

Ape Signing: Problems of Method and Interpretation

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REPORTS IN THE LAST DECADE that apes can learn to communicate with humans using a rudimentary form of sign language¹⁻⁵ have been received with great interest. For many anthropologists, linguists, philosophers, and psychologists, research with apes such as Washoe, Koko, and others appeared to provide decisive evidence bearing on several fundamental issues. For example, the belief that language is species-specific could not be sustained if apes acquired some linguistic facility, as these projects seemed to indicate. The apes' performance also appeared to rescue learning-theoretic views of language acquisition from critical analyses such as Chomsky's.⁶ The methods used in the ape sign language projects were derived from such theories, and some researchers were self-consciously behavioristic in orientation.⁷ Whatever the logical merit of such criticism of empiricist theories of language acquisition, it might be waived aside if the apes acquired linguistic skills through shaping and differential reinforcement of responses. This would at the same time undermine strongly nativist positions.

This research has gained renown outside the academic community through accounts in the print and broadcast media, documentary and educational films, and popularized texts. As might be expected, the mass media accepted the ape researchers' claims uncritically, focusing instead on speculation. If apes can talk, what will they say? Will they be

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able to provide a new perspective on the human race? Will they demand their civil rights? If language is the only capacity that distinguishes humans from other species, and apes possess that capacity, should they not be treated as human? The ape researchers themselves appeared all too willing to exploit media interest in such nonscientific issues. At the point when they were raised, the question, "What *evidence* have these projects provided concerning the linguistic capacities of apes?" was obscured. Even among scholars with a more dispassionate interest in this research, discussion was largely concerned with the *implications* of linguistic skills in lower primates, rather than evidence that they *possessed* them.

Recently, the claims on behalf of the linguistic abilities of signing apes have come under close scrutiny as a result of research on two fronts. First, a team at Columbia University (Terrace, Petitto, Sanders & Bever) attempted to replicate the experiments of the Gardners, Fouts, and Patterson.^{8,9} The Columbia team interpreted the behaviors of their subject, Nim Chimpsy, not as evidence for linguistic skill, but rather as the result of nonlinguistic processes such as imitation. We shall consider this work below. Second, the ape researchers' claims were questioned by Seidenberg and Petitto, who in a series of papers¹⁰⁻¹³ performed a close analysis of the methods and procedures of the original studies. We concluded that the available data could not sustain the radical claims on the apes' behalf, since they are compatible with and suggestive of much weaker nonlinguistic interpretations. Let us summarize those findings briefly.†

(1) The primary data in a study of ape language must include a large corpus of utterances, a substantial number of which are analyzed in terms of the contexts in which they occurred. No corpus exists of the utterances of any ape for whom linguistic abilities are claimed. The absence of this fundamental source of data means that it is impossible to evaluate the character of the apes' behavior (e.g., is it context-appropriate? imitative? spontaneous? reactive? rule-governed? repetitive?). Most importantly, its absence affects the status of the examples from the apes' behaviors (e.g., that Washoe signed *water bird* for a duck, and Koko signed *cookie rock* for a stale sweet roll), which are the primary evidence offered to this time. In conjunction with a corpus, these might constitute representative examples of the apes' behaviors. Lacking a corpus, their typicality cannot be established, nor can alternate, nonlinguistic interpretations be eliminated, for example, that they were the output of a random sign-generation process which produced a few inter-

† These issues are discussed in greater detail in the cited references (10-13).

pretable sequences amid a large amount of noise. As such, they are simply anecdotes. A small number of anecdotes are repeatedly cited in discussions of the apes' linguistic skills. However, they support numerous interpretations, only the very strongest of which is the one the ape researchers prefer, i.e., that the ape was signing "creatively." These anecdotes are so vague that they cannot carry the weight of evidence which they have been assigned. Nonetheless, two important claims—that the apes could combine signs creatively into novel sequences, and that their utterances showed evidence of syntactic structure—are based exclusively upon anecdote.

