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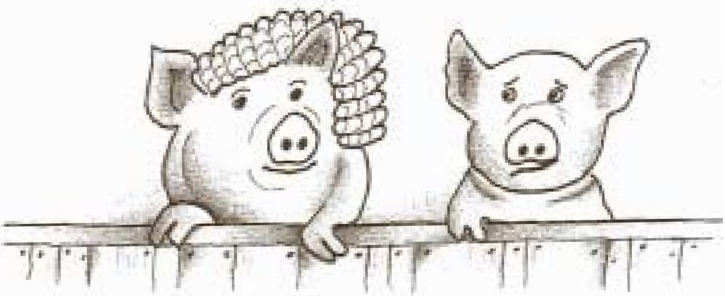
Language Instruction in the Laboratory



"They say the Doctor talks every animal language there is," said a thick fat man to his wife.

"I don't believe it," answered the woman. "But he's got a kind face."

"It's true, Mother," said a small boy (also very round and fat) who was holding the woman's hand. "I have a friend at school who was taken to see the Puddleby Pantomime. He said it was the most wonderful show he ever saw. The pig is simply marvelous; the duck dances in a ballet skirt and that dog—the middle one, right behind the Doctor now—he takes the part of a pierrot."

"Yes, Willie, but all that doesn't say the man can talk to 'em in their

> own language," said the woman. "Wonderful things can be done by a good trainer."
>
> "But my friend *saw* him doing it," said the boy. "In the middle of the show the pig's wig began to slip off and the Doctor called to him out of the wings, something in pig language. Because as soon as he heard it the pig put up his front foot and fixed his wig tight."
>
> —*Doctor Dolittle's Caravan*

Now that we have explored the naturally occurring communication systems of a variety of animals and examined some of the structural characteristics of human languages, it is time to raise a basic question: to what extent do nonhumans (especially other primates) have cognitive abilities that would support the acquisition and use of a human natural language? To put it starkly, how much of human language is uniquely available to humans?

We have already seen that human spoken languages are inaccessible to most other animals for a very simple reason. They lack the requisite apparatus for producing speech. Understanding may well be another issue, as we will discuss especially with respect to Kanzi the bonobo; but neither the vocal tract nor its controlling neurological mechanisms, as these exist in other primates, are adequate to the production of speech. Parrots do not suffer from this limitation, although they employ different means in vocalization. We will therefore conclude this chapter by examining our basic question from a perspective different from that of primate studies.

Apart from Doctor Dolittle's panglossian efforts to develop full language across the animal kingdom (and in some plants as well, in *Doctor Dolittle in the Moon*), research on language abilities that might rival our own has focused on primates, especially on chimpanzees and other higher apes. The first attempts to teach human languages to these animals got virtually nowhere, however. Chimpanzees were brought up by human parents, as normal family members insofar as possible, and unusually intensive efforts were made to teach them language. The result was extreme frustration on the part of both researchers and chimpanzees, but very little linguistic accomplishment for the latter.

One notable case of this sort involved a chimpanzee named Viki. After six years in a human family, Viki had a substantial recognition vocabulary (on the order of thirty-five to forty spoken words), but no command of ways to combine these words. She had a production vocabulary that at its most optimistic could be counted as four recognizable words: *mama, papa, cup,* and (perhaps) *up*. While not a total failure, this project came close; but some reasoned that the difficulty came from the fact that chimpanzees' abilities to produce speech (and perhaps, by extension, to perceive it) were inhibited by purely physiological limitations. We already know that in contrast to parrots, the vocal abilities of chimpanzees and other apes are limited. Their vocal tracts are different enough that they are unable to make most of the sounds that are important in human languages.

We also know that other primates are not at all successful at imitating humans, or at picking up the significance of our gestures. Monkeys are quite incapable of such imitation and interpretation, and apes have only limited capacities. Comparative studies of chimpanzees and human infants suggest that only the humans read intentionality into the actions of others and thereby extract the meaning that may lie behind those actions. Dogs, in contrast, seem to have evolved in a way that makes them quite skilled at reading human communicative signals—although their close relatives, wolves, are not.

It seems reasonable to suggest, therefore, that a good deal of the failure of the earliest ape language experiments was inevitable for these reasons alone, and that those spoken language projects tell us little about the cognitive abilities (or limitations) of nonhumans.

Just as the question of whether apes could learn human language seemed to be coming to a dead end, an alternative approach presented itself. At about the same time linguists were recognizing that signed languages (such as ASL) have all the structural properties of spoken languages, aside from modality. Researchers therefore suggested that it might be worthwhile to try to teach the apes signed languages, on the premise that their control over manual gestures is at least as effective as ours. This approach would provide science with a way to test the notion that animals can in principle learn language, while conducting the experiments in a modality that would avoid the limitations of their vocal apparatus.

Starting in the late 1960s, scientists interested in animals' cognitive capacity for language turned to investigations based on signed languages rather than spoken ones. An animal such as a chimpanzee or a gorilla has hands whose structure and controllability should put these apes well within the articulatory range of signed languages such as ASL.

The nonhuman primate's physical capacity for signed languages may not be perfect, and some physiological differences remain. Gorillas do not have as long a thumb as we do, for example, and it seems impossible for them to make the ASL "W" handshape (thumb contacts pinky, three other fingers extended). But this sort of limitation is minimal and, by and large, a signed language ought to be accessible to an ape in terms of both production and perception, if these are the only factors at stake.

We have seen that signed languages are languages in the full sense of the word—not just collections of iconic gestures, but highly structured systems that display their own phonology, morphology, and syntax. ASL and other signed languages make use of space and spatial relations in distinctive ways that are not available in the medium of sound, but these attributes do not compromise the claim that they are systems of the same fundamental sort as spoken languages, from a cognitive point of view. If an ape really could come to "speak" ASL, we would count it a successful demonstration that human language is within the cognitive capacities of an animal. Recall the caution at the end of Chapter 9, however: such an experiment must show that the animal controls the fundamental linguistic properties of a signed language, not simply that it can gesture meaningfully. Signed languages are much more than gestures, and a valid demonstration of language abilities in another species must be too.

Reaction to these studies on the part of the Deaf community has generally been negative. Many Deaf people see them as demeaning and insulting, based on the notion that while we could never teach a "real" (spoken) language to an ape, it should be possible to do so with the language of the Deaf. To the extent that research looks critically for the significant structural features of ASL in the abilities of the animals, this objection would be misplaced. Unfortunately, the standard adopted all too often is simply that of controlling an inventory of meaningful gestures. In that case, the concerns of ASL speakers are legitimate.

We can blame the lack of positive results in part on deficiencies in some of the experiments. Chimpanzees whose training was in the hands of people largely innocent of the subtleties and complex structure of ASL may have failed to acquire a system anything like the signed language for this reason alone (although hearing-impaired children exposed to rudimentary signing do in fact succeed in developing a much richer language than that of their models). The main reason for the failure of apes to learn the essential properties of a human language appears to be that, as nonhumans, they lack the human language faculty. This is not a value judgment, simply a statement of apparent fact.

Nonetheless, it is important to recognize that we probably have not come close to exploring the limits on the cognitive capacities of animals in the domain of communication. Work with a parrot named Alex (discussed toward the end of this chapter) has produced results more dramatic than anything yet seen in primates—but it is hard to imagine that a bird with a brain so much smaller than those of chimpanzees and other apes is really far more sophisticated cognitively than they are. Limitations of experimental technique, rather than of animal intelligence, therefore may have been responsible for at least some of the limitations of the results of the ape language research.

Classic Ape Language Studies

The experimental projects that tried to teach language to chimpanzees and other higher apes during the 1970s and 1980s got a great deal of attention, both from scientists and from the general public, but they were actually quite limited in number. The studies are expensive, difficult, and time consuming. They require a large and dedicated staff with special training, who must continue to work with the same animal(s) over a long period.

The work is also controversial. For some, the very notion of inducing a quintessentially human ability (language) in an ape is as close to heresy as one can get in a secular age. For others, the failures of previous work make money spent on additional projects a tragic waste of scarce research funding. Criticisms of every sort have made the whole enterprise of "ape language" research a dubious one within the culture of science. So it is perhaps not surprising that no new projects have been initiated for a number of years.

During the heyday of such research, a number of projects explored the linguistic capacities of apes. These are generally known by the name of the animal being studied: Washoe, Nim, Koko, Chantek, Lana, and others. Most were based (in principle) on a sign language as the linguistic system to be taught, though a few (Sarah, Lana, and later Kanzi) used artificial systems involving tokens or keyboards rather than manual gestures.

The first, and probably still the best known, of the early studies is the work done by Allen and Beatrix Gardner with Washoe, and it is there that any discussion of the subject must begin. The perceived accomplishments and limitations of the Washoe project provided the initial stimulus for the work that Herbert Terrace conducted with another well-known research subject, Nim Chimpsky. Terrace's essentially negative conclusions wound up having enormous (no doubt disproportionate) effects on the climate of research on this topic, and subsequent investigators have felt it necessary to discredit Terrace's results as a prerequisite to carrying out work of their own.

Three other projects deal with apes other than chimpanzees. Chantek, an orangutan, has provided interesting hints about the diversity of responses to language training in various primates, but no results that are qualitatively very different from those of the chimpanzee studies. Koko the gorilla has become a sort of folk heroine, and she stands in the popular mind as the canonical instance of "the ape who learned human language." Unfortunately, since this project represents an equally canonical example of how *not* to produce genuinely scientific results from research on the cognitive abilities of other species, we learn next to nothing of substance (though much about research methodology) from what Koko's friend Penny Patterson has written about her supposed abilities.

