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Chapter 2

Evidence from Great Apes Concerning the Biological Bases of Language*

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Until recently it was generally assumed that studies such as Gardner and Gardner (1971) and Patterson (1978) had demonstrated that great apes possess the capacity to acquire rudimentary linguistic skills. There is now little agreement about their behavior as a consequence of subsequent studies (e.g., Terrace, Petitto, Sanders, & Bever, 1979) and critical analyses of the earlier ape language research (e.g., Seidenberg & Petitto, 1979). History suggests that when an issue is polemicized, as this one has been, it is often simply set aside. However, I think that it would be unfortunate if some important implications of this research were obscured in the haze of conflicting claims. Taken with related research on the non-linguistic cognitive capacities of lower primates, and with some innovative research on language acquisition in children, the ape language studies provide important evidence bearing on a basic question in the cognitive sciences.

The question concerns the relationship between language and other cognitive capacities. Stated simply, is language the expression of a domain-specific capacity (or faculty), or is it merely one of many expressions of a general capacity to engage in intelligent behavior? The domain-specificity claim is familiar from the work of Chomsky (e.g., 1978) and has recently been incorporated by Fodor (1983) into a general theory of the organization of intelligence. The notion that language is derived from non-specific cognitive capacities to think and learn is familiar from the work of Piaget (1955, 1980), and has been elaborated by Bates and MacWhinney (1982), Bruner (1975), Anderson (1983), and others. In this chapter, I will argue that the only way to accommodate the data from recent studies of ape intelligence is in terms of the domain-specificity claim. That is, when the research on both the linguistic and non-linguistic capacities of apes is considered, it strongly supports the idea that language results from a biologically given, species-specific, autonomous faculty. Only this hypothesis provides a

* I would like to thank Laura Petitto for discussing these issues with me and commenting on an earlier draft of the paper. I am also grateful to her for permission to extensively cite her unpublished thesis.

principled explanation of the behavior that has been observed. This is a positive, non-trivial result, although not necessarily one the ape language researchers would themselves promote.

The Logic of Ape Language Research

At first glance, studies of ape language seem to be premised on denial of the obvious. Humans acquire and use natural languages, and lower primates do not. This irrefutable fact would seem to constrain a priori what might be learned by training apes to perform "linguistically." It might be useful to consider, then, how it was thought that these projects could be informative.

One reason to study lower primates is simply because it would be valuable to understand the behavior of other intelligent species. While no doubt the motivation for much traditional primatological research, this will not account for the notoriety of recent ape language research. Much of the interest in these studies derives from the possibility that they might bear on other issues, such as the evolutionary differentiation of humans from other primates, the origins and evolution of language, and the relationship between linguistic and non-linguistic intelligence.

The ape language research has often been placed in the context of evolutionary questions (e.g., Fouts, 1974, 1975; Parker & Gibson, 1979; Walker, 1983). The theory of evolution, together with the fact that humans, gorillas, and chimpanzees evolved from a common ancestor, leads to a general expectation that these species might exhibit similar behaviors, which could be revealed through comparative studies. As Wasserman (1981, p. 246) has stated, "The hypothesis that man and animal share cognitive capacities is simply the single most important idea of comparative psychology." Whether such commonalities exist among primates is an empirical question, and the facts about evolution suggest that it might be an interesting one.

Note, however, that evolution provides no basis on which to anticipate particular behavioral similarities, in terms of language or otherwise. Evolution is a theory of speciation, not of behavioral continuity. From the fact that various primates shared a common ancestor and the fact that species evolve, no specific behavioral commonalities necessarily follow. Particular homologies (or analogies) are a consequence of actual events, largely unknown, that are incidental in terms of the theory of the evolutionary process. As a consequence, comparisons of behavior need to be interpreted in the context of a theory of behavioral similarity, not merely in terms of evolution.

This point has not been sufficiently appreciated in the ape language literature. The problem is that general evolutionary facts are sometimes used in order to *establish* behavioral similarities (see, e.g., Fouts, 1974). The apes exhibit complex behaviors that are ambiguous at best. The interpretation of these behaviors

is assisted by appeals to evolution, leading to the conclusion that an ape's behavior corresponds to that of a human *because* apes and humans descended from a common ancestor. However, this reasoning is entirely circular. In the absence of an explicit theory as to how particular behaviors evolved, evolutionary facts such as common ancestry provide no basis on which to mediate comparisons of behavior.

A clear example of this inappropriate appeal to evolution is seen in a book by Gribbin and Cherfas (1982). The authors describe recent research using techniques from molecular biology which have been used to infer the evolutionary history of hominids (e.g., Sarich & Wilson, 1967). This research is controversial because the evolutionary timetable that results from these techniques is inconsistent with that developed from the paleontological record. The degree of genetic overlap among various primates (as measured, e.g., by similarities in blood proteins such as albumin) is used to infer the evolutionary sequence. These studies have yielded the conclusion that humans and chimpanzees share about 99% of their genetic material (King & Wilson, 1975). From the fact that humans and chimpanzees are genetically similar (and therefore close evolutionary relatives), Gribbin and Cherfas imply that we must be behaviorally alike as well. Numerous examples of behavioral similarities are then described. It is as though the new genetic discoveries indicate that we are more similar in behavior than previously thought. However, the molecular biological facts have no bearing on identifying behavioral continuities across species; they are facts about body chemistry, not behavior. Overlap at the level of DNA is consistent with the existence of such continuities, but does not itself validate (or invalidate) them. Nonetheless, these data are widely cited in discussion of ape language as strongly corroborating evidence (e.g., by Rumbaugh & Gill, 1976; Patterson, 1980a; and Tanner & Zihlman, 1976).¹

Another problematical argument from evolution is seen in attempts to relate ape language to questions concerning the origin of language (see, e.g., many papers in Wescott, 1974; Harnad, Steklis, & Lancaster, 1976; and Reynolds, 1981). Chimpanzees are thought to be less highly evolved than humans; therefore, by studying their natural communicative behavior, and observing their progress in learning one or another aspect of human language, we might gain evidence concerning the structure of proto-languages and the evolution of the linguistic capacity in humans. The problem here is that humans did not evolve from chimpanzees. The two species are collateral descendents from a common ancestor

¹ I think this point should be clear. A model of the Brooklyn Bridge, a loom, and a computational device with the power of a Turing machine could be constructed out of identical sets of Tinker Toys. Nothing whatever would follow about their relatedness at higher levels of analysis. The biochemical facts would be compelling only if there were a theory relating them to behavior. Reductionists must also contend with the fact that, as Malmi (1976) noted in an interesting discussion of these issues, the 1% difference involves at least 40 million base-pairs.

which have evolved in parallel (Ploog & Melnechuk, 1969). Nor are chimpanzees "less highly evolved" than humans; they simply evolved in a different manner. Inferences from the behavior of chimpanzees to the behavior of a common ancestor who existed several million years ago can only be acknowledged as speculative at best (see Parker & Gibson, 1979, for a tour de force argument of this kind).