(2) Belief that apes can use signs to name entities is founded primarily on the Gardners' "double blind" vocabulary tests.^{2,14,15} These are the tests in which Washoe named objects or pictures of objects in a box. Washoe's performance on these tests is probably the most widely known and cited aspect of her behavior. The tests have been portrayed as providing hard, objective evidence of Washoe's naming skills, and her performance has been witnessed by many millions of persons through television and film. It is startling to realize how little information has been provided either about the methods and procedures used in these tests, or about Washoe's performance. Nowhere have the experimenters provided a complete description of the stimuli used, the manner in which they were presented, the scoring procedures, Washoe's training for the test, or her performance on multiple tests. It is not even clear which of her vocabulary signs were tested. The examples cited are simple objects such as *shoe* or *apple*. But she is credited with knowing many other signs—such as *good*, *mine*, *quiet*, and *sorry*—that are not exemplified by simple objects. It is unclear how such signs could be tested under this procedure. Rather than providing an account of her performance on several testing sessions, the Gardners provide examples of her performance on individual sessions. As with the anecdotes discussed above, these examples entail a sampling problem. It cannot be determined whether the cited behavior is typical of her performance, whether, for example, it was sustained across multiple testing sessions.

Much has been made of these tests because, it is asserted, the "double blind" procedures ensured that the subject's responses could not be "cued." However, this is surely false. Blind procedures merely ensure that the experimenters could not cue responses themselves. Other aspects of the test—the order in which stimuli were presented, the number of times a particular stimulus was presented, the manner in which trials were blocked, the nature of feedback, and so forth—could provide information that would increase the probability of a correct response. This is not to say that the tests necessarily provided such information, only that an adequate report would provide the relevant pro-

cedural details, and that in their absence, the cited examples are subject to a variety of interpretations.‡

Perhaps the published reports are schematic because it was believed that films of Washoe performing this task provided sufficiently compelling evidence of her skill.^{16,17} It is certainly true that the filmed examples are provocative. However, a documentary film is not a substitute for a scientific report. The method of a documentary film is to select the best examples of a given phenomenon. In presenting the vocabulary test, the phenomenon of interest was, "Washoe successfully naming objects in the box," which the filmed examples represent well. The scientific, rather than cinematic, question is different: on what proportion of all trials, with which stimuli, presented and scored in what manner, could Washoe successfully produce the correct sign? In a scientific report, all behaviors, both desired and undesired, photogenic and otherwise, must be documented and analyzed. The Washoe films would serve a useful purpose if there were a complete and unedited characterization of Washoe's performance on the tests in a published source. However, no such record exists. In the published discussions of both these tests and other aspects of ape signing, the ape researchers follow the method of the documentary filmmaker, presenting isolated examples which, however compelling, cannot be evaluated outside the context of the ape's behavior in general.

(3) Some of the strongest claims on behalf of the linguistic abilities of apes derive from Washoe's performance on a test of her ability to answer "wh-" questions.³ It is said that she could answer questions correctly and that her performance exceeded that of a child at Stage III (as defined by Brown¹⁸). Consider for a moment the procedure used in this test. Washoe's response to a question was scored as correct not if its meaning was correct in a particular context but rather if it contained a sign from a predesignated target category. For example, the question *what's that* took responses from the category *noun*; thus, if the Gardners held up an orange and asked this question, a response containing *any* noun was correct, not merely those containing *orange*. No data are included concerning the proportion of trials on which the semantically correct sign was produced. Furthermore, in contrast to the vocabulary tests, the experimenters did not consider only the first sign in a response. Thus, Washoe could answer questions "correctly" by emitting signs from her vocabulary in random order until she produced one from the target category.

‡ Given the results of the Rumbaugh's (Reference 36), it seems likely that apes can be taught to perform this task, at least with simple objects. Clearly establishing this would motivate further experiments exploring what the ape must know in order to perform in this manner.

