ed when no further thoracic temperature increase was recorded for 2 minutes.

19. In other experiments, intact butterflies with basally blackened wings or black wing stubs equivalent to the basal third of the wings (approximately 10 mm) attain a ΔT nearly 75 percent higher than wingless butterflies. In addition, paper wings cut from black construction paper and size-matched to *Colias* wings performed as well as real wings. Although some energy is transferred from the wing bases to the thorax via conduction and radiation, most of the wing-mediated increase in body temperature is due to the wings' capacity for reducing convective heat loss by trapping warmed air beneath the wings and by increasing the effective diameter of the thorax (*16, 17*).
20. Contemporary apterygotes thought to be cladistically closest to the ancestral pterygotes are the lepidotrichid thysanurans (*14*), the members of which are thermal conformers, incapable of elevating thoracic temperature by endothermy. Efficient muscular thermogenesis in insects occurs only in those with larger thoracic mass (*17*) or those with high ratios of body weight to wing area.
21. Cool environments might be due to a combination of low ambient temperature, low levels of solar radiation, and high winds such as during times of low solar angle or intermittent cloudy weather. Cool environments also include those resulting from seasonal changes or altitudinal and latitudinal effects.
22. The thermoregulatory hypothesis is also supported when the environment of the ancestral pterygotes is reconstructed. Forms ancestral to insects evolving during the Upper Silurian likely inhabited rainy swamps with discontinuous patches of terrestrial vegetation dominated by the Psilophyta [B. M. Mamajev, in *Proceedings of the 13th International Congress of Entomology* (Moscow, 1968) (Nauka, Leningrad, 1971), vol. 1, p. 269] Kukalová-Peck (*3*) suggests that these ancestral insects inhabited moist niches and were semiaquatic. By the Middle and Upper Devonian, the amphibiotic ancestral pterygota had cursorial legs and long anal cerci and a medial caudal filament. When these amphibiotic insects began climbing vegetation to feed, mate, and disperse, their pro-wings were directed latero-horizontally [the most efficient position for thermoregulation by baskers (*7, 17*)], and these forms—like modern Apterygota (for example, Thysanura)—had an incomplete development without a metamorphic instar (*3*).
23. Wind velocity can be greatly reduced within the relatively stagnant boundary layer of air that envelops large objects. A dorsally ventrally flattened ancestral pterygote with lateral thoracic extensions would have experienced minimal form drag within the boundary layer in addition to being protected from forced convective heat loss [G. S. Campbell, *An Introduction to Environmental Biophysics* (Springer-Verlag, New York, 1977), pp. 2 and 73].
24. R. J. Wootton, *Paleontology* **15** (4), 662 (1972).

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4. T. A. Sebeok and D. J. Umiker-Sebeok, Eds., *Speaking of Apes: A Critical Anthology of Two-Way Communication with Man* (Plenum, New York, 1980); M. S. Seidenberg and L. A. Petitto, *Cognition* **7**, 177 (1979). By way of an answer to these critics, see E. S. Savage-Rumbaugh and D. M. Rumbaugh, *Am. Sci.* **68**, 49 (1980).
5. A few detailed studies of the characteristics of child language are available, including R. Brown, *A First Language* (Harvard Univ. Press, Cambridge, Mass., 1973); L. Bloom and M. Lahey, *Language Development and Language Disorders* (Wiley, New York, 1978).

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Ape Language

Scarcely a decade has elapsed since we were told by the Gardners (*1*) and Premack (*2*) about the ape's remarkable capacity to learn and use a human language. Now Terrace and his co-workers (*3*), as well as others (*4*), tell us to be skeptical of those claims. This is surprising, for the main questions about how far the naturally languageless ape can learn a human language and use it in a human way have not changed in the last decade, and they still remain to be answered. Fundamentally, these questions concern what the ape can do and would choose to do with a human language in comparison to what the human child can and does.

Appropriate ape-child comparisons cannot be made by studying the ape alone; parallel studies of the human child are also needed. It is not sufficient to compare the observed characteristics of ape language with certain presumed characteristics (*5*) of child language. We need firm answers to several questions. If the ape's "talk" is cued by the human interlocutor (prompting) and is overinterpreted by enthusiastic observers (the experimenter bias), resulting in a massive "Clever Hans effect," then what is the role of these factors in the assessment of the child's language competence?