Evaluating the question of behavioral continuities across species has proven to be a hugely complex problem. There is basic disagreement about first principles among comparative psychologists, ethologists, sociobiologists, physical anthropologists, and behavioral ecologists. I think it is fair to say that lack of a clear theory of comparative study has contributed to difficulties in interpreting the behavior of signing apes. The questionable appeals to evolution discussed above are only one manifestation of this problem.²

Despite these difficulties with specific applications of the concept, evolution provides a rationale for comparative, behavioral studies of related species. Psychologists have shown special interest in comparative studies of language because it seems to be a uniquely human endeavor. Such studies were thought to have the potential to provide an empirical test of Chomsky's claim that there is a language faculty specific to humans. One strategy might be to determine whether the natural communicative behaviors of lower primates share important properties with human language. This research is being pursued to interesting ends by Seyfarth, Cheney, and Marler (1980; Seyfarth & Cheney, 1980; see also Struhsaker, 1967). Although these behaviors exhibit interesting properties, it is highly unlikely that lower primates will be revealed to possess a natural communicative system closely resembling human language.

A second strategy has been to train apes in particular linguistic skills. What might be learned from such laboratory studies that is not revealed by studying the natural communicative behaviors of apes? These studies have often been motivated by the idea that if the ape were provided with a linguistic model and appropriate learning experiences, neither of which occur in the wild, it could acquire linguistic skills. This view presupposes an empiricist, learning-theoretic view of language acquisition, as Gardner and Gardner (1971, 1974a, b) explicitly acknowledge. Many papers in this area express the sense that these experiments would provide a definitive refutation of Chomsky's critique of Skinner. Given the well-known inadequacies of the learning-theoretic account of human language acquisition (Fodor, Bever, & Garrett, 1974), there seems to be little basis for

² I don't at all mean to suggest by these remarks that an evolutionary theory of the origins of language is impossible in principle. It is possible, for example, that, given an understanding of the neurological bases of language, and additional information about the evolution of the brain (drawing perhaps upon data such as Holloway's, 1976), such an explanation could be developed (see Deacon, 1979, for an attempt along these lines). My only point is that evolutionary (and biochemical) facts of the sort mentioned in the text are not sufficient, and that they are little help in drawing behavioral comparisons.

expecting that an ape, trained in this manner, would come to exhibit elaborate linguistic behavior. Construed in this manner, the ape language experiments could only provide a test of how much linguistic behavior could be acquired through application of the precepts of *Verbal Behavior*. This places ape language research in a very different context from studies of child language (as other papers in this volume confirm). The very limited accomplishments of the apes trained in this manner seem to provide mostly negative evidence concerning the power of this approach.

A more interesting basis for these studies might be provided by the hypothesis that the capacities of the primate brain are not fully realized under natural conditions (Harlow, 1958). Specifically, it could be assumed first, that apes possess a capacity to produce or comprehend at least some aspects of language that is unexpressed in their natural environment, and second, that the laboratory conditions of these experiments provide a means for realizing this capacity. However, the validity of these assumptions is unclear. The traditional answer as to why the apes' putative linguistic capacity isn't manifested in their natural behavior (dating from Yerkes, 1925) is that apes lack the capacity to physically produce speech signals, as Lieberman (1968) later substantiated. With a little effort (e.g., the motor theory of speech perception), this might also explain their failure to comprehend language. Given an alternate means of expression, the ape could capitalize on its latent capacities. The manual gestures of sign languages were thought to provide this alternative. These studies have relied upon a "molding" technique (i.e., physically shaping the ape's hands into signs) rather than conditioning of specific responses; "molding" would be sufficient if in fact the ape merely needed to be given a means for expressing an inchoate linguistic ability.

This logic is inconsistent, however. Apes lack part of the neuro- and motor-physiology that supports speech. The sign language researchers proposed to overcome this limitation by exploiting the apes' natural ability to gesture. This effort would only succeed if they were capable of using the alternate modality. But if apes possess this capacity, the explanation for the fact that they fail to naturally express their linguistic capacity is wholly lost. It is interesting to note in this regard that the natural communication of lower primates is not primarily gestural. On their own, they seem to make little of their opportunity to use their hands for communication.³

There have been other attempts to explain why apes might have an unrealized linguistic capacity (e.g., language lacks adaptational significance for apes; they lack the motivation to use it; they actually have a language but humans are

³ This logic also ignores the fact that having an intact vocal system is not a necessary prerequisite for human language. Evidence from the sign languages of the deaf suggests that humans have evolved a modality-free capacity for language, rather than a capacity for speech (Bellugi & Seidenberg, in preparation).

incapable of perceiving it, etc.). I do not find these persuasive, but perhaps a better explanation could be developed. Here I only want to make explicit the fact that in teaching sign language to apes, it is often presupposed that (a) they possess a latent, unexpressed capacity to use language; (b) they possess a physical means for realizing this capacity; and (c) there is a principled reason why this capacity is not naturally realized. However, (c) cannot be explained in terms of the absence of (b), leaving (a) in doubt.

I have suggested that it is unclear why sign language training should result in the production of communicative behaviors qualitatively different from those observed among untrained apes. Nonetheless, there is enough ambiguity in our understanding of the evolution of language and its biological bases to suggest that these studies might have been revealing in unexpected ways. With this in mind, we turn to the evidence. In the discussion that follows, I emphasize the studies in which apes were trained to use sign language, because these provide most of the data on which various linguistic attributions have been based. Although both Premack (1976) and Rumbaugh (1977) initially placed their research, which does not involve signing, in the context of language acquisition, they now view it as bearing primarily on other issues, to which I return later in the paper.

Results of the Sign Language Projects

Although this research has been controversial, a careful reading of the literature suggests that the behaviors of signing apes exhibited no important similarities to human language. Many claims have been made on behalf of these animals (e.g., that they could name objects and answer questions, that they used a rudimentary syntax, that they created novel expressions and comprehended their trainers' utterances, etc.), but they cannot be sustained by the available evidence. In general there are two problems: the behavioral evidence that could establish these claims has not been provided, and the evidence that is available is consistent with non-linguistic interpretations.

