Doctor Dolittle's Delusion

Animals and the Uniqueness of Human Language

Stephen R. Anderson

With illustrations by Amanda Patrick

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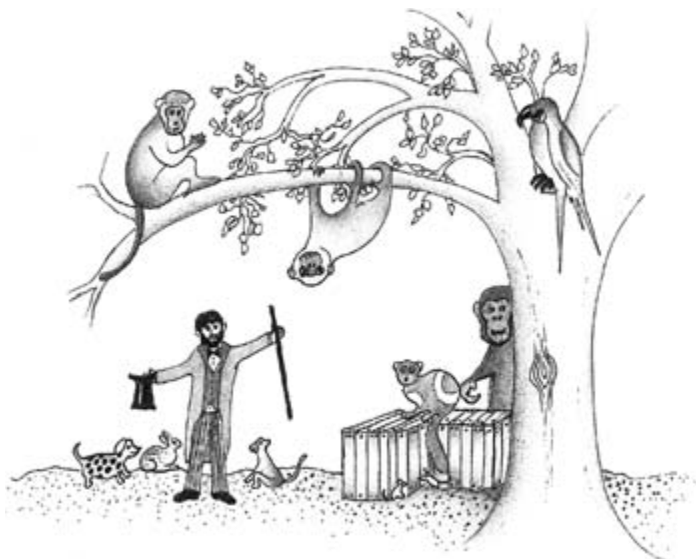


"Why don't some of the animals go and see the other doctors?" I asked.

"Oh Good Gracious!" exclaimed the parrot, tossing her head scornfully. "Why, there aren't any other animal doctors—not real doctors. Oh of course there *are* those vet persons, to be sure. But bless you, they're no good. You see, they don't understand the animals' language; so how can you expect them to be of any use? Imagine yourself, or your father, going to see a doctor who could not understand a word you say—nor even tell you in your own language what you must do to get well! Poof!—those vets! They're that stupid, you've no idea!"

—*The Voyages of Doctor Dolittle*

Hugh Lofting's fictional Doctor Dolittle certainly was kindly and well-meaning—indeed a great man, and one who accomplished much for the animals he loved. Nonetheless, he must have been suffering from a serious misconception: the delusion of this book's title. Merely believing that all animals have ways of communicating with one another would have been an eminently sensible position for the renowned naturalist to take. Where he (together with his friends in the books—and all too many others, down to the present day) went off the track was in equating these abilities with the human faculty we call *language.* In pointing this out, I certainly do not mean to denigrate the good Doctor and his colleagues, but as I am sure he would have acknowledged, scientific truth cannot be ignored.

For there is indeed a science that can sensibly establish the fact of the matter: *linguistics,* a field whose relation to language and languages is every bit as principled as the relation of, say, geology to rocks, minerals, and mountains. Over the past century or so, a scientific understanding of human natural language has developed. It is specialized and technical in its relation to its subject matter, with methods and results that are not instantly apparent but are nonetheless well supported by a long tradition of inquiry. People sometimes are incredulous to hear linguists suggest that what they are doing is somehow comparable to physics, but a great deal that is known about language has a genuinely scientific character, and can be appreciated only on the basis of an understanding of the relevant science.

Every normal human being raised under normal conditions has fluent control of at least one language. It is tempting to conclude therefore that the organizing principles of language should be evident to anyone who chooses to think about them. But this is a mistake, and one that seriously underestimates the complexity of the matter. Hardly anyone would argue that golfers or baseball players, adept as they are at controlling and predicting the flight of balls, must as a consequence know everything there is to know about the physics of small round objects. The systematic study of language similarly reveals properties that are far from self-evident.

When examined scientifically, human language is quite different in fundamental ways from the communication systems of other animals. Still, there are interesting and sometimes quite detailed similarities and we can learn important things about the one by studying the other. In the end, though, the differences are so important that we must not obscure them. What other animals do is not just their own variant of our human talk, in the way Japanese is a variant of what English is. Pursuit of that analogy makes it impossible to understand the basic nature of human language or to see animal communication systems in their fascinating richness rather than as some pale imitation of English.

Indeed, the central question of this book might be: To what extent is our use of natural language a uniquely human ability? In answering I want to convey some of what the modern science of linguistics teaches us about the basic properties of language. To put the result of that inquiry into some sort of perspective, I take other communication systems seriously as well in presenting what is known about their basic properties. I explore two fascinatingly rich and detailed areas of inquiry: animal communication and cognition on the one hand and human natural language on the other. Although they differ in fundamental respects, we can learn a great deal by comparing them.

For much of human history, use of language has been cited as a characteristic that defines human beings and sets us apart from all other animals. Since the 1970s, though, the purported uniqueness of this capacity has come under attack. It seems fair to say that the current understanding in the popular press is that the conception of language as an ability limited to humans is not only outmoded but even a kind of prejudice that science has shown to be wrong—along with many other supposed differences between humans and nonhumans such as the use of tools and the cultural transmission of knowledge and behavior. Other animals, this opinion holds (specifically various higher apes, such as chimpanzees), can be taught a human language and can use it to communicate. And anyone who says otherwise is a rank species-ist.

Consider a review article that appeared in the *New York Times Book Review* not so very many years ago. Its thrust is that we humans ought to be kinder to our ape cousins, and I have no quarrel with that. But throughout are casual references to the notion that chimpanzees, gorillas, and perhaps other apes "have become fairly fluent . . . in sign language, . . . certainly seem capable of using language to communicate," and so on. The bonobo Kanzi, of whom we will hear more later in this book, "remembers and describes" a spot in the woods. One of the several books covered in this review, the novel *Jennie,* involves a chimp who is taught sign language, and "learns to express herself."

All of this takes for granted that, with proper training, some nonhuman primates (and perhaps other animals as well) can be provided with the gift of language, even if their species has not yet figured it out. The notion is certainly not unique to this reviewer, Douglas Chadwick: a 1996 novel, *The Woman and the Ape* by Peter Høeg (the author of *Smilla's Sense of Snow*), involves an ape who is brought to language school. The wife of the experimenter comes to feel that the ape is being exploited. She takes up with him, and they run off together to have an extremely expressive relationship.

Many readers will recall George Orwell's classic *Animal Farm,* where the animal characters are fully fluent in English: they even manipulate one another by manipulating the language. When I was a child, I read a series of "Freddy the Pig" books (by Walter R. Brooks) that also involve a barnyard full of talking animals. While Orwell's book is allegorical, and I did not take Freddy and his colleagues all that seriously, Chadwick's review and Høeg's novel are not meant to be allegory, childish fantasy, or science fiction. Presented as having a basis in current science, they are intended as novelistic treatments of possible situations. Chadwick certainly thinks, for instance, that the author of *Jennie* "seems thoroughly versed in ape research and in the debates surrounding it, and for readers unfamiliar with the subject, his well-intentioned novel makes a fine introduction."

To the extent that Chadwick's assessment is shared, the ability of suitably trained apes to converse with us in a natural language (at least with proper training) has become a more or less accepted fact. It gets worse: as the article shown in Figure 1.1 makes clear, the vanishing distinction between the abilities of humans and of other primates to use language may even be something for naive Web surfers to worry about . . .

Yet, as Chadwick puts it, a proper appreciation of animals' cognitive capacities in this domain is threatened by a band of unsympathetic characters who are "intent on preserving language and reason for the exclusive use of humans." These are the so-called linguistics experts—folks such as the present author. Intent on defending the exclusivity of our scientific turf, we comprise curmudgeons, romantics, and/or elitists who cling to human uniqueness with respect to language in the face of the apparent facts.

