principles nearly always have to be taken into account. This has led most empirical researchers to pay only lip-service to the concept or to use it as a temporary place-holder. In computer science, for example, hierarchical structures have played much less of a role than one would have expected after reading Simon's essay. In linguistics, interest has shifted away from the word order and constituent cooccurence phenomena captured by simple trees toward more complex and nonhierarchical relations between words. One can conclude that Greenfield is facing an uphill struggle in proposing such a principle as a source of unification in cognitive neuroscience.

Greenfield's goal is to demonstrate that children's language and object combination develop synchronously in a structurally parallel sequence during the period from 9 to 20 months. Her starting point is her own work with nested cups, which she believes established a universal and genetically determined sequence of developmental stages (pair, pot, sub-assembly) for strategies of object manipulation. In the target article she adds data from Connolly and Dalgleish's (1989) study of spoon use in children between 12 and 23 months and, for her reply (Greenfield 1991r), fresh data of her own on spoon use. In these cases of simple object manipulation there is a natural description in terms of hierarchical task structures. For example, in the manual syntax for "sub-assembly" two objects are combined and then applied to a third: a spoon is filled with food and then put into the mouth; cup 1 is put into cup 2 and then the combination is put into cup 3, and so forth. Such manual sequences are presented as having the abstract tree structure ((*object1 object2*) *object3*).

During the period under consideration, most children's language is limited to associations of one or two words with almost no hierarchical structure, so object manipulation is in advance of syntax. Greenfield cannot, therefore, use the obvious analogy between verbal syntax and action grammar. Instead, she tries to map the object combinations into the *individual phonemes* of early words. The actual mapping she proposes (in sect. 3 of the target article) is based entirely on her analysis of her own unpublished diary data for the speech of three children. Her exposition involves five stages of phonological complexity, four of spoon use and three for nested cups and is extremely difficult to follow. I have summarized it in Table 1.

There are so many problems with Greenfield's proposal that I shall not try to cover them all here. A number were raised by commentators: the limited data base, the lack of clear metrics for complexity, the poor correspondence in chronology, the apparently arbitrary choice of units, and so on. One problem that was surprisingly not raised in the original discussion is that Greenfield does not present her child language data in the form of phonetic transcriptions, leading to the suspicion that the original data is in English spelling and thus lacking any precise phonological information (for example, did her 15-month-old subject really articulate the final 'l' in 'ball'?). A far more serious, indeed fatal, weakness is her starting assumption that children build their early words from phonemes in a process of hierarchical construction. Greenfield offers no evidence at all for this surprising claim, for the simple reason that there isn't any. As all parents know, children's early words are largely holistic approximations of the most salient syllable in the target. Indeed, it seems highly unlikely that word formation is ever generative, even in adults, in the strong sense that Greenfield assumes.

This last point was made by MacNeilage (1991) in his commentary, but Greenfield's opaque response in her reply (Greenfield 1991r) indicates that she misunderstood it. MacNeilage also showed that the types of phoneme combination proposed by Greenfield can all be found in prespeech babbling, where they emerge in no particular order. In addition, he finds no evidence for syllable-internal combinatorial procedures during babbling. Greenfield concedes that this presents something of a problem for her model, but claims that the data are in fact irrelevant because babbling differs from early speech in that it is controlled by a different neural system (the supplementary motor area [SMA] as opposed to Broca's area) and is meaningless sound. [See also Goldberg: "Supplementary Motor Area Structure and Function" *BBS* 8(4) 1985.] The function of babbling in the transition to early speech is not yet understood, but what is known suggests a much closer relation than Greenfield admits. But even if the two systems were completely independent, MacNeilage's demonstration that the phoneme strings of babbling are largely noncombinatorial underlines the implausibility of Greenfield's assumption that early words are, in contrast, produced by combining phonemes.

In his commentary, Tomasello (1991) shows that by using words as units rather than phonemes a simple analogy can be constructed between the classical one-, two-, and three-word stages and Greenfield's pairing, pot and subassembly strategies for object manipulation. Greenfield summarizes his analogy and replies as follows:

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> 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. (p. 582)

It is clear, I think, that Greenfield's analogy involves far more forcing and mixing than does Tomasello's! The analogy with words also produces a better temporal mapping between the two domains, eliminating the five-month *décalage* in Greenfield's data between the emergence of subassembly in phoneme combination (at 15 months) and in object manipulation (at 20 months). In her reply, Greenfield highlights the weakness of her model by suggesting that the *décalage* could be a result of individual differences in rate of development *or* differential environmental stimulation *or* an actual time lag between the development of the hypothesized circuits for the two domains. Admitting any of these as an explanation for such a large *décalage* (five months!) is practically equivalent to admitting that there is in fact no significant temporal correlation between developmental stages in the two domains – other than what would be expected from more general maturational constraints.