The studies conducted by Sue Savage-Rumbaugh with the bonobo Kanzi are totally different from those of Patterson. In addition to her earlier work with the chimpanzees Sherman and Austin, Savage-Rumbaugh has documented Kanzi's behavior and ability in great detail over a long period, and as a result a meaningful and very important record is available to consider and evaluate. It is Kanzi who presents the most serious and genuine challenge to those who doubt the linguistic capacities of any nonhuman animal. In the end, one comes away with the conclusion that Kanzi displays fascinating cognitive abilities not previously seen in any nonhuman primate—while still falling well short of what one would have to require of an animal who has truly acquired the structural core of a human language.

When we read on the science pages of the *New York Times* or elsewhere that "apes have learned to communicate in a human language, ASL," the evidence comes almost exclusively from the studies enumerated above. Such a conclusion would be incredibly interesting if it were correct, but we need to be critical and ask the hard questions. These include (among many others): How much system is there to what the apes in these experiments have learned? Have they actually learned ASL, a naturally occurring human (manual) language? If not, to what extent does what they *have* learned display the essential linguistic properties that could convince us that (like ASL) it is a natural language?

Washoe

The Gardners obtained Washoe, a wild-born female chimpanzee, at an age somewhere between 8 and 14 months. In June 1966 they brought her to a trailer in their backyard in Nevada, where their initial idea was simply to bring the animal up with sign being spoken around her, in the hope that she would learn it naturally as a human child would. In the beginning Washoe did not seem to be making much progress, or indeed to be paying any attention to the signing. In retrospect, we can see that this is not remarkable, since we now know that chimpanzees are rather poor at interpreting human gestures of any sort, even basic pointing, as significant.

Because Washoe was not progressing on her own, the Gardners modified their procedure: instead of just making signs and hoping she would catch on, they would show her an object and then mold her hands into the position for a corresponding sign. If she subsequently made the gesture on her own, she was rewarded. This theme is worth our attention: virtually all of the "utterances" we find reported in these projects are requests (directly or indirectly) for gratification, such as a preferred food, tickling, play, and the like.

The molding technique worked. Before long Washoe could produce a fair number of signs, and she had even learned a few from observation alone, without molding. The Gardners were trying to be careful and wanted to be sure that they did not ascribe a sign to Washoe without solid evidence. They established as a criterion that they would not count a sign as "learned" until it had been produced spontaneously (that is, not directly after seeing the same sign from a trainer) on fifteen consecutive days. That was easy enough at the beginning, but as Washoe learned more and more signs, she soon had no occasion to make most of them on any given day. Accordingly, Washoe's training came to include a lot of vocabulary testing, a great deal of "What's this?" activity.

By the time Washoe was 51 months old, she had acquired some 132 signs by this criterion. The project ended for her at the age of 60 months, at which point she had 160 signs. In 1970 she was "retired" to the Institute for Primate Studies at the University of Oklahoma. Roger Fouts has written in very moving terms about Washoe, her life with the Gardners, and much later investigation of his own. Interesting as the anecdotal reports of Washoe's later years may be, they do not provide data of the sort that would motivate a major revision of the conclusions from other work about the strictly linguistic abilities of chimpanzees or other apes.

Between 1972 and 1976 the Gardners brought several other chimpanzees into their laboratory. Moja, Pili, Tatu, and Dar were each adopted shortly after birth and raised with human sign language trainers much as Washoe had been. The results of these studies have elicited far less comment than the work with Washoe. Since the results were not significantly different, I mention them below only where they provide specific evidence not available from Washoe.

Washoe's signs were fairly general. They were learned with respect to a particular exemplar, of course (a specific dog as the occasion of learning to sign DOG, for example), but were quickly used in broader ways. For instance, a sign that Washoe learned early was interpreted by the Gardners as MORE. The ASL sign MORE involves bringing the two hands together so that the fingertips touch. Washoe, however, made her sign with palms facing her (only one of many instances in which her signs differed in major ways from those of the language she was supposedly acquiring). Washoe's MORE was first used together with TICKLE, and then extended to other requests.

The ASL sign for OPEN is flat hands, palms out, index finger edges together, swinging out so the two palms face. Washoe used a different "index" handshape, with hands together face down which then separated and rotated upward. Initially Washoe used this sign with three specific doors; she then extended it to all doors, containers, faucets, and the like, which goes well beyond simple imitation. The human signers in Washoe's environment did not use OPEN for a faucet.

On the other hand, OPEN is a sign which, like many others in ASL, incorporates its referent in the form of different handshapes that serve as "classifiers" for the object that opens. As a result, OPEN DOOR is distinct from OPEN WINDOW, or from OPEN in general. This aspect of structure (classifiers) is prominent in a number of signed languages that have been studied, but was never reported in the signing of Washoe—or any other ape.

The reason, at least in this instance, is not hard to find. None of Washoe's trainers controlled ASL well enough to use classifiers productively in their signing to her. Without having demonstrated command of this aspect of the natural language ASL, an animal cannot be said to have learned the language. The fault may not be Washoe's (although human children do generalize classifier usage from extremely limited input), but this is not the place to give her the benefit of the doubt.

How do we know Washoe was actually making signs, not just gesturing? The Gardners allowed a lot of sloppiness in her signs, on the grounds that her hands were shaped differently from human hands. In studies of this sort, if the observer knows what the answer is and is willing to accept rather inaccurate renditions of it, chances are all too good that the data will be overinterpreted. To prevent this, the Gardners did a series of double-blind tests, where the experimenters coding the response could not see the object the chimpanzee was supposed to identify.

Under these conditions, the observers' interpretations of Washoe's responses corresponded to the object she was supposed to be identifying about 60 percent of the time. Later experiments with Tatu and Dar produced about 70 percent and 52 percent correct answers. It is hard to determine the variation from chance here, because we do not know the size of the set of possible answers on any given trial. These experiments focused on whether the animal would produce a result of the appropriate class (as discussed below); the question of whether the answers were factually correct was secondary.

It would be valuable to know whether Washoe ever signed about things that were not present in the immediate environment. If she did, it would indicate some independence of the sign and the referent. Washoe did make signs for food that was not present (generally as a request), or actions that were not being performed (tickling). In one famous incident she heard a dog bark and made a sign for DOG. In ASL DOG is made with the right hand patting the knee while fingers snap; Washoe's sign involved a hand moving down to the side of the leg. The dog was not visually present, but it *was* auditorily present. We would need a large corpus (say, a record of all of her signing for a day or more) in order to know how much of her production was spontaneous, what kind of context was present in each case, and so forth. In fact, the only records available consist of individual isolated incidents, together with a summary of vocabulary.

What evidence do we have for linguistic structure that goes beyond the production of individual signs? Washoe often produced multiple signs in sequence, but it is tricky to know when to treat such sequences as complex combinations representing a single concept, and when to see them merely as one sign after another. Some combinations of signs do seem to have occurred, and some of these were evidently novel (in the sense of not having been present as such in the signed input Washoe saw from her human companions).

Reported examples include GIVE TICKLE, GO SWEET, OPEN FLOWER, although the last two would actually be ungrammatical in ASL. In that language, the signer would introduce the candy or the flower and assign it a location in space, then make the verb sign with an orientation to that location. We can see that Washoe's combinations were not just imitations, which attests to the creativity underlying their production. However, they make it clear that basic features of ASL (the system of spatial deixis and the indication of agreement based on it) were not controlled by the chimpanzee. Again, this may be a result of the limited knowledge her trainers had of ASL, but that does not lessen the importance of the point.

Other combinations were emphasizers: OPEN HURRY. By far the most famous of Washoe's signed combinations was her production of the sequence WATER BIRD on seeing a swan. Much has been made of the apparent creativity of this novel compound, but we would need to know a great deal about the circumstances of its production before we could construe it in that way, as I will have occasion to observe below.

Some combinations included (apparently) three, four, or more signs, and there is no reason to doubt that sequences at least that complex were possible. The manner in which the Gardners recorded and analyzed their data, however, makes it impossible to decide how much structure, if any, these sequences had.

Overall, what kind of structure *should* we attribute to the sequences of signs Washoe produced? A significant problem for the Gardners was that not much was known about ASL structure at the time, so they had little guidance with regard to what they should be looking for. Nor were they themselves particularly fluent signers. In fact, much of the time it appears that they and their assistants were not actually using ASL syntax. Most of what they produced was English, with signs substituted for words.

This "signed English" is one way that human deaf children are sometimes taught. Quite a bit of research now shows, however, that this kind of system (with signs substituted for the meaningful units of spoken English) is not actually learnable in the way a natural language is. Children exposed to such input either fail entirely to generalize within this system, or else creolize it and turn it into something else that is more like ASL. This was clearly a major methodological problem with the Washoe project. However, since that is what the input was from which Washoe was expected to learn, we have to ask how to assess her success.

To support the claim that Washoe's signing incorporated some grammatical structure, or at least some appreciation of such structure, the Gardners asked her a series of content questions (WHAT'S THAT? WHO'S THAT? WHOSE IS THAT? WHAT COLOR IS THAT? WHERE WE GO? WHERE SHOW? WHAT NOW? WHAT WANT?). The hope was that Washoe would consistently give answers to WHAT questions that would consist of common nouns, answer WHO questions with proper names, and so on. They had the experimenters ask her these questions several times a day. They collected answers until they had fifty responses to each question, and then coded the type of answer.

Mostly, Washoe did well on questions about WHAT, WHO, WHAT COLOR, and WHOSE (noun). *Where* questions, however, yielded a much higher number of inappropriate answers. When the experiment was performed with Tatu and Dar, the only questions considered were of the type WHAT, WHOSE, WHAT COLOR, and WHAT MATERIAL. The hope was to show that the animals had a system of distinct grammatical categories for their signs, but this is a peculiar interpretation to assign to what was actually tested. The categories were at least as plausibly based on semantics as on grammar, so the results tell us little if anything about grammatical understanding.