Actually, as David Pesetsky pointed out in his response to Chadwick's review, published in a later issue of the *New York Times Book Review,* linguists would be "delighted and intrigued to discover" language in the relevant sense in other primates—or in cockroaches, for that matter. When we look closely, however (and experimenters have tried *awfully* hard), that is not what we find. It appears to be an empirical result, not merely an anthropocentric prejudice, that human language is *uniquely* human, just as many

So you think you've met the love of your life through your computer? Think again!

Your Internet dream gal may be a CHIMP!

Trained apes are surfing the Web — posing as women!

INTERNET expert George Leriger.

By SANDRA LEE / Weekly World News

DETROIT, Mich. — If you're looking for love on the Internet, you'd better be careful — chimpanzees are surfing the Web and some of them even have the ability to type short words and simple sentences!

"Many gentlemen who are trying to find romantic companionship on the Internet fear that their dream girl is really a dirty old man pulling a mean trick on them," said Internet expert George Leriger.

"But it could be far worse and more embarrassing than that. It may well be a chimp on the other end of your computer connection."

Computer users often spend hours at a time in Internet "chat rooms" where they can "talk" directly to another user by typing in their responses.

The computer-savvy chimps have been trained in special programs in the United States, Canada and Europe. Animal experts have worked with the clever primates to teach them to distinguish words on a computer screen and type in short responses.

"It's much like Koko, the famous gorilla who learned to use sign language to communicate with humans," Leriger said.

"There will be many practical uses for society if chimps can successfully be trained to respond to computers."

Leriger said the chimps can't type in long, complex sentences but they can respond with yes, no and simple phrases. One of the phrases a few of the chimps have mastered is "I Love U."

"It seems impossible that 'yes' and 'no' responses and an occasional show of affection could be enough to make a person on the other end of the computer think they've found a love interest," he said. "But we have looked at transcripts from the chimps' computers and it's clear that has happened quite a few times."

The chimps have full access to computers for up to 14 hours a day during their training.

"The chimps enjoy the chat rooms because there is so much going on in them," Leriger said. "A lot of words flash by on the screen and chimps enjoy that."

Figure **1.1** Vanishing distinctions bring new threats

complex behaviors of other species are uniquely theirs. Doctor Dolittle, despite his good intentions, was laboring under a misapprehension.

Chadwick's review inverts the usual logic of the literature about the behavioral and cognitive abilities of animals. What we more often hear is that "apes (chimpanzees, gorillas, . . .) are a lot like us. Therefore, there is no reason in principle why they could not control a language, just as we do."

Chadwick's argument goes the other way: he suggests that since apes really can express themselves and communicate in a language, they must be a lot like us; therefore we should be more considerate of them. Surely, though, we do not need this argument to arrive at the conclusion that considerate and humane treatment of animals is warranted. It is a good thing we do not, because when we look at the evidence, there *do* seem to be significant differences in the language-using abilities of humans and other apes.

Of course, we do have much in common, and it is meaningful to study and understand these commonalities. Their existence, though, does not mean we have (or could have) *everything* in common. For instance, no one denies that humans and bats share a great deal by virtue of being mammals. But even the most dedicated and brightest of human children could hardly be trained to fly by vigorously moving their arms about, or to use echolocation to catch insects. That we are clever enough to build airplanes and sonar systems to accomplish similar ends in different ways does not alter this fact: there are genetically determined differences between humans and bats that establish the limits and possibilities for each.

It seems likely that the human capacity for learning, speaking, and understanding languages is determined by our innate cognitive and neural organization, and as such is uniquely accessible to organisms that have the same specific organization. This capacity develops in the course of human maturation, in the presence of relevant experience—much as other cognitive systems, such as vision, have been shown to do in more limited ways. In the absence of the appropriate biologically based organization, the experience that gives rise to our knowledge of language cannot have that effect, no matter how carefully structured.

Aha, you say, the bat analogy misrepresents the issue. We can't fly because we don't have wings, and we can't catch bugs for lack of the right sensory organs for echolocation. Since language is a kind of behavior, not a physical organ, the argument from genetics fails. Humans and, say, chimpanzees both have brains, mouths, and ears, and those brains, mouths, and ears are quite analogous in their overall structure. Furthermore, humans do not develop language uniformly, the way bats of a given species all come to catch bugs the same way. Rather, we each learn the particular language that happens to be spoken by the community surrounding us; surely that proves that language could not be innate.

But consider this estimate cited by Steven Pinker: "Half of our 100,000 genes are expressed primarily in the brain, [and certainly] species differ from one another innately, [and] humans differ from one another innately on every quantitative trait, and . . . human cognitive accomplishments are solutions to remarkably difficult engineering problems, [so] I myself don't doubt that much of neural organization is innate. Of course that leaves open the question of what aspects of language in particular are innate." With the recent mapping of the human genome, we now know that the actual number of genes is probably less than half the number Pinker cites. Nonetheless, the estimate of the proportion of genetic material devoted to the brain and nervous system continues to "range from 'a fair chunk' to '40%' to 'most.'"

There are excellent reasons to see much of behavior and cognition as closely related to the genetically determined organization of the organism, and thus at least adequate reasons to speak of a human language "organ," with a structure determined by human genetics. Organisms with this organ acquire and use languages of the human sort, whereas organisms without it do not (and cannot), any more than we can fly or catch mosquitos by echolocation in the absence of the relevant species-specific equipment.

How much of language is determined by our uniquely human genetics? To address the question, I need to clarify what we mean by *language*. This goal, in turn, requires distinguishing a specific sense of *language* from a much more general sense that is close to the broad notion of *communication*.

We commonly talk about all sorts of things as language—the language of dreams or of films, body language, even the language of traffic lights. Common to all of these is that they involve communication: one individual (or the film, or the traffic light) emits some kind of signal from which other individuals can derive information. Surely it is not *that* sense of language which is at stake. Everyone grants that organisms a lot less complex than chimpanzees communicate. We would not want to say, though—because organisms of all sorts can determine information from olfactory, visual, or other signs about when an individual of the opposite sex is interested in mating—that no fundamental distinctions can be made, and that language is really universal. The issue is not whether communication takes place in all these circumstances, but rather *how* that communication takes place, and what sort of system it is based on. When we make these inquiries about human communication, a rather special and much more specific sense of "language" emerges.

What I am talking about, more specifically, is the use of systems such as English, French, Japanese, or Potawatomi. Just what *is* a natural language? The definition is at bottom what linguistics is all about, and any snappy, aphoristic definition is virtually bound to fail. In general, every science starts from a presystematic notion of its subject matter, and its results serve to provide a more systematic reconstruction of the properties of the object of inquiry: rocks, molecules, organisms, political systems and economies — or languages. If we could sum up the significant aspects of any of these items in a few sentences, the scientists who study them could leave for the beach, their labors complete.

Short of a completed science, though, treating natural language the way the U.S. Supreme Court has sometimes treated pornography ("I know it when I see it") moves us quite a distance. We know that English, French, and others are natural languages in ways that traffic lights or cinematic symbolism or Fortran, for example, are not. We may not always know what *a* language is (witness the Ebonics discussion of the late 1990s), or when one language is the same as another (consider the sense of "Serbian" as opposed to "Croatian" or "Bosnian," three largely similar forms of what used to be called "Serbo-Croatian," before the breakup of the former Yugoslavia in 1991–92). Nonetheless, we know there is a difference between *language* and other forms of communication.