3. Greenfield's model of brain development.

3.1. Greenfield's model. Greenfield offers a developmental system-level account of the brain areas she claims subserve the hierarchical structuring of object combination and speech. She proposes two main stages. In *Stage I* (roughly between 9 and 20 months), Broca's area acts as a single unit programming the adjacent manual and speech motor areas. At this stage Broca's is the "highest level" programming area controlling manual and speech output. It is also an "undifferentiated" neural region. Greenfield's idea is that Broca's area would have (or generate) simple syntactic patterns that can subsequently be interpreted as action programs by either the manual or speech motor areas. The undifferentiated character of Broca's area would, among other things, result in "conjoint non-dissociable movements of hands and mouth" (p. 543). Within Stage I there is a progressive increase in the hierarchical complexity of the syntactic patterns produced by Broca's area and the resulting motor outputs; this determines the observed behavioral sequence of pairing, pot, and subassembly.

In *Stage 2* (after 20 months) Broca's area participates in two new neural circuits connecting it to two areas in the anterior prefrontal cortex. The first of these areas controls "grammar" (Brodmann's area 46) and the second (Brodmann's area 9) controls "manual object combination." Broca's area would thus lose its independent top-level control function and be subordinated to control by these two anterior areas: "the early circuits constitute subprocesses of the more mature circuits" (p. 548). Greenfield does not offer much detail regarding area 46, but she clearly views it as an amodal grammatical module responsible for most of what is considered linguistic syntax. Her view appears, in fact, to correspond closely to Chomsky's idea of language competence: an innate, declarative, and linguistically specific grammar module unique to human beings. She is even less specific about area 9, but appears to consider it as providing analogous manual competence. Her commitment to a strong nativist theory is quite explicit: the grammar that a child utilizes from two years on was developed by natural selection, is stored genetically, and unpacked into a specific cortical module (area 46) where it determines the development of complex structure in verbal output.

Before we consider the evidence Greenfield offers for her model, some general comments are required. To begin with Stage 2, it is self-evident that object manipulation and speech are under functionally distinct neural control in children after 20 months, and there is no reason to doubt that prefrontal areas are involved. This logically requires connections between the various areas mentioned by Greenfield and we have every reason to expect that when neurobiologists find a way to trace them in humans these connections will look something like the circuits Deacon (1991) describes in monkeys. In this general sense, Greenfield's claim that such circuits are in place by about 20 months is no doubt correct. But, of course, the role that such circuits play in the functional organization of the brain is still unknown; they transmit information, but what that information encodes and how it is transmitted it still a mystery. There is therefore no justification for a rigid mapping of specific high-level computational functions onto specific physical circuits, least of all in the frontal lobes. In addition, the four circuits hypothesized by Greenfield represent only a tiny fraction of the circuits normally implicated in language production and it is hardly plausible that hierarchical structuring should be functionally localized on just one of them.

Table 1 (Swann). *Greenfield's account of hierarchical structure in children's phonology, play with nested cups, and spoon use*

1. Pairing
1.1.
Phonology – 8 months: CVCV (dada, mama) reduplication.
Cups – "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)" (Greenfield 1991, p. 538).
Spoon – spoon in/out of mouth/dish (spoon stage 1 = phonological stage 1).
1.2.
Phonology – 12 months: CV . . . CV (first words).
Cups – "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" (p. 539).
1.3.
Phonology – 12–16 months: CV1CV2 (daddy, baby) consonant harmonization (pairing).
Cups – 12 months: hold one cup and (without letting go) place it in/on a second cup and then in/on a third cup.
Spoon – spoon touches food, spoon touches mouth (spoon stage 2 = phonological stage 3).
2. Pot
Phonology – 15 months: C1VC2V (tinky, kye bye) vowel harmonization.
Cups – "becomes dominant at 16 months": pot strategy: "the initial moving cup varies while the 'pot' with which each cup combines remains constant" (p. 539).
Spoon – spoon pot stage (not observed by Connolly & Dalgleish, but observed by Greenfield for her reply): spoon in mouth with right hand + food in mouth with right hand (after switching spoon into left hand).
3. Subassembly
Phonology – 15 months: "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 . . .) and/or making a two-word sentence out of two previously constructed sound combinations" (p. 539).
Cups – 20 months: subassembly: "at least one previously constructed subassembly of cups functioned as a unit, combining with another cup or subassembly of cups" (p. 539).
Spoon – (spoon + food) into mouth.