In fact, the situation is even worse than that. If Washoe was asked WHAT THAT? when shown a dog, and she responded GRAPE, she got full credit, because GRAPE is a common noun; and if asked WHAT COLOR THAT? about the same dog, she could receive full credit for ORANGE. As long as she got the right category, she did not have to give any evidence that she was answering a question about the relevant object.

Further, many answers involved more than one sign, and the sequences were systematically simplified when recorded by eliminating any and all repetition. Thus, in response to WHAT WANT? Washoe might produce YOU ME YOU OUT ME, which would then be truncated to YOU ME OUT and coded as WE OUT. The ultimate result looks like a plausible answer, but we cannot tell how much of this utterance Washoe might have intended as responsive to the question, or even how much of the recorded utterance was actually Washoe's as opposed to the interpretation of the experimenter. Since all we can see are the reduced codings, we have no idea how much redundancy and simplification were involved and subsequently cleaned up by the coding system.

So it becomes even more problematic to interpret her longer utterances as genuinely syntactic. The sequence YOU ME YOU TICKLE ME YOU TICKLE TICKLE ME YOU would get coded as YOU TICKLE ME, a result that looks much more like language than the uninterpreted original. The Gardners were explicit about the kinds of reduction they made in coding the animals' utterances, but it would still be necessary to see the originals in order to evaluate their character as language.

What about the combinations Washoe produced that were genuinely novel? We have no real way of telling that they were in fact combinations. WATER BIRD could have been a case where Washoe was asked WHAT THAT? and first attended to the water, then noticed the swan, and signed BIRD. They might be two utterances, not a combination.

It is not that these matters are undecidable in principle, only that the evidence that would help us decide is not available. In English, when we put two nouns together in a compound, they are given a particular distinctive pattern of stress. Contrast *bláckbìrd* (a compound) and *blàck bírd* (a phrase). ASL also has stress (realized by force of movement, not of course by loudness or pitch), and ASL compounds involve a shift of stress to the second element. The first sign in a compound is reduced: for instance, RIVER is a combination WATER FLOW with the first sign reduced, and GRASS is similarly like GREEN GROW with reduction of the sign GREEN.

A clear way of marking compounds therefore exists in ASL, but we have no evidence that Washoe did anything like it—or even that the Gardners would have known to look for it, since they were not signers themselves, and the indications are subtle. Without a lot more evidence, we simply do not know how to interpret these sequences, and we certainly do not know that they were intended by Washoe as complex sign combinations.

This conclusion brings up some pervasive problems with the early experiments. On the one hand, the experimenters were in many ways pioneers, so there are many matters on which we would like, in retrospect, to have much more data (and data of different sorts) than was actually collected. But there is a much less benign side of the "missing data" problem. The early experimenters did not make much useful data available for study by others. By and large, they presented only their conclusions, some summary counts, and a few appealing anecdotes, but not the data on which the conclusions were based, or enough material to allow someone else to judge the representativeness of the anecdotes. Early criticism of the work of the Gardners and others seems to have produced in them an extremely protective and defensive attitude toward their data, and that is just not the way science is done.

The Washoe project suggested strongly that it is possible to teach chimpanzees a substantial vocabulary of arbitrary signs, in the form of manual gestures with an associated meaning that is at best only partially related to the form of the gesture itself. Little or no evidence exists for any linguistic structure beyond this, and certainly none for full (or even substantial) command of a human language.

I should include another cautionary note about the individual signs. Not many of Washoe's signs were very much like the ASL signs she was supposedly learning. Her HURRY was a shaking of the wrist, while in ASL HURRY is signed with both hands in a specific handshape ("H"), palms facing, moving alternately up and down. Washoe's HURRY sign seems to have been quite unlike the ASL form. It is, however, remarkably similar to a natural gesture made by chimpanzees in the wild, identified by Jane Goodall as linked with general excitement. Not all of Washoe's signs have such obvious sources in the animal's natural gestural system, but it is crucial to establish these precedents in order to avoid inflating the inventory of "signs" we appear to have found.

Nim Chimpsky

Washoe was the first chimpanzee to undergo something like systematic training in "sign language." I have already raised some questions about whether that was actually what she was taught, and about what she learned in the way of signs—and I will return to those matters later—but that was the premise. Certainly the initial reports that came out of the Washoe project tended to make people think that a natural signed language (ASL) was what Washoe learned.

In early 1973, Herbert Terrace—a psychologist of basically behaviorist inclinations at the time—started another project, whose goal was to extend the results of the work with Washoe. As a behaviorist, Terrace was interested in the extent to which language could be taught to a chimpanzee. If language learning is merely the acquisition of a conditioned behavior, it ought to be accessible to a chimpanzee. Beyond that, he was interested in being able to talk with the animal, to find out how chimpanzees see the world. On one of the early public television programs in the Nova series, he advanced the notion that he would take Nim to Africa and use him as an interpreter with other chimpanzees.

Apart from these rather nebulous, global goals, Terrace wanted to explore the issue of how much linguistic structure a chimpanzee could acquire. Although reports from the Gardners suggested that Washoe produced not just signs, but combinations of signs, it was difficult to tell how reasonable it was to attribute linguistic structure to those combinations. Terrace wanted to ask: "Can an Ape Create a Sentence?" (in the words of the title of his well-known 1979 article in *Science*).

Terrace's bias at the outset was toward a favorable result. B. F. Skinner had proposed in 1958 that language was simply "verbal behavior" and that it was learned through the same sort of reinforcement regime as all other associative behavior. Noam Chomsky had argued that this theory was completely inadequate, and that we needed to assume a much richer innate system, especially to account for language acquisition. Terrace believed that Chomsky's refutation of Skinner was overstated and excessively *a prioristic*. Other influential psychologists (Roger Brown, for instance) also doubted that an ape could control syntax, but this opinion was based on at least some rudimentary data, as opposed to mere philosophical predisposition. Terrace hoped to resolve what he thought of as a real empirical issue.

Nim Chimpsky was a captive-born two-week-old chimpanzee when the project began. He was initially reared with a human family: that of a former student of Terrace's, Stephanie LaFarge, who had had a first try at raising a chimpanzee a few years earlier without attempting language. LaFarge knew some ASL, though she is not a Deaf (or native) signer. The premise was to raise the chimpanzee as a human infant is raised. At the age of 18 months, Nim moved from the LaFarge household in New York City to an upstate mansion owned by Columbia University.

Systematic language training had begun at 9 months. Every weekday Nim spent about five hours in a specially designed classroom at Columbia, where a great deal of recording and videotaping went on. Trainers (of whom there were many, though some, like Laura Petitto, were associated with the project over rather long periods) were supposed to sign with Nim, although for the most part they were not fluent signers either. They whispered their interpretation of Nim's signing into a tape recorder and prepared transcriptions later. A number of transcriptions of videotapes of Nim's signing at home were made as well.

The data collected in this project have largely been made available, and constitute essentially the only corpus of signing-ape data from any of the early projects. This is a rather interesting fact. As we have seen, most of the other projects adopted a rather defensive tone from the beginning, with a reluctance to let other researchers see the raw data on which their claims were based. The Gardners actually threatened to sue Terrace for the analysis he made of Washoe data derived from the Nova films.

As with Washoe, the main way Nim learned signs was by molding: the teacher would actively form Nim's hands into the desired sign. Some few signs were acquired by imitation, once the vocabulary had begun to develop. Nim's first sign (DRINK) appeared at 4 months. By the end of the project, when Nim was 3 years 8 months old, he had acquired a vocabulary of about 125 signs. He signed quite a bit, and a corpus of about 20,000 multi-sign utterances (by no means all different!) recorded during one period of two years is available for examination.

The early ape language projects often compared the abilities of the animals with those of young children at the first stages of language learning. At the very beginning, when children are producing only single words, it is hard to attribute sophisticated grammatical structure to them — and correspondingly easy to find an analogy in the behavior of an animal that produces isolated signs. Even when children enter the "two-word" stage, and begin to produce meaningful combinations, it is difficult to know how much knowledge of structure beyond mere vocabulary to see behind their utterances. Accordingly, it is difficult to refute directly a claim that chimpanzees producing sequences of signs are doing just about the same thing as children at this point. However, a growing body of evidence supports the conclusion that children have a more sophisticated understanding of grammatical structure than might be immediately evident from their productions.

The path of language acquisition in the child after the very first word combinations are produced is somewhat different from what we observe in chimpanzees such as Nim. A common (if extremely coarse) measure of this development is the child's (or chimpanzee's) Mean Length of Utterance (MLU), an index of the average length of utterances in numbers of meaningful units. From the data recorded in the Nim project, we can see that while he continued to produce sequences of signs, his MLU did not really increase. During the last year and a half of the project it was around 1.1 to 1.6, rather than rising into the 2–3 range, as we would expect for human children at (supposedly) comparable stages of development. The strong implication is that human children have a much more structured framework into which to integrate multiple word combinations than chimpanzees do.

Sign Combinations

Let us look at multisign combinations a bit more closely, to see how they might be interpreted in Nim's productions, or Washoe's, or those of any other nonhuman animal. Given a sequence of gestures that we can interpret as a two-sign utterance, there are a variety of stories we could tell about it and we need to ask how to distinguish them from one another.

One possibility is that we are simply observing superficially "complex" signs without significant internal structure. The chimpanzee has learned that certain sequences of signs have a holistically determined effect, although the components into which we might break them have no independent significance for the animal. For instance, what the experimenter analyzes as TICKLE NIM might be a complex action designed to elicit tickling, not the combination of independent ideas "tickle" and "Nim."

Another possibility is what we might refer to as the "semantic soup" theory. On this view, the chimpanzee has a lot going on in his head at a particular moment. Some of these thoughts correspond to signs he knows, and he produces the corresponding gestures. The signs that emerge reflect his ideas, but with no particular organization apart from general contextual salience. They are organized, but purely in terms of conceptual simultaneity.