Other aspects of this test are also disturbing. Although the test consisted of 500 trials, only a few examples of Washoe's actual replies to each question are included. This is problematical because her replies were transformed one or more times before they were scored. Eliminated from Washoe's answers were all signs that repeated those in a question, signs that appeared more than once in a reply, and signs from a class termed "markers." The latter is a heterogeneous category including *gimme*, *please*, *want*, *hurry*, *more*, *can't*, and others. Signs were eliminated from 46% of her replies. The effect of these transformations is to turn long, redundant utterances into strings that more closely resemble human utterances in their superficial form. In this way, her utterances are altered radically, before they have been fully documented.

Such methodological problems are not limited to this test. Fouts *et al.*⁵ report a test of the ability of another signing chimpanzee, "Ali," to use the sign *in*, *on* and *under*. They constructed a test in which Ali had to describe whether one object was *in*, *on*, or *under* another object. Performance on one test is given as 49.1% correct, significantly above chance for a three-choice test. The crucial aspect of their procedure is that a given sign was never correct on two successive trials. If the ape learned about this contingency during the long course of training and testing, chance level responding was closer to 50% correct, rather than 33%. With the guessing probability correctly calculated, Ali is merely performing at chance levels.

(4) The existing reports are largely concerned with the apes' production of signs. There have been no tests of their abilities to comprehend. Nonetheless, all of the ape researchers assert that the apes possessed good comprehension skills. For example, the Gardners state that "There were 132 signs of Ameslan in Washoe's expressive vocabulary and a much larger number of signs in her receptive vocabulary."³ Fouts *et al.* remark that "Ali has a sign language vocabulary of approximately 130 signs and a comprehension of English vocabulary at least equal to that" (Reference 5, page 167). Terrace *et al.*⁹ provide a list of 199 signs that Nim could comprehend; these range from *banana* and *hat* to *attention* and *false*. In each of these cases, the basis for concluding that the apes could understand signs or words is at best informal tests of some lexical items, and at worst the subjective impressions of the researchers. It is clear from the child language literature that extreme caution must be exercised before assuming that a child "understands" a word or sign. Utterances occur in rich contexts that provide the perceiver with multiple sources of information. This nonlinguistic information may permit enactment of an "appropriate" response without comprehension of the utterance's meaning. For this reason, young children's comprehension skills appear to exceed their ability to produce utterances, and their

receptive skills are frequently overestimated by untrained observers.¹⁹⁻²¹ Substantive evidence of comprehension could only be provided by tests that include controls over contextual cuing far more subtle than those in informal experiments or observations.

In concluding that the apes could comprehend words and signs, as in judging their other capabilities, the ape researchers ascribe control over complex linguistic skills to their subjects without a systematic characterization of the behavior in question and the circumstances under which it was elicited.

(5) In the absence of systematic data concerning the apes' production and comprehension of signs, it must be questioned whether there was an empirical basis on which to assign them specific meanings and grammatical functions. These attributions appear to have been made on the basis of the experimenters' *intentions* in teaching signs, not on the basis of the apes' actual behaviors. The question here is: What is the behavioral basis for deciding that a sign meant *banana, good, silly, false, large, sorry*? Inclusion of very abstract signs in lists of the apes' vocabularies underscores a serious question of over-attribution.

(6) Finally, despite a widespread assumption to the contrary, there is no evidence that the apes had acquired facility in American Sign Language (ASL or Ameslan). An ape could be said to have learned ASL only to the extent that its signing showed evidence of the grammatical structures and expressive devices characteristic of that language. There is no indication that any of the apes' signing behavior had these properties. A weaker claim would be that the apes learned lexical items of ASL—i.e., the citation forms of signs—but none of its grammar. The extent to which this is true is also unclear, since many of the apes' signs were either modified from the ASL equivalents or created especially for them.²⁻⁴

The extent to which an ape could learn ASL is limited by the degree to which ASL was provided as input. Discussions of the apes include clear descriptions of ASL as the language medium.^{2-4,14} For example, the Gardners stated,

We chose American Sign Language (ASL) because it is so widely used by humans in the United States. An alternative would have been a special language based on those gestures most easily performed by a chimpanzee. This alternative becomes much less attractive when one considers the formidable technical problems involved in the invention of a synthetic language. Moreover, it would be necessary to prove that our synthetic system was a language. [Reference 2, page 121.]