As Petitto and I have described elsewhere (Seidenberg & Petitto, 1979, 1981), part of the problem is that crucial information about the apes' actual behavior needed in order to establish their linguistic competence is simply unavailable. It is remarkable how little systematic information about the performance of signing apes has been provided in nearly 15 years of research. The primary data are lists of the signs in the animals' vocabularies, supplemented by examples of their usage. However, the vocabulary lists represent *hypotheses* about their performance, not *data*. The question is, what is the basis for concluding that the gesture believed to be *banana* corresponded in any interesting way to the word *banana* in a human's vocabulary? This could only be established by a systematic analysis of how the gesture was actually used, but none has been provided. There have been no analyses, for example, of the events (such as presence of particular

objects) correlated with the ape's production of a sign. Gestures are assigned glosses on the basis of the trainers' intentions in teaching the ape, rather than on the basis of their actual performance. In this way, the trainers project their own use of signs onto the apes' use of them. These studies clearly establish what the signs meant to the trainers; how they functioned for the animals is unclear.

Other claims are supported by example. Washoe is said to use contrastive sign order (evidence for syntax) because she signed *baby mine* and not *mine baby* (Gardner & Gardner, 1974a). Koko is said to combine signs creatively because she signed *cookie rock* to describe a stale sweet roll (Patterson, 1978). The problem with these examples is that the conditions under which they occurred are ambiguous, and as a consequence they cannot be interpreted outside the context of a thorough and systematic analysis of a wider range of behaviors. This is the most telling point of Terrace et al.'s (1979) data on Nim. Their data—primarily a listing of nearly 20,000 of the chimpanzee's utterances—are not without important limitations; for example, there is little systematic analysis of the contexts in which the utterances occurred. Nonetheless, their corpus is revealing. Looking at isolated examples, it appears that Nim too produced "novel" combinations such as *cookie rock*. Looking at the entire corpus, however, it is clear that his combinations were heterogeneous. Many are interpretable with a little imagination; most are not. An alternative hypothesis consistent with the data is that Nim had simple principles for combining signs without reference to meaning or context; a number of "creative" utterances fall out of this process, as does a lot of word salad. It doesn't follow that a process of this type necessarily underlay Washoe or Koko's "creative" combinations; it merely demonstrates that provocative examples cannot be interpreted outside a broader context. I think this point should be obvious, but these examples have exerted enormous influence.⁴

Looking again at the Nim data, it is clear that, as with Washoe's *baby mine*, some pairs of signs appeared in preferred orders (e.g., *me Nim* occurred more often than *Nim me*). Isolating these examples from the corpus, it might appear that Nim's utterances exhibited a rudimentary syntactic structure. However, these asymmetries are not consistent across sign classes; they appear to be specific to certain vocabulary items, as might be expected if stereotypic combinations were produced in a rote, mechanized fashion. Nor was there any evidence that sign order was used contrastively; for example, both *me give* and *give me* appear in the corpus, but there is no evidence that their meanings differed in appropriate

⁴ Dennett (1983) ascribes great importance to the role of "anecdotes" in understanding animal behavior. He has in mind a special sense of "anecdote," however. He notes that "novel" behaviors may be highly revealing and advocates research strategies which "have the virtue of provoking novel but interpretable behavior, of generating anecdotes under controlled (and hence scientifically admissible) conditions" (p. 348). Behaviors observed in this manner would not have the casual character of the *cookie rock* examples.

contexts. Finally, Nim's mean length of utterance (MLU), which provides a rough estimate of the complexity of his utterances, was essentially flat across the course of the experiment.

It is seen, then, that the Nim data contain examples of behaviors which, in the Washoe and Koko studies, are cited as evidence for linguistic abilities in signing apes. These data suggest that Nim's behavior very closely resembled that of other signing apes. However, only the Nim data permit these examples to be evaluated in the context of a broad range of utterances. When this is done, it is seen that the examples are both consistent with and suggestive of much simpler, non-linguistic interpretations. These alternative interpretations could not be evaluated with regard to other signing apes simply because the relevant data were not provided.

In support of their conclusions, Gardner and Gardner (1980; Gardner, 1981), Patterson (1980b), and others (e.g., Van Cantfort & Rimpau, 1982) assert that Nim's behavior was aberrant. They correctly note that it does not necessarily follow from the fact that Nim failed to sign creatively that Washoe and Koko failed as well. Possibly Nim was badly trained or less intelligent; maybe there are individual differences among apes in regard to linguistic skill. However, as these examples suggest, nothing in the available data indicates that Nim's behavior differed remarkably from that of the other signing apes. What differs is the interpretation of behaviors such as *cookie rock* or *baby mine*. When such behaviors are evaluated within the broader context of a corpus of utterances, it is clear that they cannot bear the weight of evidence they have been assigned.

I have suggested, then, that the conclusion that apes such as Washoe or Koko exhibited linguistic skills was based on examples inappropriately isolated from their general behavior. The net effect of this selective reading of their behavior was to make it very difficult to evaluate alternative, non-linguistic interpretations. In this way, the ape language research departed from the normal process of scientific inquiry in a profound way. The ape language researchers employed what I have termed a *consistency criterion* in evaluating their subjects' behavior (Seidenberg, 1983). They merely cite examples that are consistent with a linguistic interpretation. Washoe signing *baby mine* is consistent with the hypothesis that her utterances were structured. Koko signing *cookie rock* is consistent with the hypothesis that she could combine signs creatively. However, these behaviors are also consistent with non-linguistic interpretations whose validity could not be directly assessed because the relevant data were not presented. In other words, the linguistic interpretation of these examples could not be falsified.

The absence of any attempt to systematically address the many non-linguistic interpretations of ape signing is the single most devastating failure of the ape language research. The consistency criterion represents a radical departure from normal scientific inquiry. Contrast this method with one recently proposed by Dennett (1983). Dennett is at pains to develop a method for evaluating complex, ambiguous animal behaviors (such as those involved in communication) which

may involve intentional states (such as beliefs and desires). As Dennett notes, the notorious problem in evaluating such behaviors is that they appear to be consistent with a variety of interpretations, ranging from non-intentional, behavioristic, "kill-joy" hypotheses to those involving second- or third-order intentional attributions. Dennett advocates a strategy whereby the range of alternative hypotheses is explicitly stated and evaluated with regard to the behavioral evidence. Clever and devious experiments might be developed so as to obtain data critically bearing upon one or another alternative. This is a cogent articulation of a familiar strategy, although in practice it turns out to be quite difficult to obtain the critical disambiguating data. The problem is that no such strategy is employed in the ape language research. The researchers evaluate the consistency of the data relative to the single hypothesis that the apes' signing is the result of particular linguistic skills; other interpretations are hardly acknowledged, and the data bearing on them are not solicited.