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Greenfield's account of Stage 1 is even more problematic. First, almost nothing is known about the development and functional specialization of Broca's area in the human infant brain. Second, the specific claim that she makes seems wildly improbable: examples of "conjoint non-dissociable movements of hands and mouth" in toddlers are striking precisely because they are so unusual. And Greenfield's sketch of infants as engaged in "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" (p. 550) appears downright perverse. Indeed, an active, exploratory toddler seems to be the antithesis of the classic patient with frontal lesions. Now, precisely because the behavioral evidence does not support the model, Greenfield would have to find neurobiological evidence for the "undifferentiated" state of Broca's area in toddlers. But, as I shall show in the next section, while the neurobiological evidence she presents can be interpreted as being not inconsistent with the general ideas behind her account of Stage 2, it tells us absolutely nothing about the functional organization of Broca's area during Stage 1.

3.2. Evidence for the model in the target article. As can be seen, Greenfield adopts an information-processing style of model for her system level account of the relation between brain and language. She identifies various functional modules that exchange information via pathways. The modules are then localized in specific cortical regions and their interconnections identified with specific neural circuits. At this level of description, Greenfield can cite neuropsychological studies conducted with adults and older children that suggest some kind of association between language and hierarchical constructive skills and their common location in the left hemisphere. She is accordingly able to conclude 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" (p. 536). This is hardly new or surprising and offers no support for the specific claims of her model.

To strongly support her Stage 1 model, Greenfield would ideally have to demonstrate a sequence of neurophysiological changes in Broca's area between 9 and 20 months that would correlate with the three behavioral substages of pair, pot, and subassembly. Rather weaker support would be provided by a neurophysiological demonstration that Broca's area is "undifferentiated" with respect to manual or speech programming. Clearly it will be several decades before such detailed evidence becomes available. Instead, Greenfield uses two neurobiological studies (Simonds & Scheibel 1989; Thatcher 1991a) as evidence for the developmental chronology of the four neural circuits she postulates as progressively connecting Broca's area to the motor strip (Stage 1) and to anterior prefrontal cortex (Stage 2). The circuits proposed as the mechanism for the transition to Stage 2 are assumed to act to differentiate Broca's area, and this is implicitly taken as evidence that the area was not functionally differentiated before the circuits developed. To make use of the neurobiological data, Greenfield adopts her own speculative account of the process of circuit formation, according to which "multiple short-range connections are 'pruned' to fewer, more specific, and longer-range connections" (p. 544). She is confident that "this is the process by which differentiated circuits are created" and that "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" (ibid.). She cites no neurobiological literature in support of these assertions (nor do the commentators).

After this buildup, the actual data to be found in the Thatcher (1991a) and Simonds and Scheibel (1989) studies are a big disappointment. Thatcher has data on the development of EEG coherence in children that he suggests probably correlates in some way with circuit formation. Simonds and Scheibel conducted a quantitative study of dendritic development in Broca's area and adjacent orofacial motor cortex in human infants from 3 months to 72 months. The data come from the study of brain tissue collected after autopsy and Simonds and Scheibel are extremely cautious about what generalizations, if any, the data support; they make no mention of circuit formation. Unfortunately, Greenfield presents the data from these two isolated studies in a way that suggests she is using a validated methodology, claiming that "the . . . data converge in providing information about developing neural networks" (p. 542).

Even if her methodology were valid, the data themselves are quite inadequate to provide a developmental history for the specific circuits, nor do they elucidate or support an "undifferentiated" Broca's area. This emerges clearly in section 4 of the target article, where Greenfield tries to put together the two sources of data. Averaging over only three individuals for an age range of 12–15 months, Simonds and Scheibel found that dendritic growth in the two left hemisphere areas sampled (Broca's area and the adjacent orofacial motor cortex) had increased to catch up with the more precocious corresponding right hemisphere areas. In addition, dendritic growth was significantly higher in the orofacial motor area than in Broca's area. Greenfield believes that these data are symptomatic of the formation of the circuit from Broca's area to the orofacial motor cortex. As for the second circuit of Stage 1, connecting Broca's area to the manual motor areas: "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" (p. 543). Averaging data from two individuals at 24 months and two individuals at 36 months, Simonds & Scheibel concluded that in the 24–36-month period dendritic growth in Broca's area had caught up with the adjacent orofacial motor cortex. Greenfield believes this indicates that Broca's area is receiving input from elsewhere from about 24 months. She suggests that the input is coming from anterior prefrontal areas and writes: "To test my prediction, Thatcher analyzed his cross-sectional data and found . . . a spurt of increased connectivity between approximately two and four years of age" (p. 543). She thus assumes that dendritic growth at point A plus increased EEG coherence between point A and point B implies a circuit going from B to A. Again this appears to be pure speculation.