For generations, philosophers have agreed that the remarkable feature that gives human language its power and its centrality in our life is the capacity to articulate a range of novel expressions, thoughts, and ideas, bounded only by our imagination. Using our native language, we can produce and understand sentences we have never encountered before, in ways that are appropriate to entirely novel circumstances. We will see in Chapter 8 that human languages have the property of including such a discrete infinity of distinct sentences because they are *hierarchical* and *recursive.* That is, the words of a sentence are not just strung out one after another, but are organized into phrases, which themselves can be constituents of larger phrases of the same type or other types, and so on without any boundary.

It is this structural property that gives language its expressive power, so it is reasonable to ask of any candidate for comparable status that it display recursiveness as well. We will see that there is much more to the characteristic syntactic structure of human languages than just recursion, but this is incontestably a core property, sine qua non.

The central issue of this book comes down to a pair of related questions. To what extent do animal communication systems share essential properties with those of human language? (For the reasons just described, pay particular attention to the question of whether these systems display the characteristic properties of unboundedness, hierarchical organization, and recursion.) And if there do indeed remain significant areas of nonoverlap, can any animals other than humans be *taught* to use a communication system with the essential properties of a human natural language?

These questions define my agenda here: to arrive at an understanding of the way animals communicate in nature, to show how the properties of animal communication systems relate to those of human natural languages, and to determine whether the differences we find can be bridged by training. In the process I survey a number of different animal systems, and also provide enough of an introduction to the characteristics linguists have found in human languages to make the comparisons scientifically meaningful.

[Skipping summaries of C

Primates are the focus of Chapter 7, where I consider some of our knowledge about the communicative behavior of prosimians, monkeys, and apes in nature. This discussion centers on the set of alarm calls that a variety of primates produce in the presence of predators. These raise important questions about the extent to which we should ascribe meaning to animal signals in the sense that words of a human language refer to objects in the world external to the speaker. Besides alarm calls, primates produce a variety of other vocalizations that have communicative importance. We can learn from these calls, but the range of their external expressions turns out to be rather restricted. If writers pessimistic about the mental life of nonhumans are to be believed, the animals might just have very little to say—but the evidence for sophisticated thought processes is hardly negligible.

What does account for the massive differences in expressive capacity between human languages and the communicative systems of other animals? As already suggested, the answer turns out to be a central (if often misunderstood) property of language: the system of syntax, with its hierarchical and recursive structure. For those whose only systematic exposure to grammatical analysis came in high school English classes, syntax may seem only a perverse, prescriptive fixation. That is not at all the case. In Chapter 8 I sketch a few of the remarkable syntactic properties of human language, and some of the reasons to believe that this organization is a genetically determined capacity specific to our species.

In Chapter 9 I build a foundation for addressing another of the questions posed above, concerning efforts to teach our languages to other species. To this end, a consideration of the properties of manual (or signed) languages is in order. These have been the basis of the best-known and most ambitious experiments of this sort to date. Contrary to popular opinion (including that of the cat's-meat man quoted at the start of Chapter 9), science has shown that manual languages such as American Sign Language, or ASL, have all the essential structural characteristics of natural languages such as English or Arabic, even though they involve gestures other than those of speech.

If an ape really could learn to use ASL, that would count as learning a natural language. It was in that direction that researchers concentrated their efforts in the 1960s and 1970s. I survey a number of those projects (Washoe, Nim, Koko, Chantek) in Chapter 10, along with other studies that abandoned all similarity to the actual modality of human natural language (speech or sign) in favor of purely arbitrary symbols played out on a keyboard or plastic tokens arranged on a board. For a variety of reasons, all fall far short of demonstrating language abilities in other species.

The most interesting—and also the most scientific—work of this sort that has been done involves apes of a different species, bonobos (often misleadingly called pygmy chimpanzees), and particularly the justly celebrated Kanzi. These animals appear to come somewhat closer than other apes to what we might call genuine linguistic ability. Kanzi's interpretation of certain spoken English sentences is particularly seductive. The ultimate conclusion nonetheless seems to be that when we look at the parts of the system apes can learn to control, the crucial distinguishing properties of language (especially recursive syntax) are still missing.

It is worth stressing once more that this negative conclusion is not the reflection of some presumed species-centrism on the part of linguistic science. If we were to find that other species (say, bonobos) could truly learn the significant parts of a human language, the result would fascinate linguists, not repel them. On the available evidence, though, no such claim seems warranted.

Short of actually learning a language, some of the animals in these studies have demonstrated abilities involving the use of arbitrary symbols for rather abstract concepts. Were we to think of language exclusively in terms of symbolic communication, that would suffice. The actual richness of the expressive capacity of human language, though, depends on further elaborations of exactly the sort that animals do not achieve. Exploring the abilities they display in these studies (but not, apparently, in nature) is certainly relevant; but that is a separate issue from whether or not they have the capacity to learn and use a language in the specific sense that refers to human languages.

By using the expression "human language" repeatedly, I do not of course mean to exclude a priori anything a nonhuman might do. The properties of language that I discuss in the chapters to come are abstract enough to be dissociable from the activities of human vocal tracts and ears, hands, and eyes. They would be directly identifiable in the behavior of other animals if they were indeed found there. Nothing about language in the sense intended here is intrinsically limited to systems with our specific physical organization—though as a matter of empirical fact, the capacity for language *does* seem to be limited to organisms with our specific neurological and cognitive organization.

2

Language and Communication

At tea-time, when the dog, Jip, came in, the parrot said to the Doctor, "See, HE's talking to you."

"Looks to me as though he were scratching his ear," said the Doctor.

"But animals don't always speak with their mouths," said the parrot in a high voice, raising her eyebrows. "They talk with their ears, with their feet, with their tails—with everything. Sometimes they don't WANT to make a noise. Do you see now the way he's twitching up one side of his nose?"

"What's that mean?" asked the Doctor.

"That means, 'Can't you see that it has stopped raining?'" Polyne-

> sia answered. "He is asking you a question. Dogs nearly always use their noses for asking questions."
>
> —*The Story of Doctor Dolittle*

Communication is virtually universal among living things. Even bacteria communicate. Some classes of bacteria secrete distinctive organic molecules, for which they have specialized receptors. This apparatus allows the bacteria to detect the presence of others of the same species, a system known in the literature as quorum sensing. "Bacteria, it turns out, are like bullies who will not fight unless they are backed up by their gang. An attack by a small number of bacteria would only alert the host's immune system to knock them out. So bacteria try to stay under the radar until their numbers are enough to fight the immune system." The molecules secreted by one bacterium serve to communicate its presence to the others. Yet surely not all communication is of a piece with all other communication: the use of the word *talk* in the title of the *New York Times* story about quorum sensing is simply the journalist's effort to be clever.

To determine the true issue here, consider an example. One evening I returned home to find my wife correcting papers for her French class. When I asked her what we were doing for dinner, she said, "I want to go out." That is, she produced a certain sequence of sounds, and as a result I knew that she wanted us to get in the car and drive to a restaurant, where we would have dinner.

When I came home the following night, I found my cat in the kitchen. She looked at me, walked over to an oriental rug in the next room, and began to sharpen her claws on it. She knows I hate that . . . and as I came after her, she ran to the sliding glass door that leads outside. I yelled at her, but my wife said, "Don't get mad; she's just saying, 'I want to go out.'"

We conclude that both my wife and my cat can say "I want to go out." Do we want to assert that they both have language? Surely that is at best an oversimplification, although it is clear that both can communicate. Each can behave in such a way as to convey (somewhat similar) information to me.