Many other statements imply that ASL is the language the apes were learning (e.g., "Washoe's exposure to Ameslan did not begin until she

was nearly one year old."³¹ "Washoe's exposure to verbal communication other than ASL was kept to a minimum."² "American Sign Language was chosen as the primary mode of communication [with Koko]. . . ."4). The degree to which the apes were in fact exposed to ASL is uncertain, however. As the Gardners acknowledge,

Perhaps the greatest discrepancy between Project Washoe as we originally conceived it and as we were able to execute it lies in the weakness of the adult models that we could provide. . . . For the most part, Washoe's human companions and adult models for signing could be described as hearing people who had acquired Ameslan very recently and whose fluency, diction, and grammar were limited because they had little practice except with each other and with Washoe. [Reference 31, page 42.]

Questions as to which sign code the apes were exposed to could only be answered by systematic analysis of the teachers' actual input. These questions assume importance because instead of characterizing the structure of the apes' utterances, the researchers merely invoke ASL, a language whose properties are independently known.^{§22-25} Given the absence of any evidence that their utterances possessed the structure of ASL, and unanswered questions as to whether they were exposed to ASL, it is inaccurate to term their behavior "signing in ASL." Of course, the apes might have acquired facility in a sign language that was not ASL, e.g., a pidgin sign. It would then be necessary, as the Gardners observed, to establish that their signing, while not ASL, nonetheless had interesting linguistic properties. At this time, however, we have no detailed information concerning the nature of their sign code.¶

THE NIM PROJECT

The data of Terrace *et al.*^{8,9} on Nim are more robust than those offered by other ape researchers. Although their data are limited in several

§ They are also important because they affect the selection and interpretation of behavioral data. For example, discussions of linear orderings in the apes' utterances as evidence for syntax^{2-4,8,9} presume that the apes learned a code more similar to English than to ASL.

¶ The published reports exhibit considerable confusion over this issue. In one paper, the Gardners state, "We do not assert that our chimpanzee subjects use the signs of Ameslan. . . ."³¹ but elsewhere they freely describe their behaviors as such (e.g., "There were 132 signs of Ameslan in Washoe's expressive vocabulary. . . ."³). Patterson states that, "In this study, simultaneous communication, or the use of sign language accompanied by spoken English, was employed. American Sign Language was chosen as the primary mode of communication. . . ."⁴ However, the structure of English is so different from that of ASL that it is almost impossible to use both languages simultaneously. "Simultaneous communication" typically consists of both spoken and signed English, not ASL.²⁶ Hence, it is not clear that the ape was exposed to ASL, nor can this be determined elsewhere in the report.

respects, they are the only systematic data on any signing ape. They recorded a large number of Nim's utterances and tabulated them according to frequency of occurrence (but not, however, according to their contexts of occurrence). The single most striking fact about this corpus is that Nim relied extremely heavily upon a small set of signs. In fact, seven signs — *Nim*, *me*, *you*, *eat*, *drink*, *more*, and *give* — account for a very high proportion of the signs in their published corpus. For example, they provide a listing of Nim's 25 most frequently used single signs during five periods of recording.⁹ In each of these five periods, the seven signs account for between 43% and 47% of the data. Nim continued to rely upon these signs even though the number of signs in his putative vocabulary increased from 33 to 157. The proportion of Nim's entire output that was composed of these seven signs may in fact be much higher if his repetitions were predominately from this class. These repetitions are deleted from the corpus, however. Similarly, these seven signs account for 84% of the signs in the 25 most frequent 2-sign utterances, and 83% of the signs in the 25 most frequent 3-sign utterances. Note that the pool of favorite signs has an interesting property: they are semantically "appropriate" in nearly every context. That is, signs such as *Nim* or *eat* could always be interpreted as correct. These signs present a difficult problem for the teacher attempting to differentially reinforce "appropriate" and "inappropriate" signs. In producing these signs, Nim appears to have developed a very simple strategy for signing "correctly" regardless of the context.