It should be clear from the above that Greenfield has no evidence for the specialized circuits described in her model. Even if EEG coherence and dendritic growth were symptomatic of circuit formation in the simple and direct way she assumes, the data would still show nothing more than that connections form between Broca's area and other frontal lobe areas during the second two years of life. But surely it would be very surprising if this were not the case. The data tell us nothing about the function of these circuits or the functional organization of Broca's area before they formed. It should also be clear that there is no way to map Greenfield's cross-domain behavioral development sequence into the neurobiological data, nor does she attempt to do this. Finally, there appears to be a contradiction underlying the neat symmetry of the two pairs of circuits: in Stage 1 circuit formation leaves Broca's area undifferentiated, whereas in Stage 2 the same mechanism causes differentiation. I conclude that the proposed homology for Stage 1 is not supported by the neurobiological evidence.

3.3. Commentary and Greenfield's reply. Several of the commentators briefly discuss Greenfield's model and her use of the neurobiological evidence. Thatcher (1991b), who begins by noting that "the mechanisms of neural circuit differentiation are currently unknown" (p. 575), describes his own general theory of cerebral maturation but, unfortunately, makes no comment on the specific claims of the target article. Jacobs (1991), who has used the same methods as Simonds and Scheibel (and discussed the target article with Scheibel), notes that "what each investigative technique reveals about the brain and the degree to which these can be complementarily synthesized remains an open question." Deacon (1991), Fuster (1991), and Lieberman (1991b) all make interesting observations about the anatomy and development of various neural circuits, mostly in monkeys, but they offer no new

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evidence or support for the specific claims of the target article – nor does Greenfield's reply.

4. Conclusion. Greenfield fails almost completely in her ambitious attempt to use thin and patchy data to support an original synthesis of cognitivist and nativist accounts of language development. Her proposed synchronous stages for object and phoneme combination in children are not established by the data she presents. Her functional specification of hypothesized circuits is almost entirely speculative, while the neurobiological data she offers does not support the specification. Nor does it seem likely that new data or analysis could save her specific claims, since these are formulated in a simplistic information processing theoretical framework (language production as computation of structure, strong Chomskian nativism, Fodorian modularity . . . and so on) that surely now belongs to the past.

The general brain development model that emerges from the target article and the commentaries is, nonetheless, extremely interesting and suggestive. As Fuster formulates it, a caudal to rostral maturational gradient in the frontal lobes reflects a representational gradient from concrete motor output to more abstract and hierarchically structured plans. Greenfield makes a major contribution in associating this maturational process with Bickerton's idea of the transition from protolanguage to complex grammar. But her nativist belief that "brain development drives language development" (p. 550) is untenable. Language acquisition is not something that merely *happens* to children, it is something they actively *do* in response to strong environmental stimulation. It is probable that, in some sense, language development drives brain development. In conclusion, then, I shall briefly indicate how the phenomena Greenfield discusses are compatible with a non-nativist account of language acquisition.

Nativists take as their criteria for "language" the morphosyntactic *code* used by grammarians to describe some aspects of human verbal language. Instead, I shall take the semiotic *function* as defining language. The semiotic function is a development of the communicative function common among animals and is characterized by arbitrary symbols that require social conventions for their application and reproduction. Like many animals, we have an innate ability and drive to communicate using nonarbitrary gestures and signs such as pointing and smiling; and we share with a few higher animals, such as chimpanzees and dolphins, an innate disposition to participate in semiotic systems. It would seem likely, however, that we only become aware of and motivated to use this innate semiotic potential in social development during what Lock (1980) has aptly termed the "guided reinvention of language." From this perspective, acquiring one's mother tongue is analogous to learning to ride a bicycle; we are biologically capable and disposed to acquire the bicycle-riding function, but we need a bicycle and the social motivation to do so. This does not mean we must have evolved special purpose neural circuits for cycling; our disposition for cycling is there because the bicycle is a social artifact constructed (with some trial and error) to respect our potential biological limitations – and the same is true, I maintain, for verbal language.