If effective verbal communication depends not only on words but also on context and situational expectations, then are not all effective human interchanges riddled with the "Clever Hans effect"? If the ape mannerlessly interrupts her interlocutor, what does a child do under similar circumstances? What does a child who says "Mommy gone to work," actually "intend" by this utterance? Would a human child brought up in a languageless environment easily learn to talk when exposed to the normal human environment? (The Indian wolf-children did not.) Does a deaf girl, trained in sign language, "talk" to her dolls while playing? What is the language competence of an autistic child who shuns human contacts but is forcibly taught a language for a couple of hours each day? Without answers to these questions, any conclusions about the significance of ape language would appear to be premature.

Clearly, an ape's level of verbal communication will not match that of a child, but might the difference between an adult's language and that of the child be of the same order as the difference between the child's and the ape's? To answer this question, we need to know a lot more about the language competence of the ape and of the child under comparable conditions. The question is answerable but not yet answered.

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The semantic and syntactic analyses carried out by Terrace *et al.* (*1*) providing evidence of structurally constrained regularities in the signed utterances of a chimpanzee are important because they support and extend earlier work (*2-4*). However, their evidence of nonhuman discourse patterns in apes is inconclusive and reveals how critical well-controlled environments may be to the acquisition and expression of language in these animals. Moreover, their use of information on the gorilla Koko for comparative purposes is selective and, in some instances, inaccurate and misleading.

Terrace *et al.* present data showing that Nim's mean length of utterance (MLU) failed to increase over a 19-month period (from 26 to 45 months of age), but do not mention my data (*2*) indicating an increase of 33 percent in the gorilla Koko's MLU over a similar 12-month period (from 29 to 41 months). Figures included from my publication are reported incorrectly: Terrace *et al.* (*1*, p. 891) report "the acquisition of more than 400 signs by . . . Koko." The figure I gave was 100 (*2*). They also cite (*1*, p. 895) the same article as reporting that 95 percent of Koko's two-sign combinations were interpretable into categories similar to those used to describe two-word utterances of children. The actual figure was 75 percent (*2*).

Terrace *et al.* describe (*1*, p. 899) the television program in the NOVA series entitled "the First Signs of Washoe" as "the best [example] of . . . Koko's signing." That program showed 50 seconds of film of Koko taken in 1974 by NOVA's camera crew during which she emitted three signed utterances. They state that "all of Koko's signs [in the film] were signed by the teacher immediately before . . . Koko signed." This is not true. Furthermore, no request was made to see our videotaped samples of Koko's signing.

Not included in Terrace *et al.*'s dis-

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cussion of comprehension of multiword sequences by apes are data showing Koko's comprehension of *novel* phrases in sign and in spoken English (*5*), or data on her replies to a wide range of *wh*-questions, including anomalous ones such as *where color*? or *what good*? to which she made logical replies (*3*).

Terrace *et al.*'s account of Project Nim reveals certain methodological problems. They mention that during a 46-month period Nim was exposed to 60 different teachers, most of whom were not fluent in sign language. During the first 46 months of training, Koko was exposed every day to one primary teacher assisted by 14 others, 9 of whom were native signers or deaf individuals. Quantitative comparisons between Nim's and Koko's spontaneity and creativity with language at comparable ages show that factors in Nim's learning environment may have had deleterious effects on his signing. Terrace *et al.* report that 13 percent of Nim's utterances were spontaneous (nonadjacent); in samples of Koko's signing (*3*), an average of 41 percent of her utterances were spontaneous. Approximately 39 percent of Nim's utterances were complete or partial imitations of those of his teachers; only 11 percent of Koko's utterances were imitative. The average proportion of Nim's imitative utterances, which were expansions of his teachers' utterances, was 7.3 percent; the comparison figure for Koko is 36 percent (*3*).