The data of Terrace *et al.* cannot provide evidence that Nim could consistently name objects or actions because the corpus is not analyzed with respect to contexts of occurrence. Thus, although Nim's behaviors are glossed as lexical items, there is no evidence that they possessed specific meanings or grammatical functions, or that they were consistently elicited by particular stimuli. Much of the remainder of their discussion is devoted to a search for evidence of syntactic organization in Nim's multisign sequences. Their strategy is to determine whether there are any statistical regularities in the combinations. Indeed, there are; for example, Nim produced the sequence *give* + X more often than X + *give* (where X is any sign); similarly, *more* + X exceeded X + *more* and *me* + X was more frequent than X + *me*. The critical fact is that although in each case there is a preference for one order, a high proportion of the tokens exhibiting this order is accounted for by a very small number of types. Thus, *give* + X occurs more than twice as often as X + *give*, but 44% of the *give* + X tokens are accounted for by only three combinations, *give eat*, *give me*, and *give Nim*. Similarly, 54% of the *more* + X combinations consist of *eat*, *drink*, and *tickle*, as do 54% of the X + *more* combinations. Note also that determining whether Nim

had learned syntactic rules for combining a sign such as *give* with other signs depends upon contextual information that is not available. Either *give* + X or X + *give* could be appropriate depending upon the context.

In comparing Nim's multisign utterances and mean length of utterances (MLU) to those of children, it is important to realize that all contiguous repetitions were deleted. In this respect, Terrace *et al.* follow the practice established by the other ape researchers. The repetitions in ape signing constitute one of the primary differences between their behavior and the language of deaf and hearing children, yet they have always been eliminated from analyses. Given their ubiquity and importance, it is essential that they be evaluated eventually. The other major conclusions of Terrace *et al.* are that Nim interrupted and imitated his teachers more often than children do in conversation. The first point is subject to further confirmation, since close comparisons between the interruptions of child and ape have not been performed. The second point is supported by a comparison which shows that Nim imitated more often than the children in a study by Bloom, Rocissano, and Hood.²⁷ If the imitation hypothesis is true, then it must be concluded that Nim's teachers provided input largely consisting of the small number of signs that occur with very high frequency in the corpus. An alternate interpretation is that Nim did *not* imitate heavily, but rather had a general strategy of producing signs from his pool of favorites. In responding to Nim, and attempting to engage him, his teachers may also have produced these signs in abundance. In effect, Nim's signing may have cued his teachers' responses, rather than vice versa. These alternatives could be distinguished empirically in several ways, e.g., by determining whether Nim imitated signs that were not from the favored pool. However, the mere fact that the same signs tended to appear in both Nim's signing and that of his teachers does not establish whether imitation was occurring or who was imitating whom.

OTHER VIEWPOINTS

The ape researchers' responses to objections to their methods and conclusions have taken several forms. Primarily, they have pointed out deficiencies in Terrace *et al.*'s project.²⁸⁻³⁰ The fact, it is argued, that Nim failed to acquire linguistic skills does not necessarily mean that apes such as Koko and Washoe lacked them as well. Nim's behavior (or lack thereof) could be a consequence of factors specific to that project. Fouts believes that Terrace *et al.*'s teaching methods were inadequate.²⁸ Patterson believes that their project was too short.³⁰ The Gardners and Fouts assert that Nim had too many teachers.^{28,30} Premack states that "it is a repetition of a non-optimal experiment."²⁸ Perhaps Nim was

simply less clever than the other apes. Rather than defend the Nim project, it will suffice to note two points. The first is that the experiment was not a "failure to replicate" earlier findings. So far as can be determined from available information, Nim's behavior closely resembled that of Washoe and other signing apes. He learned to form signs, combine them into sequences, and produce them in various contexts. Terrace *et al.* merely came to a different interpretation of these behaviors, by considering aspects of the subject's performance and that of his teachers which the other researchers ignored. In light of the similarities among the behaviors of all the signing apes — and deficiencies common to all the projects — questions as to who used the best methods are largely moot.