Still another possibility is that the sequences we observe are formed by a system based on what Pinker refers to as "word chains" (mentioned in Chapter 8 as a finite state device). The signs are independently significant, but their order is determined as a fact about independent lexical items. For any given word, the animal has some knowledge of which words might come next, but nothing more. Thus, in any utterance where both "you" and "me" occur, Nim reportedly preferred to have "you" come first.

Finally, we might be seeing the workings of true hierarchical syntax: principles based on a classification of signs into grammatical categories, organized into constituents of various types; utterances with the form NP VP, where anything that is a possible NP comes first, and so on. And since constituents can contain other constituents, potentially of the same type, in principle this kind of structure has no upper bound of complexity. That is, it is *recursive,* although of course practical constraints on length that may be imposed by memory and other factors.

All of the above are logically possible accounts of what underlies the production of a multisign utterance by a chimpanzee (or the multiword utterance of a child). We need a way to distinguish among them; but in regard to, say, Nim, the evidence we have is really only the relative order of the signs as produced. When it comes to Washoe, the method of coding multisign utterances removes much information even about order.

With animals, the most powerful tools for exploring the degree of hierarchical, constituent-based syntactic structure cannot really be applied. That is because no chimpanzee has gotten to a point where it would be possible to ask, for instance, how to form the question corresponding to "The boy who is tall is tickling Nim." Children can tell us that this should be "Is *the boy who is tall* tickling Nim?" and thus confirm that *the boy who is tall* is a single noun-phrase constituent in their grammar (just as the single word *Nim* is), but there is as yet no way of asking anything comparable of a nonhuman language subject.

So we are left with what we can extract from the available evidence in the way of regularities of sign ordering. When we look at collections of chimpanzee utterances, seemingly the tendencies in ordering are only that: tendencies. That is, we do not find the fairly strict regularities that might be attributed to rules.

When confronted with the apparent absence of genuine rule-governed principles of ordering in the data from their chimpanzee subjects, the Gardners, Roger Fouts, and others responded in an interesting way. They argued that their chimpanzees were learning ASL, not English, and that while English has strict word order, ASL does not. The problem with this argument is that ASL has other aspects of grammatical structure that are relevant.

The basic order of sentence constituents is preferentially S(ubject)-V(erb)-O(bject), although OVS order is also possible where no ambiguity results: thus, both MARY READ BOOK and BOOK READ MARY can occur, with the same basic meaning. However, many ASL verbs are inflected to show who does what to whom: JOHN LOOK-AT MARY is signed with an orientation from a point in space representing JOHN to a point representing MARY. When a verb agrees with its arguments in this way, the order of overt noun-phrase expressions JOHN, MARY (if these are present at all, which they need not be) follows principles of discourse salience, rather than syntactic relations.

We have no evidence that the apes in any of the experimental projects ever do any of this when signing. Their ordering possibilities do not seem to be constrained by possibilities of misinterpretation, and they do not inflect signs to agree with their arguments in the way ASL signers do.

This is not surprising, actually; because most of the teachers Washoe and Nim had were not fluent signers, they did not produce "real" ASL any more than their models had. What they produced was a sort of pidgin signed English: English sentences (with words replaced by signs) with English order—though generally without grammatical markers for categories like tense and the much more limited form of agreement that English shows. Grammatical relations were indicated by regularities of order, but there is no reason to believe the chimpanzees ever picked up on this, and of course they had virtually no evidence for the grammatical mechanisms of true ASL.

Despite the intentions of the experimenters, the evidence from which their chimpanzees were supposed to learn their language was based on significant ordering of signs, not on the more order-independent mechanisms of ASL. We cannot therefore conclude that order is irrelevant in this language, and we are left with the question of just how much structure is implied by the order we find.

Structure in Nim's Signing

Terrace undertook an analysis of Nim's signing to explore these issues. Among the various possibilities suggested above, he could immediately exclude the one in which multisign combinations have no internal structure such that sequences of signs are holistic units, on the basis of the number of different token combinations Nim produced. These included something over 2,700 distinct types of combination of two- and three-sign sequences, arguably far too many for the animal to have memorized as distinct units.

Similarly, the theory that sequences derive entirely from the ordering preferences of individual items, along the lines of the word-chain model, seems excluded. Even though some items have strong preferences (for instance, MORE is generally initial), the preferences for some sequences over others cannot be derived from the independent ordering probabilities of the individual signs in statistical terms.

We are left with the possibility of significant structure, and Terrace offers one argument for a structural interpretation. The majority of Nim's (and Washoe's) multisign utterances can be classified into a small number of categories such as "agent-action," "action-object," "modifier-modified," and a few others. These are, of course, the kinds of semantic relations that are present in simple syntactically structured utterances in human languages, and perhaps Nim controlled a similar system.

But why, one must ask, does this constitute an argument for anything beyond what I have called the semantic soup theory? Perhaps Nim's internal state on an occasion when he produced a sequence of signs included an awareness of something that was going on (or that he wanted), and also of someone or some thing that was (or should have been) the agent or the object of that action. That still does not mean that the signed utterance Nim produced codes the relation among these ideas, in addition to the various components individually. To demonstrate this, one would have to show at a minimum that the orderings (of, for instance, the agent and the action) were consistent, and not derivable from some much simpler principle such as contextual salience. And in some cases (action-object, object-beneficiary), both orders of the signs involved occur with about equal frequency in the data on Nim's signing.

Nim's multisign utterances, similar to those of Washoe (to the extent we can determine this), display a marked difference from those of human children. As Nim signs more and his utterances get longer, they do not get more informative. Nim tends to produce repetitions, of the GIVE ORANGE ME GIVE EAT ORANGE ME EAT ORANGE GIVE ME EAT ORANGE GIVE ME YOU variety—many signs long, it is true, but containing only the information of "you give me (an) orange (to) eat." Human children essentially never do this, though they certainly repeat whole utterances, or even individual words, for emphasis.

In 1979 Terrace and his colleagues published a paper in the journal *Science* that had a tremendous effect on the scientific community involved in ape language studies. Their work concluded that, when one explores the discourse context of utterances, Nim's utterances rather directly reflected the teacher's signing. That is, many multisign utterances on the chimpanzee's part were actually initiated by the teacher, and involved signs that occurred immediately before in the teacher's utterance. As a result, the amount of signing where we can say that the structure is the product of the chimpanzee's control of the language is really quite small, and it provides little or no evidence for real structural regularities.

Notice that Terrace and his colleagues did not say that chimpanzees do not sign spontaneously, although some critics accused them of claiming this. Nim and Washoe clearly did make gestures when they wanted things—and perhaps for other purposes as well, though this is much less certain.

But the fact that so much of the potential evidence for syntactic structure came from prompted utterances that were at least partly repetitions of what the teacher had just said greatly reduces the evidence for syntax. Terrace showed that to the extent evidence was available (from videos extracted from the Nova presentations), close analysis of the productions of other signing apes (Washoe, Koko) showed the same repetition of teacher utterances.

While Terrace's analysis of the signing patterns of Nim and the earlier language-trained apes was carefully and accurately done, the phenomenon he uncovered may be due at least in part to the training situation in which the animals were recorded. Several years after Nim was retired from the project bearing his name and returned to the Institute for Primate Studies in Oklahoma where he had been born, another team of researchers visited him and recorded a series of interactions. His behavior when they drilled him on naming items in the way much of his earlier training had proceeded was entirely comparable to what Terrace and his colleagues recorded in their transcripts. Nim obviously did not like this activity and quickly became hostile; the session was ended when he bit the investigator. In a more relaxed and conversational interaction, however, the transcript of his signing suggests more spontaneity, and less repetition.

Under these conditions, Nim's signing was still almost exclusively related to requests for food, toys, and pleasurable activities. There is also no further evidence for structured sign combinations of a sort that would suggest syntactic organization. Still, his conversational behavior was qualitatively quite different from that in the training and testing situation. A full appreciation of what an animal can do with the communicative tools acquired in training seems to require a more creative approach than was characteristic of most of the classic ape language studies.

Terrace's central conclusion was that there was no evidence in the ape language research for syntactic abilities of the sort crucial to human language. We have no reason to question that result, even in light of the evidence that Nim had greater conversational abilities than he showed in the Columbia study. In this regard, it is ironic to note the subtitle of Terrace's book *Nim:* "A Chimpanzee Who Learned Sign Language." This subtitle was apparently introduced by the publisher, despite the much more modest (indeed, almost opposite) conclusions of the book. Most of those who paid attention to Terrace's volume interpreted the results of project Nim as showing that the effort to teach language to nonhuman primates had

failed. Funding for further research into the question became much harder to find.

After the appearance of the reports on Nim, researchers engaged in the other ape language projects became more defensive and retreated to unsubstantiated claims that Nim was an unfortunate choice of subject, or had too many teachers (thus making him more dependent on those teachers because of emotional deprivation), and the like. Of course, what Terrace had shown was that syntax could not be attributed to chimpanzees—not that they had not acquired incredibly interesting abilities. What they had learned was not human language, perhaps, but it was hardly negligible.

Projects Involving Other Apes

While chimpanzees are often said to be the apes that are closest genetically to humans, and thus the most obvious candidates for language-learning experiments, the other great apes (orangutans, gorillas, and bonobos) have also figured in this work. The number of projects involving nonchimpanzees is quite small, but two respond explicitly to the criticisms of the Nim project, so I mention them first. One involved an orangutan, Chantek, and the other a gorilla, Koko. (I discuss work with bonobos, especially Kanzi, separately.)

Chantek

Orangutans are the only Asian great apes, and they have not been the focus in as many studies of cognition as their African relatives. Chantek is the only orangutan who has been studied with respect to language ability, though his trainer Lyn Miles says explicitly that "the goal of this research was not to demonstrate whether or not Chantek had acquired 'language'" but rather "on a developmental perspective that seeks to identify the cognitive and communicative processes that might underlie language development." She concludes that Chantek did indeed develop an ability to use manual gestures (signs) in a referential way—an important result in its own right, independent of more controversial claims about full human language.