Here is a sketch of how "real" communication takes place: One organism has a message in mind that he or she wants to communicate to another organism. He or she emits some behavior (makes a noise, scratches the carpet) that encodes that message. The other organism (me, for example) per-

ceives the behavior, identifies it in terms of the meaning encoded, and treats the result of that decoding as the meaning of the message.

Sometimes called the Message Model of Communication, this description may seem fairly obvious, but is it a valid general definition of communication? Communication can take place even when there is no evident basis for saying the communicator "intends" to communicate anything. Think of our bacteria above, or a blush, or the visible signs in many species when a female is in estrus and receptive to mating: there is no intention on the part of the signaler, but a message is communicated all the same.

On the other side, it may be that the recipient interprets the message only in part on the basis of its literal content and relies also on various non-overt contextual or social factors. Consider "Can you pass me the salt?" Here the literal content is an inquiry about the listener's physical capacity to perform an action, but the message usually conveyed is a request that the salt indeed be passed. Or perhaps I ask my colleague what she thinks of the candidate we have just interviewed for a job, and she says "He seems very diligent." In an academic context, this implies a very negative recommendation. If a candidate's best quality is diligence, it is *not* creativity, imagination, or inspirational teaching. In both examples, clearly the linguistic content of what we say may be quite different from what we communicate.

The little story about my wife and my cat illustrates the characteristics of any communication system. First, what is the nature of the behavior or other signal? The cat scratches the carpet and runs to the door to convey a message we might interpret as similar to one my wife conveys by moving her vocal organs to produce sound. Second, what is the range of messages the system can convey? Evidently, my cat can say fewer things than my wife: what is the basis of this difference in expressivity? Third, what relation, if any, is there between the message expressed and the communicator's intentions? The cat certainly intends *something,* but her behavior actually reflects her internal state; my wife can say what she does even if she doesn't really want to go out. Finally, what is involved on the receiving end? Obviously, you have to know the code in order to get the message, but what else? My wife and I understand the cat's scratching behavior as attention seeking in the context of my evident and constant displeasure at it, but is there some kind of underlying code that all three of us share?

Another important aspect of communication systems (not significant in this case) is how the communication system came into being. Did it evolve gradually out of something else, or did it spring into operation fully formed? My cat scratches the carpet basically to sharpen her claws; whatever additional meaning may accrue to that action has grown up ad hoc between us. Most systematic means of communication have more interesting and far longer histories.

This is an area of inquiry where the questions that can be raised are potentially more interesting than the answers currently available. Historical evidence for the sounds of language is minimal; even the soft tissue of tongues, ears, and brains leaves no trace in the fossil record.

The original nineteenth-century constitution of the Société linguistique de Paris is famous for explicitly prohibiting the discussion of matters concerning the origin of language at the society's meetings. This was no mere quirk of the founders: they introduced this limitation for precisely the reason that there could apparently *be* no real science that bore on the topic. Since the late 1990s, interest among linguists and others has reawakened, and conferences are now regularly devoted to the subject. To my mind, this revival is not based on additional data, but rather on the mistaken impression that if we can *pose* an important question, we ought in principle to be able to find an answer. Fortunately, we need not resolve this vexatious problem before studying communication systems and communicative abilities *comparatively* across animal species. We will return to these matters in Chapter 11.

Notions of Language and Communication

How might we distinguish between "language" and "communication"? One way of approaching the distinction is to note that communication is something we *do,* whereas language is a *tool* we can use. We can, of course, communicate without language, though the range of material we can transmit is limited in significant ways. Most of the amusement value of the game of charades, for instance, lies in trying to circumvent these limitations. In fact, a desirable skill in this game consists in referring to words without actually using them (using gestures interpreted as "short word," "sounds like," and so on).

For comparison, the activity of building houses is also something we do, and we use particular tools to do it. Without hammers, nails, saws, and levels, we could not practice the construction trade as we know it. Yet that does not mean we could not construct shelters. We can do a certain amount of building without tools, or using different tools, as other societies do. Still, the structure of the tools makes certain sorts of construction easy and natural. We can study the structure of the hammers and saws and ask where they come from. We see, of course, that there is a close connection between the structure of the tools and what we can do with them, but we should not confuse the activity of carpentry or construction with the tools we use in pursuing it.

Suppose we want to open a nut. We do it by exerting force on the shell through a hard object—either with leverage, using a nutcracker, or by hitting it, for instance with a hammer. Chimpanzees in the wild open nuts by putting them on one rock, then hitting them with another rock—a technique similar to one used by humans. The tools are not identical, but they have the same structure in the relevant respects. There is an activity, and similar means are used in carrying it out. As far as communication is concerned, we do a lot with facial expressions, grunts, and the like. Again, considerable similarity among human and nonhuman primates exists in the activity and in the means for executing it.

Orangutans in nature do not use tools equivalent to those of human carpentry. But if we give an orangutan a claw hammer, and he knows that something good to eat is inside a wooden box that is nailed shut, he can use the claw hammer to remove the nails and open the box, much as a human would. Provide him with the tool, and his cognitive abilities are certainly adequate for using it in some of the ways humans do—ways that depend on the essential structure of the tool.

I imagine that chimpanzees can learn fairly quickly to open nuts with a nutcracker by utilizing the structure of the tool, which is novel to them but suited in form to the task. Yet if we give a chimpanzee a small tape recorder, I seriously doubt that the ape could use it to record grunts and send them to be replayed for another chimpanzee in order to communicate a message. The principal use of a tape recorder might be to serve as the base on which to put a nut in order to smash it.

These distinctions are important when asking whether another species (say, monkeys or apes) can use language. Provided with the proper tools, an ape can use them to engage in at least some "carpentry." What about language and communication? When we ask whether animals other than humans can engage in communication, the answer is, obviously. What is the structure of the means they use to that end, and how closely does their communicative activity resemble human natural language? If we supplied an ape with a human natural language, how much communication could he or she achieve? We need to know a certain amount about the structure of human natural language if we are to make these questions precise; the more we know, the more precise we can be.

Semanticity

Linguistic forms have denotations: That is, they are associated with features of the world, as opposed to many nonlinguistic signals that refer only to themselves (think once more of the stickleback's belly). I deal further with this issue in Chapter 7, in connection with the meaning of alarm calling behavior. In understanding the workings of language, we want to distinguish semantic signals (which refer to events and objects in the world outside of the signaler) from expressive ones (which simply reflect to the outside world some aspect of the internal state of the subject).

Arbitrariness

It is conventional to observe that the linguistic signal has no necessary relation to what it denotes. Speech signals, that is, are not in general iconic. *Cat* refers in my speech to instances of *Felis domesticus* not because of some perceived resemblance between the sound of the word and some aspect of a real cat, but merely because that is the English word for it. The arbitrariness is reinforced when we observe that other languages have quite different words for the same thing. In Navajo, for example, a cat is a *mósí,* but the cats themselves are just the same.

Arbitrariness is often thought to be falsified in the case of onomatopoeia: thus, a cat says "meow" because . . . well, because that is the noise a cat makes. In fact, though, different languages have at least partially conventionalized onomatopoeic words for animal noises. Cats say "ngeong" in Indonesian, for example. A rooster says "cock-a-doodle-doo" in English, but "cocorico" in French or "kikiriki" in German. A turkey says "gobble, gobble" in English but "glu, glu" in Turkish. A pig says "oink" in English but "groin groin" in French, "röh röh" in Finnish, "chrum chrum" in Polish, "nöff" in Swedish, or "soch, soch" in Welsh. Although these words generally are inspired by sounds made by the animals in question, they are nonetheless words of particular languages, and with very few exceptions they conform to the principles of words in those languages. A pig could not say "groin groin" or "röh röh" in English, because English does not have the nasal vowel [ɛ̃] of the French word or the front rounded [ö] of Finnish. English cats could not mimic their Indonesian counterparts because English words cannot begin with [ŋ] (*ng*), and so on.