The argument by Terrace *et al.* that Nim was not creating sentences, despite evidence that his utterances showed structurally constrained regularities, was based on analyses of a severely limited videotaped sample ($3\frac{1}{2}$ hours) of Nim's signing. They claim that patterns of extensive interruption and imitation found in these samples invalidate the evidence obtained from all of their other samples. Terrace *et al.* state, however, that "None of Nim's teachers, nor the many expert observers who were fluent in sign language, were aware of . . . the degree to which Nim imitated or interrupted his teachers." It is possible that no one noticed these communicative patterns because they were specific to the recording situation and not generalized characteristics of Nim's signing.

Under relaxed (low pressure) video recording conditions, the frequencies of interruption and imitation by Koko are low (less than 10 percent) and the proportion of spontaneous to elicited utterances is high (more than 40 percent). These patterns reverse, however, when the teachers allow their behavior to be influenced by the presence of the camera.

Furthermore, when the gorilla's companions stop signing, Koko continues to sign—to herself and to initiate exchanges with others.

Finally, the unique features of sign as a visual language must be taken into account in order to appropriately evaluate the apes' performances. By eliminating from consideration such simultaneous grammatical devices as modulation (*6*), Terrace *et al.* have eliminated a key way that apes creatively use the code they have learned (*7*).

Had Terrace *et al.* presented information on the factors I have discussed, their conclusion that "there is no evidence . . . that apes can combine . . . symbols in order to create new meanings" would not be justified.

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5. F. Patterson, *Sign Language and Language Acquisition in Man and Ape* (AAAS Selected Symposium 16) (Westview, Boulder, Colo., 1978), p. 161.
6. E. Klima and U. Bellugi, *The Signs of Language* (Harvard Univ. Press, Cambridge, Mass. 1979).
7. F. Patterson, *Natl. Geogr.* **154**, 438 (1978); in *Children's Language*, K. E. Nelson, Ed. (Gardner, New York, in press), vol. 2.

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We are puzzled by Bindra's (*1*) view that the references that he cites (*1a*) have given rise only to "certain *presumed* characteristics of . . . language" (italics added). The central issue that he raises is: Do young children create sentences? Psycholinguists may not agree about the precise point at which children reliably produce and comprehend sentences. There is no basis, however, for denying that they do so. In view of the spontaneity and variety of children's utterances, and their demonstrable appropriateness (*1*), there is no reason to speculate that children's utterances are simple responses to cues transmitted by other speakers. It is one thing to pinpoint particular types of Clever Hans effects, as we did in our article, and another to characterize ". . . all effective human interchanges [as] riddled with the 'Clever Hans effect.' " We agree that the role of Clever Hans effects in human communication warrants further study.

The lack of published information on a child's tendency to interrupt appears to result more from the low frequency of interruptions in conversations between a parent and a child than from a lack of knowledge about interruptions (*2*). Research on the other issues that Bindra raises would undoubtedly add to our understanding of human linguistic competence, but at present would not appear to be relevant to evaluating an ape's linguistic capacity (*3*).

The data cited by Patterson in her comment here do not require any change in our original assessment of an ape's linguistic ability. That Koko's mean length of utterances (MLU) increased 33 percent in a 12-month period matters little since Koko's MLU reached an asymptotic value of approximately 2.0 at an age of 56 months (*4*, figure 4.3, p. 75). Patterson concludes, in a comparison of the MLU of Koko and children, that ". . . MLU development appears to be an interesting species difference" (*4*, p. 156). Of even greater importance is the lack of any evidence that Koko's use of sign language increased in complexity as her MLU increased.

The validity of MLU as a measure of a child's linguistic development rests almost entirely on the positive relation between MLU and the semantic and syntactic complexity of a child's utterances (*1*). Utterances such as "please milk please me like drink apple bottle" [signed by Koko when her MLU was 1.75 (*4*, p. 345)] and "give orange me give eat orange me eat orange give me eat orange give me you" [signed by Nim when his MLU was 1.6 (*5*)] are uncharacteristic of hearing children's speech (and, as far as we know, of deaf children's signing). Such redundant constructions suggest that what these apes have learned about sign language was to continue to produce contextually appropriate signs until they got what they wanted.