Chantek was born in captivity at the Yerkes Regional Primate Research Center in Georgia in 1977. At the age of 9 months, he was moved to the University of Tennessee, where Miles worked with him until 1986. Unlike Nim, he was raised in a fairly relaxed environment. There were no trips to a specially designed classroom for sign lessons; rather, signing was taught in his customary home cage. "Class" generally consisted of simply being around trainers who signed to him about what was going on in the environment. Again in contrast to other studies, his training involved very little vocabulary drill, and more emphasis on the utility of signing to get what he wanted or liked.

At the outset, Chantek was introduced to signs through the technique of molding, but eventually he began to pick up signs by imitation. In reporting her results, Miles uses the same strict criteria for "knowing" a sign as the Gardners, and Chantek's rate of vocabulary growth was about the same as Washoe's and Nim's. This result makes it clear that vocabulary drills are not necessary to get apes to learn signs, at least not after they have learned the first few. Miles also provides us with an indication of the number of different signs used every day, showing how this increased over time. We still do not have anything like a full record of Chantek's utterances, but this is information of a type that is not available for most other studies. We can see that Chantek continued to use old signs while learning new ones.

In reporting on her work with Chantek, Miles explicitly responds to Terrace's observation about the role of imitation in the signing of other apes. While upward of 30 to 40 percent of Nim's utterances were direct imitations of his trainers, she claims that only 3 to 4 percent of Chantek's were. About 8 percent of Nim's utterances were spontaneous, as opposed to 37 percent of Chantek's. That is, Chantek was much more likely to start a conversation, or just to start signing without prompting, whereas most of Nim's signing was in response to prompting.

Like the others, Chantek apparently began to produce multisign combinations after learning only a few signs. Miles argues that this process was not just the kind of repetition seen in Washoe and Nim, but she does not provide any lists of multisign utterances, statistics on the ratio of combinations with and without repetition, and so on, so the record is very hard to evaluate.

Miles is also quite explicit that what Chantek was exposed to was not ASL, but rather Signed English. His input had English word order, with signs substituted for words, and all grammatical markers (agreement and tense endings, articles) omitted. As a result, of course, he did not come to control ASL syntax; but we have no evidence that he controlled English syntax either.

Since no one claimed that Chantek "learned language," the importance of this work lies elsewhere. First, we note that Chantek acquired a vocabulary of about 140 signs, showing that the ability to learn this kind of communicative system is not limited to chimpanzees (and humans). As with the other apes, his gestures differed in many ways from those of actual signs in ASL—Chantek apparently liked to sign with his feet, for instance—but there is little doubt that he did develop a significant set of mostly arbitrary meaningful gestures, which he achieved with minimal explicit training.

Chantek also displayed a number of indications that his signs had genuinely referential values for him, rather than being simple context-dependent gestures. These included his signing for objects that were not present in the situation (or at least not visible), as well as extending the reference of a sign to other things that were similar but not identical to its original sense. The sign for DOG came to be used for a variety of dogs, pictures of dogs, and a number of similar animals, BEARD was used for hair in general, and many other examples occurred. Since there is no evidence that orangutans (or any other apes) use arbitrary signs in a referential way in nature, the demonstration that they can nonetheless develop such communicative skills in the laboratory is of considerable interest.

Koko

Chantek got relatively little attention in comparison with Washoe or Nim—or with another project, that of Francine (Penny) Patterson's gorilla. Koko has been consistently presented as the ape who "really" learned sign language, and who uses it the way humans do—swearing, using metaphors, telling jokes, making puns. But make no mistake, we have nothing but Patterson's word for any of this. She has not produced anything for anyone to look at except summaries (lists of signs, charts of rate of vocabulary growth), and isolated stories. She says that she has kept systematic records, but no one else has been able to study them. This project is the best illustration imaginable of the adage that "the plural of 'anecdote' is not 'data.'"

Koko was a year old when Patterson began working with her in 1972. Initially she was trained just like Washoe and Chantek, with molding of signs. Patterson also spoke aloud while signing, and it is reasonably clear that Koko's input consisted of a sort of pidgin Signed English rather than real ASL. Like Chantek, Koko caught on after a while and began to imitate. Patterson used a slightly less stringent criterion for learning than the Gardners, but also did not do a lot of artificial drilling on vocabulary. By the age of 3½, Koko reportedly had acquired about 100 signs, and by age 5 almost 250. On double-blind object recognition tests, she scored around 60 percent correct, roughly the same as Washoe and the other chimpanzees in the Gardners' studies.

Although limited amounts of summarized information about Koko's signing were published in the early years of the project, none of it included the kind of raw data scientists would need to come to a reasoned assessment of her abilities. Patterson says that she keeps detailed records and transcripts of Koko's signing, that she videotapes extended sessions, and so on, but none of this material has ever been available to outside scientists for analysis and assessment.

Since 1981, information about Koko has come only in forms such as Nova or National Geographic television features, stories in the press, children's books, Internet chat sessions (mediated by Patterson as interpreter and translator in both directions), and the ongoing public relations activities of the "Gorilla Foundation" (currently soliciting funds to enable Koko and her entourage to move to Maui). We are told a great deal about how clever and articulate Koko is, but in the absence of evidence it is impossible to evaluate those claims. And what we do have does not inspire great confidence. Here is dialogue from a Nova program (filmed ten years after the start of the project), with translations as provided for Koko's and Patterson's signing:

Koko: YOU KOKO LOVE DO KNEE YOU
Patterson: KOKO LOVE WHAT?
Koko: LOVE THERE CHASE KNEE DO
Observer: The tree, she wants to play in it!
Patterson: No, the girl behind the tree!

Patterson's interpretation that Koko was indicating a wish to chase the girl behind the tree is not self-evident, to say the least.

It would be extremely useful to have real information on the abilities of gorillas to learn and use arbitrary symbolic gestures, and on the relationship between these abilities and other aspects of language and communication. Unfortunately, apart from a few data summaries produced in the first years of the project (when Koko's progress seemed parallel to that of Washoe or Nim), the Koko project has not provided such information.

Kanzi and Other Yerkes Studies

The studies we have been looking at so far attempted to teach nonhuman primates what the experimenters thought to be a natural human signed language. A somewhat different approach has characterized studies conducted at the Yerkes Regional Primate Center in Atlanta, Georgia. These were initially designed and carried out by Duane Rumbaugh and his colleagues, including his wife Sue Savage-Rumbaugh, who has become the principal scientist identified with this work.

What set these projects apart was that they did not attempt to teach ASL or any other naturally occurring language, but rather employed a completely artificial symbol system. It was based on associations between arbitrary graphic designs called *lexigrams*, presented on a keyboard connected to a computer, and meanings. Instead of producing a series of manual signing gestures, the experimental animal was expected to press the keys corresponding to what he (presumably) meant.

Prior to the lexigram studies, the general approach of devising an artificial system was tried out in David Premack's work with a chimpanzee. Sarah was trained to manipulate arbitrarily shaped and colored plastic chips on a magnetic board. Her impressive achievements included apparently learning the reference assigned by her human trainers to these chips, and developing categories of meaning. The relevance to studies of language has been widely acknowledged to be quite limited, however, and I will not treat it in detail. Its primary importance to our story is the way in which Sarah's plastic chips paved the way for later work with overtly artificial systems.

Duane Rumbaugh worked with Lana, who was the first chimpanzee taught "Yerkish," the keyboard-based language of lexigrams. Lana's training was intended in part to see whether she could learn a limited syntax. Some sequences of lexigrams were "grammatical" and others were not. Lana was supposed to produce expressions in this language to get rewards. She did achieve some success and, even more than Sarah, demonstrated skills in the domain of symbolic (and numeric) representation and reasoning.

A host of limitations on both the "language" and Lana's performance makes it difficult to draw serious conclusions about her linguistic abilities. The experimenters themselves considered that Lana had shown at least some syntactic ability, but even the most charitable interpretation of her utterances would not go beyond structure attributable to a very limited word-chain model. Rumbaugh and his colleagues have acknowledged that the Lana project was useful largely for what it taught them about research methodology.

A somewhat more significant experiment was then conducted using two chimpanzees, Sherman and Austin, who were trained by Sue Savage-Rumbaugh to use the lexigram keyboards. At first they learned to request things from each other, and later to name objects, though they seemed to have a lot of trouble transferring what they learned on one of these tasks to the other. Identifying a banana with a lexigram did not transfer directly to asking for a banana (with the same lexigram), for instance.

After a number of years of training, Sherman and Austin could do several things of interest, in addition to the appealing (though less cognitively significant) trick of using their keyboards to cooperate in obtaining rewards under complex circumstances. They could learn new lexigrams from observation alone, then use these lexigrams in new contexts. Further, they could use lexigrams to attribute properties (including color) to an object presented only through another lexigram. Thus, they could "say" that a banana is yellow without having to see an actual banana at the time. They could also classify lexigrams into one or the other of two groups depending on whether the referent was a food or a tool, strongly suggesting that the lexigrams had genuine meaning for the apes.

These results, certainly intriguing, were not particularly revealing about the presumed ability of chimpanzees (or other primates) to learn a real language. The constructed nature of Yerkish allowed the experimenters to avoid some problems presented by real (spoken or signed) languages, but the amount of structure present in the system is limited and certainly far from that in any real human language.

The research that stands apart from all of the other work with apes began when Savage-Rumbaugh began to work with Matata, a bonobo. Bonobos were long considered to be a smaller, "pygmy" form of chimpanzee, but primatologists have come to appreciate that they are actually a different species. Extremely rare in nature, they are lively and intelligent, and have a somewhat elaborate social organization in which males and females share food and child-raising responsibilities, engage in sex for social and not purely reproductive reasons, and display other traits rather atypical of their fellow nonhuman primates.