As opposed to the words of spoken languages, paralinguistic vocal features are less arbitrary, in that their dimensions tend to be related iconically to those of the internal states they express. Thus, when we are angry, our voice may get loud. When we are angrier, it gets LOUDER—and when we are extremely angry, EXTREMELY LOUD. The dimension of loudness can vary in a continuous way, showing (in principle, at least) as many degrees as does our potential anger or other internal state to which the loudness corresponds. This continuous and iconic character is one of the basic ways in which paralanguage differs from language.

Even apparently transparent iconic communication may have some arbitrariness, though, in the sense that it may have to be acquired in order to be understood. Thus, we take the gesture of pointing for granted as a way to call another's attention to something, but not all cultures use similar gestures in this way.

A story (probably apocryphal) that I heard in an undergraduate class illustrates this point. A missionary is dropped into the jungle and tries to learn the language of the surrounding community. Eventually she learns how to express "What's that?" and sets out to expand her vocabulary. She points to a house and asks "What's that?" and hears "Boogoo-boogoo," so she writes in her notebook: *house:* [bugubugu]. Then she points to a tree and asks "What's that?" and again hears "Boogoo-boogoo." She decides she must have been wrong the first time, and that [bugubugu] means *wood,* not *house.* But she points to a passing dog and asks "What's that?" to which the response is, once again, "Boogoo-boogoo." Eventually she learns that [bugubugu] actually means *right index finger.* In the local culture pointing is done with the chin, and every time she asked "What's that?" her position had been such that her chin was directed toward the pointing finger.

Although various nonhuman primates assuredly have a sense of drawing attention to an object, most do not understand finger-pointing gestures as the way to do so, at least not without extensive training.

Displacement

With language, we can refer to objects and events that are distant in space and time from the location of the speaker or the hearer. Other signaling systems do not in general have this property. To the extent that it makes sense to describe animals' signals as "referring" to something, it is always to the here and now—the attitude or the internal state of the animal doing the signaling.

Even rather rich systems devised and used by humans share this limitation, to the degree that they are not basically parasitic on language itself. A baseball coach may have a signal for *hit and run,* but there is none for *if we're still ahead in the seventh inning, I'll be able to take you out for a pinch hitter.* Bee dances perhaps are an exception, if we think of the bee as "describing" the properties of a distant food source to her fellow workers. Still, it may also make sense to think of this system as reflecting the bee's own internal state, a state that results (here and now) from the foraging flight she has just undertaken. If we think of the hive as both the location of the dance and the origin of the flight vector it indicates, the putative spatial displacement is less evident. In any event, there is no question of a temporally displaced referent: Bees' dances relate to food sources available within a very short temporal horizon from the present, not to where they found a nice patch of hollyhocks last season.

Openness and Productivity

An open or productive system is one that is capable of expressing an unbounded range of possible meanings. Most animal communication systems serve to convey at most a few dozen different possible messages. Once again, bee dances are a possible exception. Since the parameters of the dance can potentially distinguish a continuous range of possible food source locations, it follows that the number of distinct "messages" is unlimited, at least in principle.

If we ignore the point that the bees themselves may not be infinitely precise in producing and interpreting these dances, there can thus be an infinite number of dances. This kind of productivity is completely different from what we find in human language. Even on the most charitable interpretation, the bees are always "talking about" the same thing (however many subtly distinct variants there may be), whereas the variety of things humans can refer to when talking is not limited in that way. This difference in the productivity of communication systems requires us to distinguish *continuous* openness—as illustrated by bee dances—and *discrete* openness of the sort we find in natural language.

Prevarication

Some theorists of language origins, fond of paradoxes, have said that language "must have been invented for the purpose of lying." Charitably, we can interpret this statement as emphasizing that language can describe things that are not literally realized or true. We can talk about unicorns and squared circles, even if we cannot ever point them out. We can also use language to lie more literally, referring to states of affairs that are contrary to what we know about the world—doing so not just to exercise our theoretical imaginations, but to actively mislead our listener. Yet insofar as a communicative signal is simply an external manifestation of an animal's internal state, it is not really possible for the animal to "lie."

Some reports suggest that animals other than humans use supposedly communicative behavior to deceive. One celebrated case is that of the (mother) piping plover (*Charadrius melodus*), who apparently pretends to be hurt to distract predators from the nesting site where her relatively helpless offspring would be endangered. But is this undeniably effective strategy really an instance of intentional misleading? The plover's behavior is not just a reflex: the bird clearly tries to lead the intruder away from the nest. I discuss the interpretation of this case in Chapter 3; to anticipate the conclusion, there is no reason to believe that the bird is lying so much as engaging in behavior that she knows will attract a predator away from her nest.

Vervet monkeys sometimes behave in ways that could be seen as an attempt to deceive their fellows about the presence of predators, and Dorothy Cheney and Robert Seyfarth have explored ways to disentangle intended deception from other interpretations. There is also a substantial (and controversial) literature on apparently deceptive behavior in primates, under the heading "Machiavellian intelligence." A substantial corpus of incidents has accumulated, but the evidence remains at the level of intriguing anecdotes rather than systematic patterns of behavior.

To say that some communication is genuinely deceptive, we would want to establish that the communicator has some sense that the recipient of the message has a view of the world, and that the communicator is attempting to manipulate that view (rather than directly manipulating the behavior itself). It is a thorny issue, and one that has been much discussed. Do any animals other than humans have a *theory of mind,* in the sense that they see other individuals not merely as acting but as as holding opinions that underlie their actions? There seems to be no valid evidence for this claim in any species, and some evidence in higher primates that argues against it. For current purposes it suffices to mention that this philosophical question is relevant to the notion of deceptive communication.

The fact that we can use language to talk about things that are not true, or not possible, or simply imaginary, is qualitatively quite different from this point. We can also use language to lie and deceive, but it is difficult to see that as its principal role in our lives.

3

On Studying Cognition

Now this Nino was just an ordinary, cream-colored cob who had been trained to answer signals. Blossom had bought him from a Frenchman; and with him he had bought the secret of so-called talking. In his act he didn't talk at all, really. All he did was to stamp his hoof or wag his head a certain number of times to give answers to the questions Blossom asked him in the ring . . . Of course, he didn't know what was being asked of him at all, as a matter of fact. And the way he knew what answers to give was from the signals that Blossom gave to him secretly. When he wanted Nino to say yes, the ringmaster would scratch his left ear; when he wanted him to say no, he would fold his arms, and so on. The secret of all these signals Blossom kept jealously to himself. But of course, the Doctor knew all about them because Nino had told him how the whole performance was carried on.

. . . "Look here, Mr. Blossom," said [Doctor Dolittle] quietly, . . . "I know a good deal more about animals than you suppose I do. I've given up the best part of my life to studying them. You advertised that Nino had understood you and could answer any questions you put to him. You and I know that's not so, don't we? The trick was done by a system of signals. But it took the public in. Now I'm going to tell you a secret of my own which I don't boast about because nobody would believe me if I did. I can talk to horses in their own language and understand them when they talk back to me."