During the protolanguage period, children acquire their patterns of participation in language by means of predominantly implicit learning in an input–storage–output–feedback loop based on many neural circuits, including those connecting Wernicke's area to Broca's area. There is abundant evidence that the hierarchical structures of protolanguage could be assimilated in this way, without any need for innate knowledge (Knowlton et al. 1992). The gradual accumulation of a large number of language games leads to the emergence of complex grammar in a kind of "phase transition" around 24 months. This phase transition is not something the child generates from within; rather, it is an inevitable product of its increasing participation in external dynamic symbol systems. Happily, the problem of dealing with this emergent complexity would coincide with the increasing availability of frontal functionality (Case 1992). In my view, the child would now utilize an increasing proportion of explicit learning and memory, with partial subordination of the protolanguage system to frontal control and reorganization leading to the production of complex morphosyntax. Instead of emerging circuits "driving language acquisition," I would imagine a much more flexible relation, along the lines of Bates (1992), in which some part of the capacity of general-purpose circuits would be specialized and directed by the requirements of the child's participation in language. Such a scenario would accommodate a realistic degree of localization and potential functional dissociation for language while excluding most of the strong nativist claims. It would also suggest a great deal of flexibility as to where, when, and how language functions get instantiated in the developing infant brain. [See also Pinker & Bloom: "Natural Language and Natural Selection" *BBS* 13(4) 1990.]

Author's Response

Language, tools, and brain revisited

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Abstract: The target article presented a model to stimulate further research and ultimately, a more definitive theory of the ontogeny and phylogeny of hierarchically organized sequential activity. Methodologically, it was intended to stimulate methods for integrating data from different neuropsychological techniques. This response to Givon and Swann focuses on several substantive areas: (1) the role of automaticity in hierarchically organized activity and its neural substrate, (2) the neural ontogeny of planning, (3) cognitive and neural architecture for language functions, and (4) the role of environmental input and interaction in the ontogeny and phylogeny of language, tools, and brain.

Truth or model? I am criticized by **Swann** for too much literalness in subscribing to my own theory. He states that if my "claims are taken as a temporary conceptual framework to integrate work from many disciplines, or as a sketch of what a general theory might one day look like, then her paper is a more modest but certainly more useful contribution" (Swann, p. 156). This is exactly what I meant the paper to be (see, for example, "If this theory is confirmed by further research" and "evolutionary hypothesis worthy of further investigation" in the conclusion 1991t, pp. 550, 551). Because I was as concerned about delineating the empirical evidence currently supporting my model as I was about setting out its theoretical parameters, the exposition gave a mistaken impression of greater literalness than was intended. I certainly was not so grandiose as to suggest hierarchical organization "as a source of unification in cognitive neuroscience," although, now that Swann has mentioned it, it does seem an idea worthy of exploration.

Indeed, my hope was to stimulate new research on my theory and even to carry out some more comprehensive research myself. At the present time, the main locus for new research has been primate behavior (Johnson et al., in press; Westergaard & Suomi 1994) and a connectionist computer simulation (Reilly 1996).

Automaticity, language, and brain. In his discussion of grammar, **Givon** brings up the important issue of automat-

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icity and attributes it to Broca's area. I would like to elaborate on this point. Bryan and Harter (1899) did a crucial and classic study of the relationship between hierarchical organization and automaticity of activity, using the learning of Morse code and Braille as their behavioral domains. These domains are particularly relevant to the present discussion because both Morse code and Braille are forms of language, albeit visual rather than oral language. What Bryan and Harter found was that, as skill increased, the lower levels of organization became automatic and conscious attention came to be addressed to the higher levels. Thus, beginners in both Morse code and Braille focused their attention on each letter. At the next level, letters became automatic; at this point, both Morse code operators and Braille readers focused their attention on the level of the word. After even more practice, words became automatic, and the sentence or thought became the object of conscious attention. I conceptualize this microdevelopmental sequence as follows: when lower levels of activity come under the control of higher levels, they become automatic (see also Bruner & Bruner 1968).

I hypothesize a similar process on the neural level: when one area of the brain comes under the control of another in a directional circuit, the operation (and corresponding behavioral activity) of that area becomes automatic. I believe that the automaticity endowed by Broca's area to grammar occurs because Broca's comes under the control of a more anterior area in the left prefrontal region, as the more complex circuit depicted in Figure R1 develops out of the simpler one depicted in Figure R2. (Figs. R1 and R2 are elaborations of Fig. 11 in the original target article.)