We disagree with Patterson's interpretation of our description of Nim's socialization. As described (*5*, note 16; *6*), Nim spent most of his time with a small group of teachers. The majority of Nim's 60 "teachers" are more appropriately described as occasional playmates who spent relatively little time with him (*7*). Patterson's observation that, for the first 46 months of training, Koko was exposed every day to 9 (out of 14) native signers or deaf individuals should not imply that Koko's signing environment was more advantageous than Nim's. None of the projects attempting to teach apes to sign used ASL. [(*5*), note 7, and (*8*)]. Also, the main personnel associated with this project did not profess to be fluent in ASL (*9*).

Patterson now characterizes Koko's use of sign language as spontaneous. Nevertheless she states that "The majority of Koko's utterances were not spontaneous, but elicited by questions from her teachers and companions. My interactions with Koko were often characterized by frequent questions such as *What's this*?" (*4*, p. 153). In saying that 41 percent of Koko's utterances were spontaneous, Patterson refers to samples of Koko's signing (*4*), but our examination of that reference revealed no discourse analysis. Appendix B (*4*, p. 320), however, presents five 1-hour transcripts that contain enough information for performing a discourse analysis.

Using the same rules that we followed in analyzing Nim's transcripts, we calculate that 28 percent of Koko's utterances were spontaneous (range: 16 to 43 percent). Having examined all the relevant data we could find no basis for Patterson's view that Koko's utterances are more spontaneous than Nim's (*10*). Should other films or videotapes show a different basis for Koko's signing, we would, of course, welcome the opportunity to view them.

Even if Koko's signing were more spontaneous and less imitative than Nim's, this would not demonstrate grammatical competence. A rich interpretation of a sequence of signs as a sentence has to be supported by clear demonstrations that such sequences cannot be produced by nongrammatical processes (*5*, *6*, *11*).

Patterson notes three inaccuracies in our description of her results. Two of these enhanced Koko's presumed linguistic ability. Patterson is correct that the reference which we cited (*12*) listed Koko's vocabulary as 100 (and not 400). However, elsewhere, Patterson states that she ". . . would estimate that Koko's current working vocabulary—signs she uses regularly and appropriately—stands at 375" (*13*, p. 459), and that "The cumulative number of different signs Koko had used spontaneously and appropriately was 645 at the end of 1977" (*4*, p. 76). That we stated that 95 percent, rather than 75 percent, of Koko's two-sign combinations were interpretable into categories similar to those used to describe two-word utterances of children was an unfortunate typographical error (*14*). The third inaccuracy Patterson cites has to do with our transcript of the NOVA film (*15*), a brief segment of which is devoted to Koko's signing. Without specifying our error (or errors), Patterson denies our assertion that "all of Koko's signs [in the film] were signed by that teacher immediately before . . . Koko signed." Having reexamined the film we see no reason to change our original transcript (*16*).

Because we lack the relevant details of Koko's training, we are unable to evaluate "Koko's comprehension of *novel* phrases" (*17*) (italics in original) and her presumed ability to use "unique features of sign language [such as] grammatical modulation." Patterson reports greater than chance performance by Koko on the test "The Assessment of Children's Language Comprehension" (*18*), but makes no mention of the kind of training used to make her responsive to such a test. Without such information it is difficult to assess the extent to which Koko's performance results from rote drill, a set for a particular kind of problem (*11*), or true comprehension of the relationships shown on the cards used in this test. Moreover, if Koko did understand the relationships, that would not demonstrate comprehension of *sentences*.

Having considered the arguments raised by Bindra and Patterson and having had an opportunity to observe extensive footage of Koko's signing (*10*), we see no reason to alter our original conclusion.