Matata was to be trained to use the lexigram keyboard like Sherman and Austin, but she turned out to be rather a poor student. Many long training sessions, with experimenters pressing lexigram keys on a keyboard connected to a computer (which responded by lighting up the key and also producing the spoken English word) and indicating the intended referent, seemed to get nowhere. Matata was evidently too old to learn this particular new trick.

Then something remarkable happened. Matata's infant son, Kanzi, was present during these training sessions, since he was too young to be separated from her (although he was considered more of a nuisance and a distraction than an experimental subject). When Kanzi was about 2½ years old, however, the unsuccessful Matata was removed to another facility for breeding. Suddenly Kanzi emerged from her shadow, showing that although he had had no explicit training at all, he had nonetheless succeeded as his mother had not. He had obviously learned how to use the lexigram keyboard in a systematic way. For instance, he would make the natural bonobo hand-clapping gesture to provoke chasing, and then immediately hit the CHASE lexigram on the keyboard.

From that point on, the focus of the work was on the abilities Kanzi had developed without direct instruction. His subsequent training did not consist of formal keyboard drills, with food and other treats as rewards for successful performance. Instead, the keyboard was carried around and the trainers would press lexigrams as they spoke in English about what they and the animals were doing. For instance, while tickling Kanzi, the teacher said "Liz is tickling Kanzi" and pressed the keyboard keys LIZ TICKLE KANZI. Kanzi himself could use the keyboard freely, which he did to express objects he wanted, places he wanted to go, and what he wanted to do. More structured interactions took place, as when Kanzi was specifically asked to "Show me the tomato lexigram" or to press a key in response to "What is this called?"

By the time he was about 4 years old, Kanzi had roughly forty-four lexigrams in his production vocabulary (according to a criterion that required consistent, spontaneous, and appropriate use), together with recognition of the corresponding spoken English words. He performed almost perfectly on double-blind tests that required him to match pictures, lexigrams, and spoken words. He also used his lexigrams in ways that showed clear extension from an initial specific reference to a more generalized idea. Thus, COKE came to be used for all dark liquids and BREAD for all kinds of bread (including taco shells). Certainly, further questions can be (and have been) asked about just what the lexigrams represent for Kanzi. Nearly all of the ones on which he can be tested for comprehension involve objects, not actions, so the richness of his internal representation of meaning is difficult to assess. Nevertheless, the lexigrams definitely appear to have a symbolic value.

Kanzi is reported to have used his lexigrams not just when interacting with an experimenter, but also when alone. He would take the keyboard away and press keys in private. He might press PINE-NEEDLE and then put pine needles on the key, press ROCK and put little rocks on the key, press HIDE and then cover himself (or the keyboard) with blankets. If a human attempted to interact with him while he was doing this, he would stop immediately. As a result, no systematic data exist on his private keyboard activities. We have anecdotes that are enormously suggestive, but no information about the possibility that he may have pressed the keyboard by himself many more times in random or otherwise unintelligible ways. The same can be said about the reports that Washoe and other chimpanzee subjects from earlier experiments made signs in private while looking through magazines and books of pictures. It certainly looks as if these animals are "talking" to themselves, but we need much more evidence to understand exactly what is going on.

Kanzi's Control of Syntax

Kanzi surely learned a collection of "words" in the sense of associations among an arbitrary shape (the abstract lexigram pattern), an arbitrary sound (the spoken English equivalent), and a meaning of some sort, and he can use these symbolically, independent of specific exemplars or other contextual conditions. Over the years, his vocabulary has continued to expand. His keyboard now contains 256 lexigrams, and his recognition vocabulary for spoken English includes many more words.

What can we say about Kanzi's potential syntactic ability? A major difficulty is that we need to assess two different and incommensurate systems, those of production and of recognition. Kanzi's production centers on the keyboard, and he understands a great many things in spoken English. He cannot, of course, produce English words, although he is reported to vocalize sometimes in ways that suggest an attempt to form spoken words. Let us look at each of these systems in turn for evidence of syntactic understanding.

When Kanzi uses his keyboard, he does not produce enough multilexigram sequences to permit true analysis of their structure. This is not to say that he does not produce complex utterances, however. In addition to his keyed lexigrams, he uses a number of natural, highly iconic gestures with meanings such as "come," "go," "chase." He also employs pointing gestures to designate persons, and he frequently combines a lexigram with a gesture to make a complex utterance. We might be able to analyze those combinations to see what emerges in terms of potential rules of grammar.

When we do so, we find some reliable tendencies, such as the orders action-agent, goal-action, and object-agent. These are somewhat unusual, for they certainly are not the orders that occur in Kanzi's input. English has agents preceding actions, not the other way around, and so on. In any event, a semantic analysis of these orderings is beside the point, because virtually all Kanzi's complex utterances of this type conform to a single overarching rule: lexigram first, then gesture. This principle of combination is intriguing, based as it is on the modality rather than the content of the symbolic expression, but it does not provide any support for syntax.

The principal evidence that has been cited for Kanzi as a syntactic animal comes not from his production, but from his comprehension of spoken English. An extensive study explored Kanzi's understanding in relation to that of a human child (Alia, the daughter of one of his trainers) at a similar stage of language development—at least in terms of vocabulary and MLU. A complete presentation and assessment of this study (and subsequent work on this aspect of Kanzi's abilities) requires far more space than we can devote to it here. One great advantage of the studies of Kanzi in general is that many of the relevant data have been made generally available, and those who are interested can explore the facts and come to their own conclusions.

Both Kanzi and Alia showed considerable ability to respond appropriately to requests like *Put the ball on the pine needles*, *Put the ice water in the potty*, *Give the lighter to Rose*, and *Take the snake outdoors*. Many of the actions requested (squeezing hot dogs, washing the TV, and the like) were entirely novel, so the subjects could not get along by simply doing what one normally does with the object named.

The range of possibilities correctly responded to by both Kanzi and Alia was sufficient to demonstrate that each of them was able to form a conceptual representation of an action involving one, two, or more roles (participants and/or locations) and then connect information in the utterance with those roles. This is the sort of representation of meaning that linguists refer to as a "thematic" description, with the individual participants associated with distinct "theta roles." It seems likely that many animals have internal representations of complex concepts with this character, but Kanzi is the first nonhuman in whom we have evidence for an ability to link the various parts of such a representation with parts of a communicative expression.

We can also see that the connections Kanzi makes between parts of what he hears and parts of a complex, thematically structured concept respond to some extent to the form of the utterance. He can satisfactorily distinguish between *Make the doggie bite the snake* and *Make the snake bite the doggie.* At a minimum, he must be sensitive to regularities in the order of words; he did not simply interpret the content words of a sentence in their most familiar way, or in some consistent, invariant way.

These facts provide evidence for something like a word-chain model, which has regularities in terms of what follows what (for instance, agents precede actions and objects follow them). This is a totally unprecedented result in the literature on animal cognition, but it does not in itself argue that Kanzi represents sentences in terms of the kind of structure we know to characterize human understanding of language. Much of what we see might not rely on any particular structure, but rather result from a sort of "substitution in frames" procedure. That is, perhaps Kanzi has learned that certain complex utterances have places in them where there is room for one of a small set of different possibilities. Such an analysis would not require any appreciation of hierarchical organization, constituent structure, or the like. The range of patterns on which Kanzi has been tested is limited, but very little in the way of structural knowledge seems to be required.

In fact, on those sentences whose interpretation depended on information provided by grammatical words, such as prepositions or conjunctions, Kanzi's performance was quite poor. Distinctions such as that between putting something *in, on,* or *next to* something else appear not to have been made. Sentences with *and* (whether conjoining nouns, as in *give the peas and the sweet potatoes to Kelly,* or sentences, as in *go to the refrigerator and get the banana*) frequently resulted in mistakes of a kind that suggest such words simply went uninterpreted.

One class of sentences on which Kanzi did well supposedly showed his ability to understand the structure of relative clause constructions: *Go get the carrot that's in the microwave.* But it does not follow from his ability to respond appropriately to this request that he has understood it on the basis of a hierarchical structure with an embedded relative clause. If we attend only to the content words here (*go get, carrot, microwave*) and try to fit them into a semantic schema, *carrot* obviously has to be the object of getting, but *microwave* has no role to play in that action and can only be interpreted as a property of the carrot (its location). A coherent interpretation requires an appreciation of meanings and their thematic structure, but not of specifically grammatical organization.

Actions and objects (as represented by concrete verbs and nouns) correspond to things in the world, and they can constitute the meanings of symbols for Kanzi. Grammatical markers, however, get their importance not by referring to something in the world, but by governing the way *linguistic* objects are organized. Kanzi has a method for associating the referential symbols he knows with parts of complex concepts in his mind when he hears them. This method does not involve genuinely grammatical structure, so "words" that have significance solely in grammatical terms can only be ignored.

It may seem that I have gone to great lengths to avoid the conclusion that Kanzi has a meaningful appreciation of the grammar of English, given that he can apparently understand many English sentences. It is certainly not my intent to underestimate the interest and importance of the abilities that Savage-Rumbaugh has demonstrated and carefully documented in Kanzi. But while the evidence available takes Kanzi far beyond the other animals whose cognitive and communicative abilities have been studied, it does not in fact show that he has acquired an understanding of the syntactic structure of a natural language. Without that, he cannot be said to have acquired language in its core sense.

Apes and Language

Having surveyed the evidence that is available from the attempts to teach apes a human language, we can now draw some conclusions. Apart from

Savage-Rumbaugh's ongoing work with Kanzi and other bonobos, it is unlikely that further projects of this sort will be undertaken in the near future, in part because of the perceived air of failure that surrounds the earlier efforts. That is unfortunate: while it seems evident that apes do not have the specialized cognitive faculty that would allow them to "learn language" in a complete way, the research has demonstrated abilities in these animals that had not previously been suspected, and about which it would be exciting to learn more. It may be that at least some of the limitations of the existing body of evidence are limitations of the experiments, and not necessarily of the subjects.