Is one-year-old behavior under anterior prefrontal control? The answer in Greenfield (1991t) is no. **Swann** takes strong exception, citing an image of the "active, exploratory toddler" as "the antithesis of the classic patient with frontal lesions." But the absence of planning does characterize the behavior of the one-year-old, just as it characterizes the behavior of prefrontal patients (e.g., Petrides & Milner 1982). Indeed, I would submit that it is precisely the absence of such strategic planning control that makes the toddler so exploratory: the toddler is doing by trial and error what an older human would plan out in advance. From a neural point of view, the one-year-old toddler would lack the input from the behavioral planning center shown in Figure R1. The circuitry of the toddler between age one and two is hypothesized to look like the model in Figure R2.

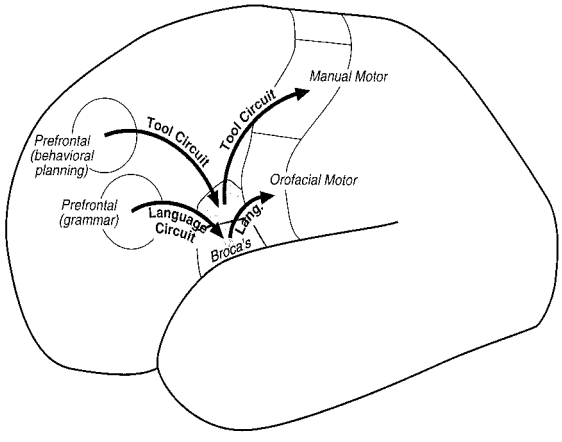


Figure R1. Hypothesized neural circuitry for the production of hierarchically organized manual sequences and grammar: Development from 2 to 4 years of age.

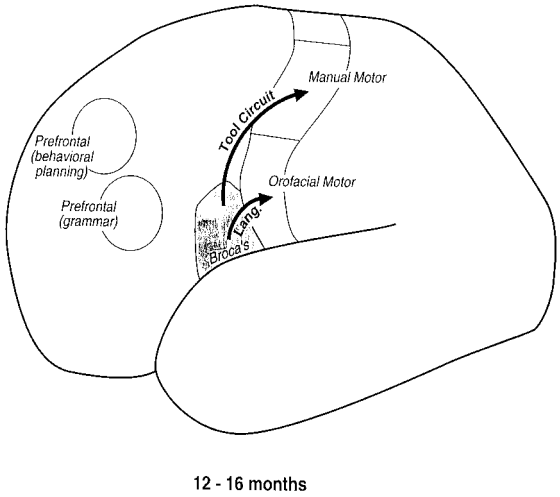


Figure R2. Hypothesized neural circuitry for the production of hierarchically organized manual sequences and grammar: Development from 12 to 16 months of age.

Localizing production and comprehension in the brain. A grammatical mode that is Broca-dependent is posited by **Givon.** In the theory put forth in Greenfield (1991t), it is most definitely not the classical Broca's area alone that is posited in this role. Rather, a circuit in which a prefrontal area (Brodmann's area 46) provides input to the classical Broca's area (Brodmann's areas 44 and 45), which in turn provides input to the orofacial motor strip, has primary responsibility for the production of complex grammar. (See Fig. R1).

In contrast to his emphasis on Broca's area for complex grammar, **Givon** attributes the production of simple, grammatical propositions and multipropositional discourse to Wernicke's area. In my view (Greenfield 1991t), the production of these simpler structures – "pregrammatical" but hierarchically structured speech – utilizes the circuit from Broca's area to the orofacial motor strip. (See Fig. R2.) The more anterior prefrontal part of the circuit in Figure R1 gets added in at a later point in development to enlarge the circuit for more complex, truly "grammatical" production.

One problem with **Givon**'s model of speech localization is that he does not differentiate comprehension from production. However, this differentiation is necessary in discussing the neural circuitry used for language functions. Although comprehension and production probably have some overlapping components, the degree of overlap is an empirical matter.

There are other problems as well. For example, Givon (personal communication, 1996) uses Snyder et al.'s (1995) experiments with event-related potentials (ERP) to bolster his argument concerning speech localization. These experiments involve the following task. In response to a written noun, the subject must orally generate a use for it (e.g., "pound" in response to "hammer"). The neural activation

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produced by simply reading the noun aloud (e.g., "hammer") is subtracted from the neural activation produced by the use-generation task (e.g., "pound"). If, on the level of the task, we subtract reading the noun aloud from generating its use, the remaining mental operations include comprehension (of the noun) and very simple word combination (noun plus use). After carrying out subtractive operations on the ERPs, Snyder et al. found the use-generation task to sequentially activate several brain regions. Most critical to the argument here, two of these regions were the left inferior frontal cortex (Broca's area) and the left posterior cortex (Wernicke's area). While not excluding Wernicke's area, which may have come into play for the comprehension part of the task, at very least these data are consistent with the target article's hypothesis that Broca's area is involved in the process of protogrammatical word combination.