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References and Notes

1. D. Bindra, *Science* **211**, 86 (1981).
1a. R. Brown, *A First Language: The Early Stages* (Harvard Univ. Press, Cambridge, Mass., 1973); L. Bloom and M. Lahey, *Language and Development and Language Disorders* (Wiley, New York, 1978).
2. L. Bloom and R. Brown in personal communications have stated that their videotapes of child-parent conversations show that a child almost never interrupts when conversing with a single parent. U. Bellugi, in a personal communication, states that young deaf children almost never interrupt the parent they are signing with. Supportive data are provided by M. McIntyre's analysis of a transcript of a deaf child signing with her parent ("Learning to take your turn in ASL," unpublished manuscript, University of California, Los Angeles). It is important to note that our view that children rarely interrupt when talking with a single parent cannot be generalized to all conversations involving children. Appreciable levels of interruptions have been reported when one child talks with a second child [X. Ervin-Tripp, *Developmental Pragmatics*, E. Ochs and B. B. Schieffelin, Eds. (Academic Press, New York, 1979); E. Keenan, *J. Child Lang.* **1**, 163 (1974)]. We are, of course, also aware that in sign language there are occasions on which the signs of each signer overlap with one another (*5*, note 56a). We see no evidence, however, that these usages of signing have been apprehended by signing apes.
3. That autistic children benefit from the procedures used to teach language to apes [R. L. Schiefelbusch and J. H. Hollis, Eds., *Language Intervention from Ape to Child* (University Park Press, Baltimore, 1979); see especially chapters 17 and 18] does not imply that apes and autistic children learn language by the same processes.
4. F. G. Patterson, thesis, Stanford University, 1979 (University Microfilms International Edition, Ann Arbor, Mich.).
5. H. S. Terrace, L. A. Petitto, R. J. Sanders, T. G. Bever, *Science* **206**, 891 (1979).
6. H. S. Terrace, *Nim* (Knopf, New York, 1979).
7. See (*6*), appendix B, for details concerning Nim's teachers. It should be noted that Patterson's project also experienced disruptive shifts in personnel (*4*, p. 77ff.)
8. M. Seidenberg and L. A. Petitto, *Cognition* **7**, 177 (1979).
9. Prior to her training with Patterson, Koko spent 11 months at the San Francisco Zoo where it is clear she was handled by many individuals who had little or no knowledge of sign language (*4*, p. 42ff.). Patterson, who did not consider herself to be a fluent signer when she began to train Koko (*4*, p. 156), notes that "Koko was exposed to a bilingual bimodal language environment in which most of her models were far more fluent in English than in sign language" (*4*, p. 156). G. S. Marmor and L. A. Petitto [*Sign Lang. Stud.* **23**, 99 (1979)] note an important deficiency in the signing of signers whose first language is English when they attempt to speak and sign simultaneously: large portions of their signed messages are omitted.
10. A discourse analysis of Koko's signing in the documentary film "Koko, the Talking Gorilla" would be of interest. This film left us with the impression that, in those scenes in which both Koko and her teacher were visible, the teacher initiated most of the signing and that Koko's signing was imitative of the teacher's prior utterance. An analysis similar to the one performed on Patterson's data was performed on four transcripts of Ally and Booee (chimpanzees trained to use sign language by R. Fouts) that were appended to a dissertation written by L. Miles (University of Connecticut, 1978). An average of 16 percent of Ally's and Booee's utterances were spontaneous (range 3 to 24 percent). Copies of our analyses of Patterson's and Miles's transcripts can be obtained by writing to the first author. R. J. Sanders (thesis, Columbia University, 1980) analyzed transcripts of 15 hours of Nim's discourse with his teachers (1176 utterances). No differences were observed in the properties of the discourse seen on these tapes and those reported in our earlier article. We have not yet found any evidence that Nim's signing during sessions in which he was videotaped differed systematically from his signing in other sessions.
11. H. S. Terrace. *J. Exp. Anal. Behav.* **31**, 161 (1979).
12. F. G. Patterson, *Brain Lang.* **5**, 72 (1978).
13. ———, *Natl. Geogr.* **154**, 437 (1978).
14. In our article (*5*, p. 896), we question the psychological reality of the semantic categories used to describe an ape's combinations of signs. Thus, the value of the percentage of interpretable combinations we attributed to Patterson is irrelevant to our argument.
15. "The First Signs of Washoe," WGBH Time-Life, NOVA (Time-Life Films, New York, 1976).
16. Our transcript showed Koko signing *more*, *candy*, and *flower*. Each of these signs was signed by Patterson immediately prior to Koko's signing. *More* may have been contracted with *drink* (our interpretation); on the sound track, Patterson states that Koko signed *more food*.
17. F. Patterson, *Science* **311**, 86 (1981).
18. See (*4*), pp. 173-181; F. Patterson, *Sign Language and Language Acquisition in Man and Ape* (AAAS Selected Symposium 16) (Westview, Boulder, Colo., 1978), pp. 186-191.
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