Some factors are obvious. No ape can learn to *speak* a language like English, because the anatomy of their vocal tracts is incapable of producing the relevant range of sounds. Some factors are less obvious, but probably true (and relatively uncontroversial). Apes reach a plateau as far as complexity of expression is concerned. No matter how extensive the training, no animal is going to produce long, complex sentences. If we want to know whether an ape can develop an ability to use a human language that is comparable to that of even a grade-school child, the answer is a definite no.

But we can ask a different question: Do the apes in these experiments show evidence of having learned something that has significant resemblance to human language—a system that has some properties human languages have, and naturally occurring systems of animal communication do not have? Let us enumerate the essential components of our knowledge of language, then look for evidence in the ape experiments that bears on the animals' achievements with respect to each element.

Our knowledge of language includes at least the following:

- *Lexicon:* a collection of *words,* in the sense of a set of arbitrary associations between external expressions (in sound or signs) and meanings.
- *Phonology:* a discrete combinatorial system that supports the combining of formative elements (sounds or the formational components of signs, including handshape, location, and the like), taken from a small basic set, into expressions that are linked to meaning as words.
- *Syntax:* another discrete combinatorial system, which licenses the combining of words into phrases, of phrases into larger phrases, and so on. This system derives its force from the fact that it is based on word classes, grammatical relations, and other properties. In particular, it is

recursive, so it accommodates an unlimited range of distinct sentences on the basis of a relatively small set of "known" words and rules.

In this listing I have more or less left out semantics, the principles by which the meaning of a complex expression is determined on the basis of the meanings of its parts and the manner of their combination. Unless a system includes complex syntactic structures, it makes little sense to explore the ways in which these might be assigned an interpretation. I have also left out principles like those that determine the interpretation of pronouns (see Chapter 3). These and other aspects of human knowledge rest on the foundation of syntactic structure, so the first aspect to explore is whether apes have a system with that essential structure in place. It does not make sense to ask whether they can learn how to interpret pronouns if they do not have knowledge of the kind of structure on which the working of that system rests.

Postponing the question of a lexicon for the moment, let us start with the matter of a phonological system. Do any of the animals we have discussed have a discrete combinatorial system at the base of their meaningful communicative expressions? In the case of lexigrams such as those employed by Kanzi (and before him, Lana, Sherman, and Austin), there is no question of any system. The lexigrams are carefully constructed, in fact, so as to consitute unanalyzable wholes. In the case of signs, we have seen that the apes get these structural matters wrong, and get them wrong in ways that suggest they do not grasp the notion of a specific set of formational elements.

For instance, the animals in these experiments show no awareness of the fact that in a language such as ASL certain handshapes are possible and others are not. When the apes make up novel signs, as they sometimes do, or distort the form of signs they are shown, there are no obvious constraints on the shape their hands adopt apart from those of physiological necessity. Recall that in ASL the difference between basic forms of pronouns (I, you, he/she/it) and possessive forms (my, your, his/her/its) is systematically a difference between a pointing and a flat handshape. While some of the apes have learned MY in relation to I, they show no appreciation of the generalization of that difference to YOUR, HIS, and the rest. In general, we find no evidence of any combinatory system underlying the expression system of any of the apes. Indeed, we will suggest in Chapter 11 that this absence may be related to the fact that their vocabularies seem to be limited to a few hundred signs at most—small in comparison with the lexicon of even a rather young child.

What about the special case of Kanzi, who clearly recognizes a variety of spoken words? I argued in Chapter 5 that speech recognition in people is based on a motor theory, and on a decomposition of the speaker's activity into abstract formational elements of motor control. Of course, the reason we make this kind of assumption about humans is in part because of the speed, efficiency, and flexibility with which we recognize an unbounded range of possible sound combinations. Because Kanzi does not have more than a few hundred words (on the most optimistic assessment) to distinguish, no such argument is valid.

Savage-Rumbaugh has argued that Kanzi has a "phoneme-based" system for recognizing words, an argument that I find extremely weak. What she did was present him with three choices for a spoken word: the correct choice, one that shared the beginning sound, and one that shared the final sound. Thus, *paper* might be the stimulus, and *paper, peaches, and clover* the possible responses. Kanzi did very well at choosing the original word correctly, but what does that prove? It just shows that he can discriminate among (holistic) acoustic patterns that overlap somewhat in physical form. There is no reason to presume that any analysis of the internal structure of the pattern is responsible, for none is necessary. Many animals actually can learn to discriminate members of a small closed inventory of human vocalizations—just as we can learn to discriminate theirs.

What about syntax? Do the animals in these studies develop a discrete combinatorial system? That would require that they combine elements, of course. Discrete elements. And that they combine them according to a system, one that is based on generalizations such as the fact that nouns behave in one way and verbs another; and that noun phrases have the same form regardless of whether they are used as subjects, objects, or in some other grammatical function.

We must distinguish the animals' production from their recognition ability, since the evidence is somewhat different in the two cases. In terms of production, the range of their sign combinations is rather limited. Furthermore, the predominance of repetition in longer sequences suggests something like the semantic soup view: at a given time many things are salient to the animal, who makes signs (or chooses lexigrams) that correspond to them—but individually, rather than as a complex internally structured whole.

What we need in order to establish a syntactic view of the animal's competence is a set of rule-governed regularities. What we get, however, is at best statistical regularities. Some ape language researchers argue that their animals behave in a way that corresponds to early stages of language acquisition in human children. However, the regularities in children's speech are categorical, not merely statistical tendencies.

An exception may be Kanzi's combinations, which seem to reflect the genuine rule that "lexigram comes before gesture." This is, however, a strange sort of rule, since it involves not two distinct grammatical categories, but two quite different modalities. Apart from this single odd example, the other regularities we find look more like word-position preferences (YOU before ME) than like structurally based regularities (subject-verb-object). The proposed objection that the lack of regular order in the animals' productions is related to the fact that ASL has free word order does not survive examination, since the apes did not have ASL as input and they did not produce the specific devices that ASL uses. The bottom line is that there is little or no evidence for any real combinatory structure in the productions of any of these animals.

On the perception side, by far the best evidence is the set of perceptual tests given to Kanzi. I suggested above that Kanzi's recognition system for English allows him to make connections between spoken words and particular roles in a semantic (or thematic) structure. Furthermore, the connections he makes are sensitive, to some degree, to word order. From these facts we conclude that he may have structure of the sort we should call a word-chain model. If confirmed in further research, this would be a remarkable fact; no other nonhuman animal has plausibly been shown to do better than semantic soup on the informal scale we have been using. It is still a long way from syntax of the sort found in human languages, however.

Much more would need to be shown before we could accept the claim that Kanzi (or any other animal) has a real appreciation of the syntactic form of sentences in a natural language. To say that is not to denigrate his remarkable achievements, or to cling to an outmoded exaggeration of human uniqueness. It is merely to require evidence commensurate with the capacity that is being attributed to him. Unfortunately, those who conduct these experiments are often unfamiliar with the real nature of syntax in human languages, and they tend to accept any sort of demonstrated combination of meaningful elements as "syntactic" enough to count as language-like. If one believes that syntax is simply a matter of putting words (or signs) together one after another, the burden of proof is not huge; but that is not what is at stake in claims for syntactic ability in nonhuman animals.

We must conclude that the parts of language that form discrete combinatory systems, including phonology and syntax, seem not to be accessible to the primates that have been the objects of investigation. I have ignored another combinatory system in natural language here, that of *morphology* or word formation. Words are commonly formed from other words according to patterns of modification that can be cumulated to produce very complex structures internal to a single word. We saw in Chapter 9 that ASL has a rather complex morphological system, and it would certainly be relevant to know whether such systematic relations among classes of words could be appreciated by a nonhuman subject. In the absence of phonology and syntax, it seems highly unlikely.

What about a lexicon? What evidence is there that apes can use a set of arbitrary signs in the kind of way speakers of human languages do, to refer to concepts, objects, and relations in the world? To establish this thesis, we need to show symbol use that meets at least the following conditions:

- *Noninstrumentality:* The symbols are genuinely used to *refer* to something, not simply as a means for carrying out some action or getting something.
- *Displacement:* The symbols can be used to refer to things that are not necessarily present in the environment when used.
- *Noniconicity:* The symbols are not direct representations of what they represent in the world.

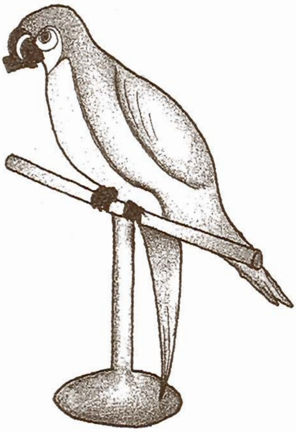
The last two are perhaps obvious requirements for treating gestures or lexigrams as "words." To see the importance of noninstrumentality, imagine what happens when I go to the vending machine in the basement, insert money, and press the buttons A-0-9 in sequence to receive a package of M&Ms. This is one possible interpretation of the situation in which a chimpanzee presses a prescribed sequence of lexigrams on a keyboard and receives a reward. Both of us press a sequence of buttons, in my case labeled A and then 0 and 9, for the chimpanzee having abstract symbols. The chimpanzee has learned the sequence from many trials, gradually built up from a single symbol, while I have the advantage of being able to read A-0-9 on the slot with the M&Ms. Can I interpret my gestures "Insert money," "Press A," and so on, or the corresponding button presses of the ape, as "utterances" like "Please machine give me M&Ms!"?