Cognitive and neural architecture. My view of cognitive and neural architecture is neither domain-specific nor domain-general. Instead, I posit overlapping neural circuitry for various domains of adult activity and cognition. I also posit a general developmental trend from domain-general to domain-specific circuits. I also am open to the suggestion of Bates (1992) that "some part of the capacity of general-purpose circuits would be specialized and directed by the requirements of the child's participation in language." However, I believe that certain circuits are predisposed to be specialized and directed by language and manual input. Unlike me, Swann sees a greater degree of "flexibility as to where, when, and how language functions get instantiated in the developing infant brain."

Swann cites Knowlton et al. (1992) as demonstrating that hierarchical structures of protolanguage could be assimilated without innate knowledge. Knowlton et al.'s study concerned the learning of rules for generating letter strings. However, the sequencing rules to be learned in the Knowlton et al. study were organized in a linear, Markovian fashion; they were not hierarchical. Therefore, this study does not bear on the construction or learning of hierarchical organization in language.

I agree with **Swann** that "during the protolanguage period children acquire their patterns of participation in language by means of predominantly implicit learning," without any need for innate knowledge. Although he attributes an important role for innate knowledge to my theory, I believe that I am taking about innate *structure,* not innate *knowledge.*

Methodology for integrating data from different neuropsychological techniques. One of the goals of my target article was to develop a logic for combining information from different techniques for studying the brain. Such a logic was conspicuously absent from the literature. Of each set of neural data, whether emanating from EEG, neuroanatomical study or another technique, I asked what were the implications for speech and manual function and development. I hoped to advance both the methodology and the integrative knowledge of brain–behavior relationships by so doing. I discussed the complementary patterns of strengths and weaknesses of data generated by the various techniques (Greenfield 1991t, p. 542) in the hope of advancing methodology that would be relevant to integrating data from different brain study techniques. I certainly did not mean to imply that I was using a "validated methodology." Again, I saw my methodology as generating hypotheses for further research, not certainties. What I wanted to do was stimulate others to think about the value of integrating information about brain structure and function that is generated by diverse methods and methodologies.

Differentiation of neural circuits. Contrary to **Swann**'s conclusion, I believe there is no contradiction between my Stage 1 circuit (Fig. R2) in which an undifferentiated Broca's area provides input to the motor strip and my Stage 2 circuit (Fig. R1) in which input from two separate prefrontal areas acts to differentiate Broca's area. On the conceptual level, I was distinguishing between *input* to Broca's, which could serve to differentiate it, and *output* from Broca's, which could not serve this function. Again, this was in the form of a hypothesis; it must be tested by future research.

Words, phonemes, and brain. Swann reintroduces Tomasello's (1991c) idea that words, rather than phonemes, would be the appropriate homologues to objects in manual construction behavior/tool use, pointing particularly to MacNeilage's (1991c) original critique. Swann's remarks have stimulated me to rethink the issue, and I now believe he may be right. This idea solves two problems: (1) the problem of *décalage* between the speech stage and the object stage that arose with phonemes as the combinatorial unit and (2) the problem that individual phonemes do not function as independent units in syllable structures. Indeed, it was the word level that I originally conceived as the appropriate level of analysis (Greenfield et al. 1972). Tomasello (1991c) presents an interesting way to look at word combination that avoids the problems of *décalage* with object combination that had originally troubled me.

The need for functional brain mapping of young children in action. I believe that developmental studies of functional neural imaging are now required to settle empirically the issue of identifying the correct linguistic units for my developmental theory. Indeed, such studies are required to determine whether any *analogy* is in fact a true *homology.* This is also my reply to **Swann**'s discussion of my model of brain development: Swann himself agrees with this, pointing out the necessity of mapping the behavioral sequences onto the neurobiological data. The data that I used, although imperfect, were the best available at the time. They functioned to support the development of a model that can serve as a guide to such mapping when the appropriate technology is developed. Brain mapping technologies still do not exist for the functional mapping of speech and manual construction activity in one- to three-year-olds (Bookheimer 1996).

Transcription and structure of sound in language development. Doubts are cast by **Swann** on the transcription of the examples used for the developmental evidence of increasing hierarchical complexity in language. To clarify my transcription method: it was phonemic rather than phonetic. This was the appropriate level for the phonemic level of the analysis.