It is interesting to observe that, in entertaining only a single level of analysis, the ape language researchers follow the strategy of radical behaviorists. The behaviorist is committed to a particular level of analysis, one that does not acknowledge higher-level, intentional explanations. The ape language researchers are committed to such higher-order, linguistic analyses of their subjects' behaviors, and fail to evaluate lower level ones. It is this a priori commitment to one or another level which Dennett argues against.

Breadth of Comparisons

A related problem concerns the nature of the comparisons between ape and human behaviors offered in this literature. As I stated above, whatever the merits of evolutionary or biological arguments, behavioral comparisons require an adequate theory of the behaviors in question. With a sufficiently narrow conception of a linguistic skill, it is a simple (if uninteresting) matter to establish "continuity" across species. As Chomsky (1980) has noted, "This is much like the question whether humans can fly, almost as well as chickens though not as well as Canada geese." In the ape language research, linguistic skills are conceptualized in a manner that limits the interest of the resulting comparisons. Consider, for example, naming. It is widely believed (following Terrace et al., 1979) that while apes cannot combine signs into sentences, they can nonetheless use signs to name objects. The validity of this claim rests on the theory of the naming task and the evidence from actual performance bearing on it. It is often asserted in this literature that a barrier to drawing decisive conclusions about the apes' performance is the absence of any general agreement about the "definition" of language or particular linguistic functions such as naming. Evaluating whether the ape could name objects, for example, is difficult because of our limited understanding of the concepts of meaning and referring. It is sometimes suggested

that these limitations preclude making theoretically informed comparisons across species (Gardner & Gardner, 1974b). I think that the limitations on our knowledge of linguistic structures and functions have been overstated. Given our understanding of language, revealing comparisons between the ape and human are possible. The general strategy must be to understand the principles governing the ape's use of signs (in naming, for example), and to determine whether these principles could account for analogous behavior in humans. It is important to acknowledge the theoretical lacunae, but these do not preclude detailed comparisons of ape and child. Indeed, I doubt if the technical questions in the theory of meaning or syntax have much bearing on understanding the nature of ape signing.⁵

What, then, of the claim that apes can learn to use names? The first point to note is that, whatever one's theory of naming, the conclusion cannot be sustained on the basis of the evidence currently available. As with other aspects of the apes' actual performance, very little systematic data related to naming has been reported. Terrace et al.'s (1979) large corpus of utterances provides little information about how these were actually used. Nor does Patterson provide documentation of how Koko used the several hundred names for objects in her putative vocabulary. The primary evidence that apes can name objects derives from the Gardners' experiments in which Washoe signed the names of objects (or pictures of objects) presented in a box (Gardner & Gardner, 1971, 1974a). While this task could provide interesting information about the ape's use of signs, it does not provide a good basis for determining whether its ability to name objects was comparable to that of a child. The problem is that the task could be performed successfully if the ape had learned simple associations between signs and objects. Construed as a test of naming, it presumes a behavioral theory of meaning and reference, a theory known to give a poor account of the child's knowledge and use of names (MacNamara, 1983). Thus, the manner in which the task was constructed limits the potential for drawing positive comparisons to children.

It would be important nonetheless to know if the ape could learn to perform this task; if nothing else, it would provide information about its ability to form certain associations. As noted previously (Seidenberg & Petitto, 1979), there are important unanswered questions about Washoe's performance on this task; details concerning the stimuli, scoring procedure, and Washoe's performance have not been provided. Although it appears that she probably learned some such associations, the number of signs used correctly on this task, the reliability of her sign usage and whether her performance generalized to new exemplars without specific training are unclear. There is independent evidence, however, from a

⁵ This situation is a common one in science. To take an example from a very different domain, our theoretical understanding of the molecular bases of water currently does not provide an explanation for why water boils at 100°C and freezes at 0°C (Franks, 1981). This basic limitation does not preclude distinguishing water from other substances.

study by Savage-Rumbaugh and Rumbaugh (1978) suggesting that it is difficult for apes to form associations between arbitrary symbols and particular referents. They explicitly attempted to teach their apes such associations (using lexigrams rather than manual signs); the apes had limited success in learning these associations even with intensive training.

Whether the ape's use of names depends greatly upon such associations is as yet unclear. In pursuing this question, however, other interesting issues could be addressed which might provide the bridge to deeper comparisons to children. For example, it appears that apes can learn to identify categories of objects (Premack, 1976), and interesting additional questions could be asked about this behavior. Does the ape categorize objects on the basis of their functions, as Nelson (1974) has suggested of children? Do apes respond to semantic features that determine category membership, as Clark (1974) suggests is true of children? Are they capable of abstracting prototypical members of categories (Rosch, 1973)? While each of these proposals presents difficulties as a characterization of the basis of object naming in children, information concerning the apes' performance would provide a more substantive basis for comparisons to children. At the same time, it might be determined whether the apes' associative and categorial processes differ in any important respect from those observed in other species (e.g., pigeons; Herrnstein, Loveland, & Cable, 1976). At the present time, however, I think it is premature to conclude that the apes could name objects under any interesting conception of this skill. Given a more adequate theory of naming (or referential communication in general), I believe it would be possible to delineate the ways in which the ape's performance differs from that of a child. However, there are many unanswered empirical questions in this regard.

This skeptical view of the ape's ability to name is also prompted by my own experiences with Nim. It was clear to me that, while Nim did consistently associate a very small number of signs with particular objects (or classes of objects), his general strategies for producing signs were quite different. Rather than associating particular signs with particular objects, he associated groups of signs with stereotypic situations. For example, eating contexts (e.g., sitting in the kitchen of his residence) would invariably provoke a cascade of eating-related signs: eat, more, drink, give, etc. Other very general contexts (e.g., a picture book in the classroom) would produce other clusters of signs. Another strategy was simply to imitate signs in the teacher's input. This was a very clever strategy for producing signs the teacher would consider "contextually appropriate." In neither case was sign production mediated by specific associations between signs and objects.

Another example of the manner in which the conceptualization of a task limits the interest of cross-species comparisons is provided by the Gardners' test of Washoe's ability to answer questions (Gardner & Gardner, 1975). Washoe was trained to respond to questions such as *who that* or *where that*, and was tested

on 500 trials. Each response was scored as correct if it contained a sign from 1 to 5 pre-designated target categories (in the above examples, a proper noun or a locative, respectively). It is reported that Washoe answered questions correctly on a statistically significant proportion of trials. A response was scored as correct if it anywhere contained an item from the target category; the appropriateness of the signs to the contexts was ignored. Thus, if a banana were held up, and the question were "what that?" the response *you me give ball more* would be scored as correct, because it contains the sign *ball*, a member of the target category noun.