The second point is that criticism of the Nim project does nothing to validate claims on behalf of Washoe and Koko. We have argued that these claims are suspect because they are not supported by data. This criticism can only be countered by providing such data, not by attacking the merits of another experiment.

Another line of defense is to suggest that critics have applied more stringent criteria to the evaluation of the apes' behavior than are applied to children. The Gardners in particular believe that their critics have applied a "rubber ruler."^{29,30} They fault Terrace *et al.* for using the method of "rich interpretation"¹⁸ with respect to children but not apes. This argument is orthogonal to the primary problem, however, which lies in the absence of adequate data on which to base any interpretation. Both Patterson and the Gardners have consistently misinterpreted the "method of rich interpretation" to mean, "apply the richest interpretation to the data" without evaluating weaker alternatives.*

The Gardners' comment raises an important point. It is clear that many studies of child language suffer from the same problems of understatement and over-attribution as do the ape studies. One value of the ape research has been to emphasize the methodological issues in the study of language acquisition. However, it is also clear that evaluation of the apes' behaviors would be advanced immeasurably by having available the kinds of evidence seen in many studies of child language. Beyond this there is a limited sense in which it may be justified to require documentation of the apes' behaviors that is both more complete and more rigorous than that in most studies of children. There are several reasons why this might be so. The first is that while the general

* This reaches the heart of the matter. Researchers in this field appear content to establish that aspects of the apes' behaviors are *compatible* with a "rich" interpretation. With the exception of Terrace *et al.*, they do not actively attempt to evaluate simpler interpretations.

character of child language is well known, the same cannot be said of ape signing. Access to children for the purpose of obtaining data on their linguistic performance is easy; access to apes is not. The ape researchers' primary responsibility was to provide a full characterization of their behavior regardless of the interpretation assigned to it. This was especially important because the behavior had not been observed previously, and it could be observed independently only by a few persons at enormous expense. This is not a responsibility that typically falls on students of child language, whose data and interpretations can be verified by others.

Second, it might be argued that questions about ape language should be resolved using the most robust data that can be obtained under existing procedures. It is not sufficient simply to apply any method that has been used in a study of child language. Surely it would be wrong to decide these issues on the basis of observational and experimental data that are themselves questionable. Stringent criteria of scientific investigation are rightly invoked in the face of an unusual, implausible, or controversial claim (as, for example, in recent debates over ESP and heritability of intelligence). The nature of the language acquisition process makes it difficult to develop methods that offer the rigor available in other areas of psychology (and other sciences). In light of this, it may be that the best strategy is to seek converging evidence from a variety of sources, i.e., multiple measures of a wide range of behaviors.

It is also important to note differences in the goals which the ape researchers have set and those of persons studying child language since these affect the types of data that are cited. Psycholinguists typically do not ask of children's behavior, as some have asked of the apes' behavior, "Is it language?"³¹ Rather, they attempt to track the developmental sequence by which humans move from an initial state (lack of language) to a steady state (adult competence). Most questions in the study of child language are diachronic: How does language behavior change over time, and how do such changes reflect the growth of knowledge? The ape researchers are concerned with a synchronic question: Do the behaviors that the ape has come to produce possess certain linguistic properties? It is not that a "rubber ruler" is being applied, but rather that different kinds of evidence are necessary to decide these questions. Evaluating the apes' behavior turns heavily on the issue of nonlinguistic response strategies, which is an important issue in the study of child language, but one that is irrelevant to many other topics.