Swann also alludes, critically, to my "starting assumption that children build their early words from phonemes in a process of hierarchical construction" (**Swann**, p. 156). This was not an assumption. Rather it was a hypothesis for which I presented evidence. I in no way felt or feel that the hypothesis was firmly established by the sequence I found

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in my diary studies. However, the evidence was promising enough to merit further investigation of the hypothesis.

Nativism, social interaction, and culture: Phylogeny. After an excellent summary of the model put forth in Greenfield (1991t), **Swann** labels my evolutionary scenario nativist, but I think it had a good balance in its explanatory mechanism between neural structures and environmental interaction in the service of the co-construction of culture. The pedagogy of tool use (with its use of symbolic gestures) in nonhuman primates was put forth as the motor of language evolution and an important force in tool evolution as well. Pedagogical techniques, including symbolic elements, are conceived as environmental factors from the perspective of the tutee, but as having a biological basis from the perspective of the tutor. It is the interaction between tutor and tutee, however, that drives both the ontogenetic and phylogenetic development of culture, including both language and tools. It is in the conventionalization of such interactions (Tomasello et al. 1993) that culture is born, both ontogenetically and phylogenetically.

The status of phylogenetic speculation. As mentioned in Greenfield (1991t), all evolutionary scenarios relating to behavior are of necessity speculative. There are constraints in the cladistics of behavior, however, that allow us to eliminate and compare the probabilities of various scenarios. Indeed, since the publication of Greenfield (1991t), cladistic analysis has become ever more quantitative and precise. This precision opens the way for the development of future scenarios based on more species. Such scenarios will be even more highly constrained by data.

Nativism, social interaction, and culture: Ontogeny. I certainly had no intention of proposing "an absurdly strong form of nativism in which the development of children's speech and object combination is as genetically constrained as is the growth of their teeth" (**Swann**). However, for expository purposes I emphasized what appeared to be universal patterns founded on a neural substrate and did not discuss the other side of the coin, the role of environmental interaction, which is influential in actualizing the developmental pattern, as Swann implies. Let me now redress this balance by discussing a specific instance of the ontogeny of hierarchical organization and complexity.

In presenting results of the nesting cup studies, I emphasized evidence for the universality of the sequence from pairing to "pot" to subassembly by referring to data from Boston (Greenfield et al. 1972) and an indigenous Maya community in Chiapas, Mexico (Greenfield et al. 1989). I noted that the Zinacantecs, a Maya group in highland Chiapas, went through the same sequence of manual "grammars," despite their unfamiliarity with nesting cups in particular and objects/toys in general. This was considered evidence for the universality of the behavioral sequence and of the neural substrate guiding the development of hierarchical complexity.

What I did not mention were other results that pinpointed environmental influences on the behavioral development (Greenfield et al. 1989). First, we found that Zinacantec babies lagged behind Euro-American ones in their rate of progression through the sequence. This was hypothesized to be due to a much lower level of stimulation in the domain of object manipulation. Second, we had experimental evidence indicating the role of socially guided experience with the materials. After mothers, at our request, taught their babies how to perform the most hierarchically advanced strategy with the cups (the subassembly), we found that older Zinacantec babies advanced through the sequence in just a few minutes and could subsequently perform more complex strategies on their own. So, environmental stimulation affected the *rate* but not the *shape* of the progression. It is this shape that was my focus in Greenfield (1991t).

I have a similar response to the comments on nativism in language acquisition. First, as a cultural psychologist who specializes in cross-cultural research, I have a strong commitment to human diversity in learning and development. However, there are underlying biological universals that guide this learning and development through the specification of structural principles. The environment cannot create a capacity *de novo,* but interaction with the environment can select, instantiate, and amplify a capacity already present as a potential (cf. Chomsky 1980). [See also Chomsky, "Rules and Representations" *BBS* 3(1) 1980.]

Just as environmental interaction facilitates the development and instantiation of these structural principles in the object domain, so too does interaction facilitate the development and instantiation of the same principles in the language domain. To see that this is true, it is only necessary to observe what happens to structural development when this interaction does not take place (Curtiss 1979). Actualized by the communication process, the structural principles of hierarchical organization guide the acquisition of a full range of languages whose specific features are input from the environment.

Conclusion. Both **Swann** and **Givon** have raised interesting issues concerning the relations among mind, brain, and behavior. I welcomed the opportunity for discussion of two important areas that had thus far not been addressed in either the target article, the commentaries, or the responses. These areas are (1) the role of automaticity in hierarchically organized activity and its neural substrate and (2) the role of environmental input and interaction in the ontogeny and phylogeny of language, tools, and brain. This initial discussion indicates that they are promising ones for future research and theory.

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