The effect of this procedure is to permit Washoe to perform at the reported levels by means of various non-linguistic strategies. One, for example, would be to generate very long chains of signs, increasing the probability that one of them would be from a target category. Another would be to associate a small number of signs with each target frame. This could be accomplished during the extensive pretraining on the task. It insults the intelligence of the animal to think it would not latch upon these strategies, since they are much simpler than actually being able to comprehend and answer questions. Because of the manner in which the task was constructed, the data provide no basis for interesting comparisons to question-answering in humans. Although the test is promoted as a test of Washoe's ability to understand and answer questions, no specific linguistic knowledge was required in order to perform it.⁶

This study is representative of the literature in several ways. First, a linguistic skill (question-answering) is construed in a manner that bears a remote relation to the human skill. Answering questions is not simply a matter of responding with items from particular syntactic categories. Second, the task was constructed so as to permit simple, non-linguistic responding strategies. Third, the results were interpreted in the richest possible way (as evidence for the ability to understand and answer questions), and other interpretations were not evaluated. Finally, the data were reported in a fragmentary manner; although there were 500 trials on the test, only a few examples of her actual responses were described. This made it difficult to ascertain exactly how Washoe had performed the task.

Given the partial reporting of the results of this experiment, all that could be said was that the data did not provide compelling evidence for a linguistic skill. It remained a possibility, however, that while the task afforded a simple, non-linguistic strategy for performance, Washoe did understand and respond correctly. However, I recently obtained the unpublished list of Washoe's responses to the 500 questions.⁷ These confirm that Washoe's strategy was, in general, to associate a small number of signs with each question frame. For example, 71%

⁶ This experiment is actually messier than this discussion suggests (see Seidenberg & Petitto, 1979). The conditions created a very complex discrimination task for which it is difficult to appropriately assess the chance probabilities of correct responses, even on an associative basis.

⁷ I thank Beatrice and Allen Gardner for providing this information.

of the responses to the question "Who that?" contained the signs *Roger*, *Washoe*, or *you*; 75% of the responses to "Who action?" (e.g., "Who eat?") contained *you* or *me*; and 58% of the responses to "Who trait?" (e.g., "Who funny?") included *you* or *me*. Collapsing across the three types of "who" questions, it is seen that four signs—*Roger*, *Washoe*, *you*, and *me*—account for 83% of all correct responses. A similar pattern holds for most of the other questions (e.g., 82% of the responses to "What now?" contained the response *time*, as in *time eat*; 100% of the responses to "Whose that?" contained *yours* or *mine*). The only exceptions to this pattern are the questions "What want" and "What that." For the former, Washoe appeared to use the strategy of responding with a food name. For the latter, she responded by correctly naming 26 different items, consistent with her performance on the box test. These data present a very different picture of her ability to answer questions than in the 1975 report.

I have suggested that there are two related problems in the ape language literature. One is the failure to consider simpler interpretations; the other is the reduction of linguistic skills to non-linguistic tasks. Both derive from attending to examples of the apes' utterances without regard to the *processes* by which they were produced. It is because of these factors that ape sign language appears to be a Clever Hans effect. Hans was the famous nineteenth-century horse who could tap his hoof to indicate the correct answers to simple problems of addition (he could also use the other three types of arithmetic operations, change common fractions into decimals and back again, and give the day of the month; Miller, 1962). Careful investigation by Pfungst (1911) revealed that the horse's responses were unwittingly cued by humans. Sebeok (1980) provides fascinating details of this case and others like it. The "Clever Hans effect" is often taken to refer to cases in which an animal's behavior is cued in a simple manner. Umiker-Sebeok and Sebeok (1980) believe that much of ape signing is cued in this manner. I don't believe that any simple notion of cuing will account for the apes' behaviors; as I have suggested, they use more general responding strategies such as "produce signs from a certain cluster in certain contexts." Unlike Hans, their behavior is not dependent upon particular signals from particular people. Much of the discussion of ape signing as a "Clever Hans" effect misses the mark because it only considers the narrow question of experimenter cuing. The Gardners, for example, stress the fact that the double-blind testing conditions of their naming test ensured that Washoe was not cued by the experimenter, while Umiker-Sebeok and Sebeok (1980) believe that unconscious, uncontrollable cuing pervades such experiments. I think it is more important to emphasize the fact that the ape could use sign production strategies that require neither cuing from the trainer nor knowledge of the language.

The correspondence between the Hans case and ape signing runs much deeper than the question of cuing. Hans could be said to add under a sufficiently narrow conceptualization of that skill. Given an inadequate theory of the task—one that focused on a narrow range of behaviors and ignored the processes by which they

were produced—it was possible to draw positive comparisons across species. Once Pfungst attended to the processes by which Hans responded, it was obvious how his behavior differed from that of a human. The inadequate conceptualization of a quintessentially human skill, the focus on a narrow range of behaviors without regard to the manner in which they were produced, and the failure to evaluate simpler alternatives, are exactly the problems I have attributed to ape language research.

What did signing apes learn? Most discussions of the signing apes' behaviors have attempted to relate them to human language. However, these behaviors may be better understood by considering the demand characteristics of the sign language experiments. The researchers attempted to create an environment in which the ape would sign communicatively. However, they may only have succeeded in creating a task which could be successfully performed if the ape merely learned the *instrumental function* of signing in the laboratory context. Assume for a moment that the apes knew nothing of the meanings or grammatical functions of individual signs, or even close associations between signs and objects. The ape could perform in a manner consistent with the descriptions in the literature by learning the *consequences* of producing particular sign sequences. The experimenter's task was to create conditions under which signing was elicited. In doing so, the apes learned that signing was highly valued. Producing signs became a means for obtaining desired outcomes (food, attention, playtime, etc.). Under these conditions, the ape's task was to determine which sign (or signs) was required in a particular context. The ape could fulfill the demands of the experimental context in several ways, the "strategies" I have alluded to above. It might associate particular signs with certain objects or general contexts. It could imitate the teacher's input. It might generate long chains of signs, increasing the probability of producing a sign that would satisfy the teacher. In other words, it could develop rituals of varying complexity that produced the desired outcomes.

I believe that this account of ape signing is essentially correct. The apes had little or no knowledge of the specific meanings or grammatical functions of signs; they were not symbolic, iconic, or indexical. None of this knowledge was required given the demand characteristics of these experiments. The ape's task reduced to finding the sign or signs that met the requirements of the teacher, who would then effect desired outcomes. These requirements varied across contexts and experiments. In the Gardners' naming test, Washoe had to produce the particular sign associated with a particular object (or class of objects). This was perhaps the most restrictive context, but only a small proportion of her sign vocabulary was tested in this way (specifically, concrete nouns). In their question test, the requirements were less restricted; here all that was needed was a sign from a set of "correct" answers. In the Nim project, in which there were no specific comprehension tests and the primary goal was simply to record a large corpus of utterances, the demands upon the subject were even more minimal.