—*Doctor Dolittle's Circus*

Much of what looks enormously complex to us in the world is in fact based on remarkably simple principles. A standard example in complexity theory is the elaborate constructions termites produce in building their mounds: these prove in the end to be based on nothing more complicated than each termite's putting a new bit of the nest right next to the bit added by the previous termite. Conversely, much that looks simple can be accounted for only in terms of complicated models. The baseball player's apparently effortless glide toward a dropping fly ball involves the (virtual) solution of systems of differential equations based on keeping constant certain angles in the visual field and other visual properties.

Because of this disparity between the actual complexity of the world and our interpretation of it, we must always be skeptical of the extent to which our first impressions correspond to the way nature really works. This cautionary lesson is the point of the present chapter, approached in terms of two apparently disparate topics. On the one hand, we consider some instances of animal behavior that seem "obviously" to reveal elaborate underlying cognitive processes, but that actually lend themselves to much more conservative interpretation. On the other hand, we take a first look at some properties of human natural language that turn out to be far less simple than they appear.

These two matters may seem unrelated, but to understand the issues in comparing animal communication and human language it is essential to appreciate the tension between apparent and real complexity of structure in behavior and cognition. In evaluating the evidence for language-related abilities in nonhuman animals, we must be constantly aware of the temptations both to overestimate what the animals are doing and to underestimate what we ourselves know and do as speakers of our native tongue.

There is another danger. If we measure everything we find in animals by the standard of its approximation to something we might find in humans, we run the risk of underestimating the animals and thereby missing what is interesting, complex, and significant about these creatures' abilities and actions in their own right. We can hardly hope to evade these difficulties altogether, but preliminary study of some examples for their methodological value may be worthwhile.

Giving Animals (Exactly) Their Due

In Chapter 2 we looked at Hockett's classic attempt to arrive at a precise characterization of the difference between human language and other communication systems. His 1960 paper proposed an extensive set of criteria for assessing a communication system, with a specific set of values on these dimensions taken to characterize human natural language. Looking through the other end of the telescope, we could see these tests as a set of criteria such that if, in exploring some particular animal's behavior, we were to find the same set of values as Hockett, then we should attribute humanlike language ability to the animal.

Surely, however, this approach gets things the wrong way around. Even if, in the classic philosopher's illustration, the members of the species *Homo sapiens* are the world's only featherless bipeds, we do not want to equate our humanity with erect posture and lack of plumage. Similarly, the nature of language cannot be reduced to some collection of its external properties, although of course those properties may, if well chosen, tell us much about what we ought to be paying attention to. And if the cognitive organization of nonhuman animals cannot accommodate itself to the kind of system constituted by human language, it will not be because the animals fail to score well enough on a specific suite of tests.

The communicative abilities of animals can be valuable to the cognitive scientist in several ways. To begin with, the behavior and abilities of animals are fascinating in their own right. As a simple illustration, Carolyn Ristau's study of the evasive displays of the piping plover makes it clear that this bird shows a level of apparently intentional behavior whose exact characterization in relation to the categories of human cognition presents intriguing challenges. Cheney and Seyfarth's studies of vervet monkey vocalizations (discussed in more detail in Chapter 7) have led to interesting conclusions about the limits and the possibilities of the animal's cognitive world, the individual's awareness of other animals, and much else.

We can learn about an animal's cognitive capacities not only by studying what it does when communicating with other animals, but also (potentially, at least) by utilizing this same system of communication as a tool to explore cognition directly. For example, if it were possible to teach an animal to respond meaningfully to "same-different" questions, the answers could be used to gather information about just what objects or phenomena in the world seem equivalent to (let us say) a parrot. This scheme may seem far-fetched, but we will see in Chapter 11 that approximately these results have been achieved with an African grey parrot named Alex. More ambitious hopes that animals can tell us the details of their lives and experiences appear for the present to be the stuff of science fiction, not science—or else of serious overinterpretation.

Finally, the very enterprise of asking how human language and animal communication differ does not tell us only about the ways in which the animals fall short of us. Focusing on what it is about humans that fits them to acquire and use languages tells us much about ourselves as well, because languages in the human sense are systems not known to exist in any other organisms.

How do we go about exploring the cognitive capacities of animals, especially their abilities in the domain of communication? Much as we would like to (and despite scientific conferences organized around the possibility), we cannot really talk about these topics with the animals themselves. We have to rely on our interpretations of their behavior under controlled conditions. But in making sense of what we observe about animal behavior, we face a serious problem known as the Clever Hans phenomenon.

The story of Clever Hans (Figure 3.1) has been told many times, perhaps in greatest detail by Oskar Pfungst. The horse's trainer, Wilhelm von Osten, maintained that the reason horses (and other animals) displayed less intellectual capacity than humans was primarily their lack of educational opportunity. He set out to rectify this omission with Hans, training him in a number of basic skills, including arithmetic (whole numbers, fractions, decimals), object identification, spoken and written German, days and dates, and standard German coinage. He then presented exhibitions of Hans's abilities, demonstrations in which the horse responded correctly to questions posed in German by tapping his hoof and shaking his head a number of times corresponding to the correct answer.

Figure **3.1** Clever Hans and Herr von Osten

Audiences of course were skeptical and attempted to demonstrate that Hans was actually receiving signals from von Osten (whose belief in the animal's abilities appears to have been quite sincere). This possibility seemed to be excluded, however, because Hans performed essentially as well when others posed questions to him in the absence of his trainer. In 1904 the director of the Berlin Psychological Institute, Carl Stumpf, established a commission to assess Hans's abilities. The thirteen members were selected so as to ensure that no deception would go undetected. Chosen to cover as many bases as possible, the commission included a circus manager, a teacher and a school administrator, two zoologists, a veterinarian, a physiologist, a psychologist and a politician. In spite of their skepticism, there seemed no sign of signals being passed (intentionally or not) between von Osten and his horse. Although Stumpf later insisted that the commission members were not at all "convinced that the horse had the power of rational thinking," they could say nothing more than that further investigation was warranted.

Oskar Pfungst, unwilling to let the matter rest, continued to test Hans. Eventually he established that the horse's abilities were indeed illusory. For one thing, if his questioner did not himself know the answer to the question posed, Hans performed much less well. He was also unable to answer when he could not see the person who asked the question, or someone else who knew what the question was.

Pfungst published his own report in December 1904. As it turned out, Hans would start tapping on the basis of one set of quite unconscious signals from his questioners and keep going until he got another signal to "stop." That is, he was not adding, making change, or understanding the question in any relevant sense. He was picking up on something totally different: unconscious involuntary movements of the head and body, small changes in facial expression, perhaps changes in his audience's heartbeat—completely unintended events that accompanied the listener's changing internal state as the "answer" to the question unfolded. Pfungst was even able to get Hans to start tapping his hoof by standing in front of him and making appropriate slight movements, without posing any question at all; the tapping stopped when Pfungst straightened his head slightly.

Clever Hans was clearly doing something rather more interesting than Nino (in the epigraph at the start of this chapter). No one had ever conspired with him about a set of signals: he worked them out on his own, without his trainer's even being aware of the external indicators Hans was detecting about his internal state. Certainly Hans did not need to know German, or arithmetic, or any of the other skills he was supposedly exhibiting in order to perform successfully. In the words of a newspaper story of the time, "The horse of Mr. von Osten has been educated by its master in the most round-about way, in accordance with a method suited for the development of human reasoning powers, hence in all good faith, to give correct responses by means of tapping with the foot. But what the horse really learned by this wearisome process was something quite different, something that was more in accord with his natural capacities,—he learned to discover by purely sensory aids which are so near the threshold they are imperceptible for us and even for the teacher, when he is expected to tap with his foot and when he is to come to rest."