There is another important difference between research on apes and children. In interpreting child language data, researchers are able to draw upon the fact that in the absence of pathology, children learn languages and retain this skill through maturity. Although it may not be

possible to determine exactly when a child has acquired a particular type of linguistic knowledge—indeed, this may not be an empirical question—one knows that the child's fragmentary utterances are evidence for an emerging linguistic skill. We cannot have the same confidence about the apes' utterances, both because they have a very different character from those children, and because the apes do not achieve this higher competence.†

A NONLINGUISTIC INTERPRETATION OF APE SIGNING

The available information concerning ape signing—derived from published reports, films, and our own experiences with a signing ape—leads us to an interpretation that differs radically from that offered by the ape researchers. What the apes appear to have learned is not the meanings or grammatical functions of signs, or how to combine them using productive rules, or how to decode what was signed to them, but rather that signing behavior was very important. The activity of producing signs was highly valued by their teachers. Under this circumstance, they learned that the mere behavior of producing signs could be used to effect certain outcomes, e.g., getting food, social approval, release from work time, and the like. This behavior is similar to that of a very young child who has learned to say things it does not understand. Consider the child who learns to say something very adult, perhaps mildly obscene. It learns that this utterance will produce certain behaviors in the adult audience: they will laugh or titter, pat it on the head. The child understands the consequence of saying the phrase, even though it has no knowledge whatsoever of the meanings or grammatical functions of the elements, or any control over rules for combining these elements. All it has is the very general knowledge that saying the phrase is highly valued—at least until the audience tires of it.

In the service of effecting a variety of positive outcomes, the signing ape learns response strategies of various sorts. If the Nim data are representative, then it is clear that one strategy is “sign from the pool of favorites.” In the Gardners' test of Washoe's ability to answer questions, the strategy, “sign until the experimenter terminates the trial,” would have worked effectively, as would, “randomly select signs from the

† Some observers have detected another kind of bias against the ape research. Failure to accept the ape researchers' conclusions has been attributed to anthropocentric bias, an irrational attachment to belief in the “uniqueness” of man and the inferiority of other species.^{32,33} Those who criticize the ape research are portrayed as objecting not to the methods, procedures, or conclusions of the ape researchers, but rather to the mere idea that apes can talk. This move trivializes the issues, and does nothing to advance the debate.

vocabulary of responses until you hit the correct one." Being able to respond in these ways is not an uninteresting skill to have learned. In a very limited sense, it concerns the pragmatics of language use, that is, the relation between an utterance, the context in which it occurs, and the effects it has on the perceiver. This is a form of communication, but not one that relies upon shared linguistic knowledge. This behavior places the ape at a more advanced level than children with certain language disorders. For example, in some extreme forms of echolalia, children have control over the production of linguistic forms without possessing knowledge of their meanings and grammatical functions, or knowledge of the ways in which the act of producing utterances can be used to affect another. This is performance, so to speak, without either grammatical or communicative competence. Comparing the apes' behaviors to those of autistic children who have acquired primitive signing skills might also be revealing.^{34,35}

CONCLUSIONS

We believe that the above considerations establish that there is no basis on which to conclude that signing apes acquired linguistic skills. Simply stated, the existing claims are not supported by evidence. There are numerous methodological problems with this research; perhaps the most damaging is uncritical over-reliance on production data. Production is, of course, an important skill to evaluate, and it is a simple matter to record examples of such behaviors. It is far more difficult, however, to establish a systematic relationship between the utterances and their contexts of occurrence. In the absence of this information the data are of limited value.

We should close by emphasizing that evaluating the apes' behaviors is not beyond the limits of the scientific method. In this paper and elsewhere¹⁰⁻¹³ we have described the kinds of observational and experimental data that would bear decisively on the important issues raised by this research. It would be unfortunate if the inadequacies of the existing research—and widespread publicity concerning this controversy—were to inhibit further work with these complex animals, whose behavior is so little understood.

ACKNOWLEDGMENT

We are grateful to William Brewer and Edward Lichtenstein for comments on an earlier draft.

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