The most extreme case is represented by the Koko project, in which nearly any utterance is satisfactory because her trainer interprets even anomalous ones as Koko being funny, or lying, or expressing a special insight.

The Nim data are again revealing in this regard. The most striking aspect of the data is that seven signs—*me, you, Nim, more, eat, drink, give*—account for a very high proportion of his output. The distribution of Nim's signs by frequency is highly skewed. A small number of signs (principally the above seven) occur very frequently; the remaining signs occur very infrequently. For example, these seven signs account for 84% of the tokens in his 25 most frequent two-sign utterances, and 83% of the tokens in the 25 most frequent three-sign utterances. The occurrence of these particular signs with numbing regularity is easily explained. The experimenters created a context in which "contextually appropriate" signing was the basic demand upon the subject. Each of the above signs could be interpreted by the teacher as appropriate. In this context it was difficult to *dissuade* Nim from producing these signs.

There are many other examples of this kind of instrumental use of language. A child may say something it does not understand, knowing only that an adult will find it funny. I know curses in several languages and that they are likely to elicit aggressive responses, but I have no idea what they mean. The apes' behavior is perhaps most closely related to that of at least some autistic children who have been trained to sign (Creedon, 1973; Konstantareas, Oxman, & Webster, 1977). My own experience with such children (those described by Creedon) suggests that they too learned to produce sequences of signs in a ritualistic fashion because it was demanded in the therapeutic context. These children learn the outcomes associated with utterances, not their intensional content. Much closer comparisons between the signing of these children and that of chimpanzees would be useful.

All of these examples fit Skinner's (1957) description of mands. Mands are instrumental utterances likely to evoke responses from the addressee. As such examples suggest, the manding function can be effected even when knowledge of the language is wholly absent. There is no need for linguistic forms to be used at all to perform this function; under the appropriate circumstances, a smile may be sufficient. Seen in this light, the fact that the apes learned to sign is not critical to interpreting their behavior; it would not be qualitatively different had they been taught to use other arbitrary behaviors in this way. If this view is correct, ape signing is more closely analogous to the use of tools than to language. Tools, like the signs in these apes' vocabularies, do not have meanings or grammatical functions. They are the instruments by which certain outcomes can be obtained. As studies dating from Köhler (1927) and Yerkes (1925) have demonstrated, chimpanzees can use tools in solving problems. Thus, the signing behavior of apes represents the adaptation of a native intelligence to the peculiar demands of the signing context. In this way, their behavior is continuous with that of apes who were not language-trained.

This discussion suggests that the apes used signs instrumentally to meet the demands of the laboratory context.⁸ It implies that their behavior might change substantially if the eliciting conditions were varied. This represents an interesting empirical question. Savage-Rumbaugh and Rumbaugh (1978) have come closest to designing experiments that would address this issue. They present a series of experiments in which chimpanzees were required to produce lexigram responses under various carefully controlled contingencies. These happen to have been instrumental, but a similar strategy could be employed to evaluate non-instrumental lexigram usage. This study provides crucial information about the cognitive capacities of chimpanzees. They describe the heroic measures needed in order to train the animals so as to use lexigrams in a non-trivial way. Their procedures involved the step-by-step manipulation of various false associations, position biases, and response strategies over a period of several months and tens of thousands of trials. My own feeling is that it will be very difficult to create conditions under which the ape uses signs non-instrumentally. The conditions in the various ape language experiments afforded the subjects many other ways of using signs, and the fact that they use them instrumentally across a wide range of conditions may reflect a powerful generalization about their behavior.

Another way to introduce very different task demands is to study comprehension rather than production. The strategies that will be effective for one task will not work for the other. Although the comprehension abilities of signing apes have only been assessed superficially, there is one careful study of comprehension in a non-human species. That is Herman, Richards, and Wolz's (1983) elegant study of two bottle-nosed dolphins. The dolphins show an impressive ability to comprehend simple syntactic structures, and are able to follow instructions to move objects around their pool. The strategies that work in the sign production studies are irrelevant to this task. Although the behavior of the dolphins is clear (unlike that of the signing apes), the knowledge and processes that mediate their performance are not. These bear further investigation and comparison to the performance of children and the signing apes. Here I only want to note the possibility that, as long as animal language researchers focus on either comprehension or production skills, the potential for creating a non-linguistic, problem-solving environment is very great. Perhaps the most powerful strategy would be to use an integrated approach in which both skills are required. The only study of this type to date is Savage-Rumbaugh and colleagues' research with Austin and Sherman concerning chimpanzees' cooperative tool use (Savage-Rumbaugh, Rumbaugh, & Boysen, 1978).

I have suggested, then, that there never was any evidence that signing apes exhibited linguistic skills. Given this radical conclusion, it is necessary to con-

⁸ The extreme statement of this hypothesis is that the ape's behavior is similar to that of a dog that has been trained to perform one arbitrary sequence of behaviors before being given a bone, and another before being taken for a walk.

sider why it was so widely believed that they had (indeed, why this is still believed in many quarters). One important factor was the existence of movies and television films documenting the behavior of Washoe and Koko.⁹ These appear to provide dramatic evidence of the apes' abilities. Personally I find these films unconvincing and misleading. The Washoe and Koko films invite skepticism about the manner in which particular examples were selected and edited. The filmmaker's methodology is fundamentally inconsistent with that of the scientist. The filmmaker's task is to present the most compelling, cinematic examples consistent with a particular point of view, while the scientist's task is to evaluate a range of behaviors without selective editing of the sample, and without a particular *a priori* commitment to the results. It is ironic that one of the basic problems in the ape language literature is that the researchers essentially followed the filmmaker's method, relying upon isolated examples consistent with their point of view. The filmed examples of novel combinations are not compelling for exactly the same reason as the published examples. Furthermore, the examples are interpreted through the eyes of the narrator. In the Washoe films, for example, much of the interpretation is provided by Beatrice Gardner, who elaborates Washoe's signing by providing stress and intonation that contribute heavily to the impression that Washoe is talking. In the numerous films of Koko, Patterson provides a running commentary that often appears inconsistent with what is on the screen.¹⁰

The films illustrate in an extreme fashion the fact that the apes' behaviors have been heavily filtered by researchers committed to a particular point of view. This filtering took at least two forms. One was partial presentation of the behavioral data. This is important because of pragmatic constraints on undertaking independent replication studies (it is for this reason that Terrace et al.'s, (1979), study has had enormous impact, despite its imperfections). Second, few people had the knowledge, either of primates or of sign languages, necessary to interpret the ape researchers' claims. So, for example, when it was falsely claimed that Washoe or Koko learned American Sign Language (ASL), few people knew enough about the language to refute the claim (Seidenberg & Petitto, 1979). Similarly, viewing the apes on film, most observers are dependent upon the commentary of the narrator-translator because they don't know the language and aren't familiar with the behavior of chimpanzees or gorillas. Independently evaluating the apes' behavior under these conditions is extremely difficult, and the

⁹ These include "The first signs of Washoe," from the public television series "Nova"; "Teaching sign language to the chimpanzee Washoe," a film widely used in teaching; and Barbet Schroeder's documentary film "Koko, a talking gorilla."