The story of Clever Hans points out two morals that must be kept in mind when considering the interpretations we give to the behavior animals exhibit under even the best controlled conditions. Investigators may unwittingly give cues to their subjects, so that the observed behavior is actually determined by something quite different from what the experimenter is trying to explore. This prompting need not be intentional, and it can happen in a variety of ways, even those completely removed from the communicative modality on which the experiment is concentrating.

Furthermore, we cannot be sure that the animal's conception of what is going on in a given situation is anything like ours. Some of the early work done to explore the language abilities of chimpanzees employed artificial systems such as keyboards or plastic tokens as the communicative medium, rather than spoken words (or the manual signs that would later replace them in this research). In these experiments the animals learned to produce "utterances" that we can translate as "please—machine—give—Lana—M&Ms," each such utterance consisting of a specific sequence of symbol manipulations. Has Lana therefore come to control the structures of sentences like the English ones we give as "translations"? No. Lana has merely learned that the signal that elicits M&Ms from the machine has a complex form. There is no reason (at least not without a lot more research) to say that the key or plastic token which *we* translate as "please" has any signification for Lana or the other animals in these experiments such as what the English word means to us: she is merely doing something that gets her M&Ms.

It is possible to train pigeons to peck a sequence of several different levers in a particular order to obtain a reward, a behavior that we would surely be misguided to interpret as the production of structured "sentences." The temptation to impose such interpretations on animal behavior is sometimes irresistible: classic parrot utterances ("Polly want a cracker") seem quite definitive in their interpretation to us, but there is almost never reason to believe that they have any such interpretation for the bird. The case of Alex is quite different: Alex does mean what he says, at least in large part, but he is a special case.

In light of these difficulties, it is generally assumed that observations of an animal's behavior should be interpreted in terms of a very conservative principle known as Morgan's canon: "In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychical scale."

There are obvious problems with this formulation. What is a "faculty"? What makes one faculty "higher" or "lower" than another? Anyway, why should we believe in such a restriction? But by and large, Morgan's canon simply corresponds to a moderately conservative notion of what constitutes a scientifically warranted conclusion.

Suppose you are trying to show that animals have some particular ability, and they emit some behavior that is consistent with their having that ability. If their behavior is also consistent with some other assumption, you have not shown that your preferred interpretation is correct until you have excluded the alternative. Clever Hans's behavior was *consistent* with his having learned German, arithmetic, and so forth; but it was also interpretable in terms of his simply having learned to pick up subtle start-stop signals. The more elaborate interpretation has not been established until the simpler one is ruled out. And since it is fairly easy to show that a horse can indeed learn to start and stop tapping his hoof on the basis of signals, it is more conservative to interpret what Clever Hans was doing in that way. Of course, the more elaborate story is still logically possible, too; but we have not shown it to be correct unless and until we can rule out the simpler one.

We do not, perhaps, need anything as grand as Morgan's canon to decide what to say about this case, but other examples are subtler. I have already mentioned another classic of the experimental and philosophical literature on the cognitive capacities of animals: the behavior of some birds, including the killdeer and the piping plover, who seemingly pretend to be hurt in order to distract predators who might endanger their nestlings. During the nesting season, when such danger approaches, the mother bird attracts the intruder's attention with a "broken-wing" display (Figure 3.2) while moving away from the nest site. Seeing this apparently wounded animal as easy prey, the predator follows the mother until both are safely out of range of the nest, at which point she reveals her true state of fitness by flying off.

Such behavior seems to wear its interpretation on its sleeve: surely the bird is actively feigning injury with the intention of misleading the predator, drawing it away from the helpless chicks. But do the facts really lead us to that conclusion? Think of what is involved in actively deceiving someone else. First you have to imagine how the other individual interprets the world as it is. Then you have to work out that *if* the other were presented with certain (apparent) facts, that interpretation would be altered (in a way that you personally would prefer). And then you have to implement some course of action that will seem to present the relevant "facts" to the other for this purpose. Quite a lot of cognitive computation, after all.

Ristau examined in detail the plausibility of this interpretation of the piping plover's deceptive display. There is little reason to doubt that poten-

Figure **3.2** The piping plover's broken-wing display

tial predators are effectively deceived by the bird's broken-wing display and lured into following the mother away from the vulnerable nestlings. But does this necessarily entail that the plover actually intends to mislead? Although undeniably an effective trick, the behavior in question is nonetheless a somewhat isolated element in the bird's behavioral repertoire. We have no evidence that the plover deceives other animals (or other plovers) about other things in other ways.

The simplest possible interpretation of the bird's actions would be that the broken-wing behavior is no more than a reflexive and completely automatic response to danger. Ristau shows that cannot be correct, however. When the predator does not follow the mother, she comes back toward it, repeats the display, and moves away again, until finally she attracts its attention.

Further, other intruders (animals such as cows that do not eat the eggs but might step on them) do not provoke this behavior—although if they get too close, the bird may fly up in their face to scare them away. The bird monitors the intruders, and if they do not get drawn away, she goes back and tries harder. It seems to be exactly those intruders that present a real danger to her chicks that she is trying to lead away from the nest. Thus, her actions cannot be the result of a simple reflex to twist her wing and move away when others approach the nest.

Apparently the bird has the intention to draw intruders away from her nest. But that does not determine the cognitive mechanisms that underlie the broken-wing display. Indeed, that behavior has variants. Sometimes the bird just makes a lot of noise and flaps her wings while heading away from the nest, attracting the intruder's attention. There seems no reason to interpret the broken-wing behavior as specifically and intentionally deceptive (in the rather elaborate sense laid out above). It is just another way to draw the enemy away from the nest.

The difference in interpretation is subtle, but crucially important in assessing the kind of cognitive processes we want to attribute to the plover. As with Clever Hans, we have not excluded the possibility that the bird has an intention that involves an understanding of the predator's interpretation of the situation—but we have not shown that, either. It is simpler to assume the bird adopts this behavior as an effective diversion from her eggs. Without any understanding of why it works as it does, we must adopt that interpretation until it can be shown that something more elaborate is at work.

What is at stake here, in the jargon of the field, is whether we need to assume that birds have a "theory of mind," by which they attribute interpretations and attitudes to other beings (and sometimes seek to influence their behavior by affecting the content of their minds). We can fairly easily convince ourselves by simple introspection that *we* work that way, but it is notoriously hard to explore this issue in others.

Experimental evidence suggests that an understanding of the minds of others emerges early in human infancy. Autistic children, on one view, differ from normal children primarily in lacking this kind of understanding of others. Daniel Povinelli has argued that chimpanzees fail at tasks in this area that young children easily pass. If he is correct, we should probably not attribute a theory of mind to these primates. Before we can adopt a view of behavior as genuinely deceptive, we need to show that the animal in question has a conception of other animals as having minds whose content can potentially be influenced. Such evidence is not at all easy to come by, and probably is presently lacking for all species other than our own.

Charles Munn describes two species of birds in the Amazon that provide a similar example. In mixed-species flocks the white-winged shriketanager and bluish-slate antshrike serve as "lookouts" while others feed, because they are generally the first to give alarm calls when dangerous hawks approach. The other birds respond to these calls by freezing, looking up, or taking cover. Sometimes, though, when there is competition for the insects all the birds feed on, the sentinel birds give alarm calls when no danger at all is present. The others disperse, leaving the crafty watchmen a clear path to the food.

Should we interpret this activity as the result of an *intention* on the bird's part to deceive his non-conspecific companions? Probably not. The more conservative view is that the bird gives the alarm because doing so (occasionally—it would not do to overuse this strategy) provides unencumbered access to food. Accordingly, there is no reason to believe that the calling bird has any particular sense that the other birds "believe" anything, and thus that it might be possible to "mislead" them.