In both cases, interpretation of the sequence of buttons pressed as essentially equivalent to an English sentence ("Please machine give me M&M's!") is wishful thinking at best. What is going on need in no way involve the essential properties of a language. It is just a routine we go through to get M&Ms (which both the chimpanzees and I like, and are willing to go to some lengths to obtain). To the extent that an ape's utterances all have this character—and by and large, those of the signing chimpanzees do—they do not represent what we do with language.

Most of the apes' utterances are instrumental: ways to get food or treats, including being taken places or other enjoyable experiences. Even Kanzi rarely seems to comment on the passing scene or to ask questions out of curiosity. In virtually all instances, his utterances are intended to get something. The major exception seems to lie in the reports by Savage-Rumbaugh or the Gardners of times when an animal sits quietly by himself paging through picture books or magazines, and sometimes makes signs or presses keys that correspond to what he sees. To the extent that this behavior can be seriously documented, it constitutes genuinely noninstrumental use of signing.

Perhaps, indeed, the fact that most of the signing observed in language-trained apes is unambiguously directed at obtaining rewards says more about the nature of the relationship between the animals and the humans who study them than it does about cognitive or language abilities. From the animals' point of view, the humans may be around mostly to provide food and fun, and the reason the apes learn to make these gestures is to ensure their supply of these benefits. They may well be able to use their signs in other ways (and there is limited evidence available to suggest that that is the case), but most of what human experimenters see illustrates only instrumental uses.

As for noniconicity, it is not seriously in doubt. Kanzi's (or Sherman and Austin's) lexigrams, for example, are wholly noniconic. If we accept that the apes have a sense that the lexigram is a sign for something, it is obviously noniconic. And in the sign experiments, while many of the gestures the animals use represent their referent directly (pointing gestures, touching parts of the body that are to be attended to), and still others are naturally



occurring (probably innate), many others are likely to be learned arbitrary associations. The learned part is presumably important: our vocabulary has the open-ended quality it does because we can learn new words and are not limited to a fixed, innate set. Some of the chimpanzees' signs are apparently ones that occur in nature and those are presumably innate. If those were *all* the animal had, they would not constitute much of a vocabulary—but they are not.

On balance, there does seem to be considerable evidence that the animals in these experiments have learned a set of arbitrary symbolic expressions, even if their primary use for them is to get what they want. It is still a rather remarkable ability, apparently not displayed in nature. I shall return to this point in the closing chapter of this book.

Alex the Parrot

"Stubbins is anxious to learn animal language," said the Doctor. "I was just telling him about you and the lessons you gave me when Jip ran up and told us you had arrived."

"Well," said the parrot, turning to me, "I may have started the Doctor learning but I never could have done even that if he hadn't first taught me to understand what *I* was saying when I spoke English. You see, many parrots can talk like a person, but very few of them understand what they are saying. They just say it because—well, because they fancy it is smart, or because they know they will get crackers given them."

—*The Voyages of Doctor Dolittle*

One of the more fascinating and (to my mind) significant animal "language" studies deviates markedly from the ape language studies we have focused on in this chapter. Since the late 1970s, Irene Pepperberg has been working with an African grey parrot named Alex. Her research is reported in detail in her book *The Alex Studies*.

The activity of most "talking" parrots, mynah birds, and others is relatively uninteresting from the point of view of language. These birds can learn to produce some noises that humans hear as sentences, but whatever meaning these productions may have for the bird has nothing to do with what the sentences mean to us. Indeed, the acoustics of this bird "speech" differs interestingly from normal speech, though there are also similarities. Given the differences in human and avian anatomy, the mechanisms of production are significantly different as well, although unlike most other animals, a parrot does manipulate the shape of its vocal tract in forming different sounds. Arguably, despite the variations of these acoustic signals from actual speech, they nonetheless have the acoustic characteristics necessary to engage the special speech mode of auditory perception discussed in Chapter 5, and thus to be interpreted by humans as speech.

Alex has apparently learned a substantial vocabulary of color words, numbers, names for objects, shapes, and the like. More to the point, he can deploy these words so as to answer questions, ask for objects, and say what he wants. He has probably not acquired anything much in the way of syntax (Pepperberg explicitly avoids the claim that Alex "has language"), but the obvious potential problems with this research (such as the possibility of a Clever Hans effect) have been ruled out. Alex seems to be the genuine article, suggesting that in an animal capable of producing speech-like sound with some fluency, a surprising amount of language-like behavior can be elicited.

Recall that the ape sign language projects were originally started on the basis of the premise that apes had enough cognitive capacity to learn language, but could not deal with the articulation of speech. The opposite would seems to be true for a parrot. These birds produce sound in somewhat different ways from humans, but they can imitate a wide range of sounds in a readily recognizable way.

Pepperberg was working on her doctorate in chemical physics at Harvard University in the 1970s when she heard (via a Nova program) about the signing ape projects, and decided that they sounded like more fun than what she was doing. She took courses in avian biology and related subjects, and after getting her degree and moving to Purdue University, she bought Alex in a Chicago pet shop. The project started at Purdue, moved to Northwestern University in 1984, and then to the University of Arizona in 1991. In 1999 she and Alex moved to the Media Lab at the Massachusetts Institute of Technology, where in addition to language, they worked on a Web browser for parrots. As of this writing their research is continuing at Brandeis University.

A major aspect of this project is the training model Pepperberg originated. Building on earlier work by the German ethologist Dietmar Todt, she developed a competitive ("model-rival," or "M/R") technique of interaction, which has proved to be her key to success in this endeavor. On this approach, the researcher and an assistant interact with each other in the parrot's presence, an activity that seems to be highly motivating. The parrot wants to play too, and wants to learn how to get the objects the humans have, as well as generally seeking their attention and approval. Through this training regime, Alex has learned the names for a number of objects, which he produces appropriately. Considerably more interestingly from a cognitive point of view, he has learned names for a number of colors, shapes (expressed in terms of number of corners: "four [corner]" for "square"), materials, the numbers through six, "none, no" and much more.

What can Alex do? He can label objects ("key," "nut," and so on). When he does this correctly, he usually gets the object named, which he may eat or simply chew on (parrots are fond of chewing or gnawing on things). He can ask for what he wants, when it is not present ("want nut"). He can identify the shape (2, 3, 4, 5, 6-corner), material ("wood," "paper," "cork"), and color of an object. Presented with an array of things on a tray, he can give the number of objects in the set. More dramatically, he can give the number of objects that meet some criteria ("How many four-corner wood?") out of a larger set. When appropriate, he can identify the answer as none ("No"). He can classify colors, shapes, materials, and quantities (numbers) together. Perhaps his ultimate tour de force is the following: presented with a diverse collection, he can identify the dimension with respect to which the objects are similar or different ("color," "matter," and the like).

How should we characterize the communication system Alex has acquired? He has an inventory of individually meaningful words, rather than a set of holistically interpreted utterances. He often makes errors that consist in leaving out a word ("four" is a common error for "four corner" in

"What shape?" questions), which suggests that the words have a sense by themselves and not just in a specific context. He clearly has a system in which these words are combined to form larger wholes. We have no reason to believe in anything like internal constituent structure, but his internal grammar must have (at least) the properties of a word chain as far as receptive capacity for syntax is concerned. This trait is all the more meaningful in light of the absence of evidence for anything so complex in the behavior of most other animals.

What should we say about the nature of Alex's "words"? They are certainly noniconic (as opposed to many of the gestures seen in the signing chimpanzees), since the acoustic products of his (and the experimenters') vocalizations have no intrinsic connections with what they refer to. *Do* they "refer" to something? Evidence in favor of that interpretation is that when he asks for a nut and the experimenters give him something else, he can say "No. Want nut."

Are Alex's utterances instrumental, in the sense that he produces them as a way to obtain a reward? Largely so. Pepperberg stresses that when his answer to a question is correct, he gets what he named: that is, his rewards are intrinsic, not extrinsic. When the object named is one that does not really interest him and he answers correctly, his reward is the right to ask for something else. This procedure makes it a bit more circuitous to interpret his utterances as directly instrumental, in the sense of producing a direct reward. And Alex does vocalize when he is alone, even engaging in what seems to be verbal play with the sound patterns he uses in interaction with the experimenters.

The most interesting results to date as far as cognition is concerned involve Alex's ability to establish higher-level categories such as shape, color, and number. Work currently under way is attempting to teach him to use visually presented arbitrary symbols (such as Arabic numerals for numbers) for the categories he already knows verbally. Essentially, his trainers are trying to teach him to read. Other parrots are now involved in the same training, and Alex is serving as one of the tutors.

Pepperberg has no illusions that Alex is learning English. Rather, she is interested in exploring the possibilities of using English words as a code for "interspecies communication" in order to learn about concept formation and other aspects of the mental life of an animal. That is, she is interested in exploring the parrot's cognitive abilities, and in that endeavor, (some aspects of) language can serve as a tool, rather than necessarily as the object of inquiry.

This seems to me the best kind of language-related research to pursue with animals. There is no reason to believe that human language per se is accessible to other animals. It is always possible that we will learn differently at some point, and novel training methods could show the way toward some such result, but basically animals do not learn language in anything like the sense we do. On the other hand, we *can* use their communicative abilities to ascertain more about animal cognition.

Alex is truly a remarkable bird. Yet when we compare the abilities he has shown with those that have been demonstrated in language-trained chimpanzees, the contrast is at least superficially dramatic. It is hard to believe that the overall cognitive skills of parrots are more sophisticated than those of chimpanzees, so we can only anticipate that different approaches to our evolutionarily closest kin will eventually lead to much more exciting insights into the primate mind. The same conclusion is supported in a limited way by the finding that Nim's signing was somewhat more spontaneous and interesting in a conversational setting than in the setting of explicit training. It would seem, perhaps, that we need to abandon the approach that sees "learning language" in a human sense as the only worthwhile goal, and use the communicative abilities that animals can acquire as a window into their cognitive processes more generally.