¹⁰ It is interesting to observe that viewing the ape films and films of Herman et al.'s (1983) dolphins yield very different subjective experiences. Films of the apes leave a strong impression that they are talking in a very human-like way. The dolphin films do not leave the impression that they are comprehending language, even though they have learned a far more complex skill than the apes. (I am grateful to Thomas Bever for discussion of this point.)

willingness of viewers to draw definitive conclusions about the apes' performance based on these films is difficult to comprehend.

Language and Cognition

In light of these observations, it might seem remarkable that it was so widely believed that signing apes could talk. There are other factors, largely extra-scientific, which can account for this belief, especially among the non-specialist, general public, but they are beyond the scope of this chapter. However, there is another factor that is important to consider. It is abundantly clear that apes are extremely intelligent beings. Given their general intelligence, why *shouldn't* they possess some rudimentary linguistic ability? It is the fact that apes are intelligent that generated the language studies in the first place. This question invites consideration of the studies of the apes' non-linguistic cognitive capacities.

These studies have demonstrated that apes are capable of a wide range of intelligent behaviors. The classic studies of Yerkes and Köhler showed chimpanzees to be able to solve complex problems. In the modern research of this type, Premack (1971; 1976; Premack & Premack, 1983; Woodruff & Premack, 1981; Willan, Premack & Woodruff, 1981) reports that apes can perform a number of complex tasks. Among these are making same-different judgments based on physical identity or common properties of objects; completing analogies (such as key:lock::can-opener:can); identifying causal relations between instruments and objects; and conservation of liquid or solid quantity. Savage-Rumbaugh et al. (1978) describe chimpanzees who can cooperate in order to obtain a common goal, using several tools appropriately. Although not wholly free of methodological and conceptual limitations, these studies present convincing evidence of the chimpanzee's ability to perform a wide range of intelligent tasks. The results of these laboratory studies are also consistent with the picture presented by descriptions of ape behavior under more natural conditions (e.g., Menzel, 1974; van Lawick-Goodall, 1970). Chevalier-Skolnikoff (1976) and Parker and Gibson (1979) observe that lower primates exhibit behaviors that are characteristic of Piaget's stage of sensori-motor intelligence.

Although Premack (1971) and Rumbaugh (1977) originally related their research to the question of language in non-human primates, more recently they have presented it as addressing the "preconditions" (Premack) or "cognitive prerequisites" of language (Savage-Rumbaugh and Rumbaugh). It is for this reason that the above discussion of language largely focused on the signing apes. Their current views are consistent with the Piagetian idea that language emerges out of general cognitive capacities, rather than involving a domain-specific type of knowledge. The question which then arises is this: why should an animal so demonstrably intelligent fail so miserably with regard to language? If these

studies of synonymy judgments, analogies, cooperative tool use, and the like are in fact concerned with cognitive skills that are the basis for language acquisition, they leave unanswered exactly why the apes do not progress further in using language. If the Piagetian claim that language emerges out of sensori-motor intelligence is correct, and they possess this intelligence, as Chevalier-Skolnikoff suggests, it is unclear why they do not exhibit linguistic skills comparable to that of a young child. The striking dichotomy between the apes' linguistic and cognitive capacities is the most important finding emerging from these studies and it requires an explanation.

One possibility is that while the view that language acquisition is parasitic upon general cognitive capacities is correct, apes don't possess the right ones. Absence of the "right" prerequisites would then account for the absence of language. It is difficult to evaluate this possibility because of vagueness of theories proposing that language evolves from pre-linguistic cognitive capacities. These theories rely heavily upon metaphors such as the "mapping" of linguistic forms onto non-linguistic actions, or the matching of grammatical forms and actions by "analogy," or the "building up" of linguistic forms from a non-linguistic base (Petitto, 1983). However, one relatively explicit proposal concerns the role of pointing. Werner and Kaplan (1963) and others have identified several types of pointing behavior; here I have in mind the advanced form in which the individual uses a pointing gesture to index an object or location. Indexical referencing of this kind typically involves mutual visual regard of the indexed item, and shared eye gaze and attention. This kind of pointing is distinct from (and emerges after) simpler, non-communicative uses of pointing gestures (e.g., to explore physical space). This form of pointing has been analyzed as the basis for the child's early acquisition of several linguistic forms. According to Clark (1978), the child's knowledge of verbal deictic terms such as *here*, *there*, *you*, and *me* emerges directly out of indexical pointing gestures in a natural and continuous progression. Clark (1978) also analyzes pointing gestures as nascent markers of definite and indefinite reference (i.e., the precursors of *the* and *a*). Because pointing functions to direct the adult's attention to objects, events, or people, and to convey requests, Bates, Camaioni, and Volterra (1975) analyze these gestures as protodeclaratives and protoimperatives. Once pointing gestures are firmly established, verbal deictic terms are thought to be mapped onto these "prelinguistic placeholders" (Bruner, 1981).

It appears that chimpanzees and gorillas do not use pointing gestures of this kind (see Parker & Gibson, 1979, p. 373, for discussion). Premack and Premack (1983) observed that pointing emerged among some chimpanzees in the course of learning another task. Although it is not clear whether this behavior was isomorphic with indexical pointing (rather than a simpler type of pointing), this is a potentially important finding, and it bears further investigation. It is clear in any case that indexical pointing is a complex skill that is not part of the natural communicative repertoire of lower primates.

The ape's failure to acquire simple deictic and referential terms could then be seen as secondary to the inability to point. This would at the same time seem to provide support for the idea that language production is parasitic upon non-verbal intelligence. The problem is that it isn't clear that this account of language acquisition will succeed. There are strong arguments against it (see Piatelli-Palmarini, 1980; Gleitman & Wanner, 1982). Specific evidence concerning the role of pointing is available from Petitto's (1983) study of the acquisition of American Sign Language as a first language by two deaf children. Her results strongly argue against the idea that names and deictic terms are "mapped onto" pre-linguistic pointing gestures. The study is decisive because indexical pointing is incorporated within the grammar of the language. The shift from pre-linguistic pointing to speech involves a change in modality, while the shift from pre-linguistic pointing to pointing in ASL does not. This provides a powerful means for testing whether pointing plays a crucial role in early language. If certain early linguistic forms were "mapped onto" indexical pointing, there should be no discontinuity between the pre-linguistic and linguistic uses of pointing in the acquisition of ASL. The deaf child might be expected to show facilitation in the acquisition of lexical items (such as *me* and *you*) which have the same form as pre-linguistic points, compared to hearing children learning spoken forms.

Petitto's basic findings are these. Very young deaf children use pointing gestures prior to the acquisition of any lexical signs (at 10 to 12 months). These include points to self (proto-ME) and other (proto-YOU). Acquisition of a sign vocabulary and the emergence of structured sign combinations closely follow the timetable for hearing children acquiring speech. During this early acquisition phase (approximately 1 to 2 years of age), deaf children continue to use paralinguistic deictic pointing (i.e., points that are not within the vocabulary or grammar of ASL), as do hearing children. However, both deaf children in Petitto's study failed to point to self or other during this period. That is, they did not use the signs *me* or *you*, even though their forms are *identical* to those of self-addressee indexical points that had been used earlier. Instead, they used full lexical items (names) to refer to self or addressee. Thus, a selective function of pointing disappeared for an extended period, even though the children's acquisition of the language was in all other respects normal.

For one child, pointing to the addressee returned at age 22 months, but in a remarkable way. She exhibited a consistent reversal error, thinking that the sign *you* referred to herself. Pronoun reversals of this type have been observed in some hearing children acquiring speech (Chiat, 1982). However, the deaf child's error is dramatic because she would point to another person while referring to herself, despite the transparency of the indexical gesture. She treated the *you* gesture as a name for herself despite the fact that its form is indexical. That is, she oversymbolized. She did not use the *me* gesture at this time, presumably because she already had a sign for indicating herself, namely *you*, and she continued to refer to other persons by name. The second child exhibited sporadic

pronoun reversal errors, and other errors (e.g., failure to indicate the referents of third person pronouns; substitutions of *me* for *mine* and *you* for *your*). Complete recovery of the *you* and *me* pointing gestures was not accomplished until 25 months in one child, and 27 months in the other. In sum, neither child showed a smooth transition from the prelinguistic use of self-addressee pointing to the use of these same forms as the signs *me* and *you* in ASL.

Two contrasts between the behavior of these children and that of apes should be noted. First, apes do not show the prelinguistic deictic pointing which deaf children use as early as 10 months of age. Second, during the period when the deaf children did not have control over *you* and *me* pointing, their behavior could not be corrected. Sign languages, with their external articulators, allow for physical manipulation in a way that speech does not. There are amusing examples in Petitto's data of the mothers attempting to "mold" the children's hands into *me* or *you* points. These attempts utterly fail. I do not believe that apes will show this resistance; it was not difficult, for example, to fashion a *me* sign for Nim through molding.

These findings are discussed in greater detail in Petitto (1983). The long period of selective avoidance of pointing to self and others, and the errors that initially occurred when pointing returned are difficult to reconcile with the view that linguistic forms are mapped onto prelinguistic pointing. Were this the case, no loss of the pointing function should have occurred. Furthermore, the child's acquisition of the language proceeded normally despite the disruption in the use of pointing. The reversal errors occurred only because the child had acquired meta-linguistic knowledge of the arbitrary, symbolic relationship between most signs and their referents. Unfortunately, this knowledge fails in the case of the genuinely indexical signs *you* and *me*.

Thus, the fact that lower primates do not engage in referential pointing is important, but if, as Petitto's results suggest, pointing does not itself provide the basis for early language, it will not account for their lack of linguistic skill.

Petitto's results suggest that the child's early language is not elaborated out of mechanisms of sensori-motor intelligence such as pointing. It is rare that the performance of children can be related to this question in such a direct fashion. Her performance data complement other empirical and theoretical arguments bearing on this issue (see Piatelli-Palmarini, 1980, for a summary). The research on the cognitive and linguistic capacities of apes can be seen as providing another source of evidence converging on the view that language is the result of a biologically constrained, species-specific, autonomous faculty. A direct explanation of the disparity between the ape's linguistic and non-linguistic abilities is provided by the hypothesis that language is not merely elaborated out of other cognitive capacities. If language is the expression of a domain-specific faculty—Universal Grammar in Chomsky's theory—the ape, lacking this faculty, would fail to show the ability to use language even though its other cognitive capacities could be quite sophisticated.

I think that this is the important implication of recent ape research. Studies such as Petitto's provide independent evidence for the existence of a language faculty in humans, as does much other work in theoretical linguistics and language acquisition (see other chapters in this volume). The ape research is also consistent with this evidence. Taken only as addressing the question of linguistic abilities in apes, the sign language research and related studies appear to permit only dishearteningly negative conclusions. Taken in the context of studies of the cognitive capacities of apes, and research on child language acquisition, the data provide positive evidence for the independence of a uniquely human language faculty.

This is not to deny that non-linguistic cognitive capacities play any role in language acquisition. A precise characterization of the interactions among different sources of knowledge is a primary goal of current research. It could also be the case that apes lack cognitive capacities that contribute to the achievement of higher linguistic competence. Further studies of apes may be able to provide additional information concerning both their own capacities, and the bases of language in humans.¹¹

¹¹ Since this paper went to press, there have been a number of developments in the animal language area. One was a fresh spasm of mass-media coverage of "talking apes," principally Patterson's gorilla Koko. Patterson's credibility is extremely low because she has effectively placed herself outside the constraints of organized science, since Patterson (1978) has not published her findings in the scientific journals, and her research is funded by a private foundation. Her claims are solely promoted in the mass media, which report them uncritically. Under these conditions, it is not possible to independently verify any of these claims. Anyone persuaded by the coverage in the mass media should have the privilege of watching several uninterrupted minutes of film documenting Patterson's interactions with Koko. Having seen such films, I can report that they leave a very different impression than the 10- or 20-second excerpts presented in television news reports. Similarly, at the 1985 meeting of the American Association for the Advancement of Science, Fouts reported on filmed examples of chimpanzees signing to each other. Again, having seen some of this film, it does not provide the basis for a systematic evaluation of the apes' behavior, for reasons noted in the text.

A second recent development is Savage-Rumbaugh's work with a pygmy chimpanzee who appears to acquire signing skills rapidly and without intensive training. This research is as yet unpublished, but it is certainly promising. Since the chimpanzee learns signing skills more rapidly, the study should eventually provide a much stronger basis for evaluating what has been learned.

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