In Ristau's piping plover study, the initial observations were consistent with the bird's acting in the way it does as a reflex; or with her having a fixed behavioral routine; or with her having an intention to lead the intruder away; or, finally, with her aiming to deceive the intruder. Ristau's subsequent observations showed that the behavior has a flexibility that is inconsistent with reflexes or fixed repertoires, allowing us to conclude that some sort of intention is involved, and that it entails leading the intruder away. There is, however, no evidence to require the conclusion that deception is a part of this intention.

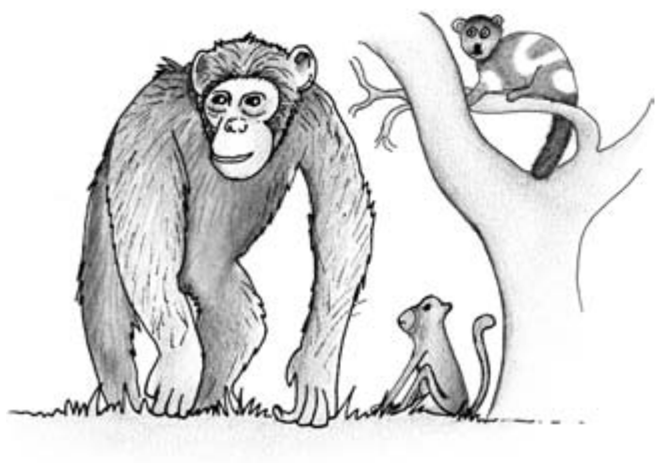
Each step along this path involves an application of Morgan's canon, at least implicitly. Sometimes the result is that yes, some interesting structure is necessarily entailed by the facts surrounding the behavior under study. At other points we must conclude that no data support going further in our interpretation. That does not in itself constitute evidence that some additional structure is *not* present: it is perfectly possible, at least logically, that plovers are really a kind of robot, controlled from inside by devious little green individuals from Mars, who do indeed intend to toy with the minds of the other animals their hosts encounter. But in the present state of our knowledge, we cannot regard *any* interpretation that involved intentional deception as scientifically justified.

As we read the literature interpreting animal communication and explore the attempts that have been made to teach language to animals, we must bear these methodological issues in mind. We want to avoid inflated claims and excessive romanticism; but we also want to give the animals their due, not treat them as mindless simply because they are not human. Questions of the cognitive complexity of other animals are at least in principle a matter for serious scientific investigation, and the results that emanate from such study are not at all obvious in advance.

However, in developing a serious science of animal cognition we must resist the temptation to limit our questions to those based on human capacities. In the domain of communication, if we ask "Can nonhuman animals learn a human language?" the answer is likely to be (as I argue in the rest of this book) that they cannot. If we let the matter rest there, however, we will undoubtedly be ignoring the vast range of other possible questions about animals to which the answers might well be quite different—and far more interesting.

7

What Primates Have to Say for Themselves



And many of the tales that Chee-Chee told were very interesting. Because although the monkeys had no history-books of their own before Doctor Dolittle came to write them for them, they remember everything that happens by telling stories to their children. And Chee-Chee spoke of many things his grandmother had told him—tales of long, long, long ago, before Noah and the Flood—of the days when men dressed in bear-skins and lived in holes in the rock and ate their mutton raw, because they did not know what cooking was—having never seen a fire. And he told them of the Great Mammoths and Lizards, as long as a train, that wandered over the mountains in those times, nibbling from the tree-tops. And often they got so interested listening, that when he had finished they found their fire had gone right out; and they had to scurry round to get more sticks and build a new one.

—*The Story of Dr. Dolittle*

The birds, the bees, the frogs, and the other animals we have looked at all can teach us. If we hope to find communicative behavior anything like our own, though, surely we ought to look at animals closer to home: the (non-human) primates.

If we are interested only in finding some anticipation of human language, the resulting survey is largely discouraging. Primate communication is qualitatively similar to that in other animals, and while intriguing and novel features are present, they do not in the end point to systems that are interestingly closer to English than, say, bird calls. We can certainly learn a great deal, though, by asking how our primate cousins communicate in their natural circumstances.

Compared to bird calls, monkey (and ape) vocalizations, as systems, are rather similar. They comprise a limited range of signals, each of which seems to express some aspect of the animal's internal state. The set of signals is innately determined, though in some special circumstances, limited kinds of learning affect the ways in which they are used and interpreted. In a few cases, we have evidence that individually simple signals can combine to yield a new signal whose import is not identical with any of its parts. The possibilities for such combinations are extremely limited and do not yield any sort of productive system that serves as the basis of free innovation.

In some instances, a connection exists between the choice of a vocalization and some aspect of the external world (the presence of one specific sort of predator as opposed to another); some researchers would say that the calls in question *refer* to eagles and leopards, much the way words of a human language refer to things. If this is indeed the right way to characterize these cases, the role of reference (or any other relevant property of human languages) remains both limited and rare. Furthermore, it may turn out that thinking of these signals primarily as references to things in the world will actually obscure our understanding of some aspects of their structure.

Natural communication among primates can be based on vocalizations or on gestures. A difference in biological classification coincides to a surprising extent with this distinction: monkeys tend to have richer systems of vocal signals but use fewer significant gestures, while the opposite is true, more or less, of the apes. These are broad generalizations, of course, but they explain why our discussion of vocalizations focuses on monkey species such as vervets and rhesus macaques, and not on the chimpanzees, gorillas, and bonobos that are of interest when we turn to gestural signals.

We begin with a look at the alarm calls given by vervet monkeys (*Cercopithecus aethiops*) upon sighting a potential predator. Distinct calls are given for distinct types of predator, and these elicit distinct types of appropriate responses in the caller's fellows. This somewhat remarkable fact has produced a large literature on the question of whether the calls are genuinely referential—and it has also stimulated related studies on the alarm behavior of a variety of other animals. The example of the vervet throws interesting light on the superficially similar, but structurally rather different, behavior of other animals such as chickens, ground squirrels, and marmots.

Researchers have also studied alarm calls of several species of lemur, a primate somewhat more removed from us although within our order. Lemurs produce a number of other communicative vocalizations, and these are worth investigating as well. Perhaps even more interesting is the lemur's use of a completely different channel, that of olfaction, in structured ways to get across messages that are central to the animal's way of life.

Of course, it is not only prosimians such as the ring-tailed lemur that use sound for purposes other than sounding an alarm. Further study of the vervet monkey reveals a range of other calls beyond those that first made the animal famous in the ethological literature. Close observation has taught us a tremendous amount about the cognitive world of the vervet on the basis of these other vocalizations. In fact, the clearest evidence for the referential nature of vervet vocalizations is perhaps to be found in the properties of calls that are related to the social organization of vervet groups.

What of our closest relatives, the higher apes, such as the chimpanzee and the bonobo, the gorilla and the orangutan? These animals will be of particular interest to us in Chapter 10, where we ask about the extent to which they can be taught to use a human language under special conditions. What do they do on their own by way of communicating? It would be fascinating to discover the precursors of human language in their behavior, a result that some argue would be absolutely essential if we are to confirm the continuity of evolutionary sequence.

That does not seem to be the case, although laboratory studies *do* reveal cognitive capacities in apes that seem to be lacking in monkeys. It is an extremely interesting result, not only in its own right, but also for what

it shows us about how to interpret the skills that can be elicited from these animals in the laboratory.

But we are getting ahead of our story. Let us begin by looking at the vervet monkey's most famous accomplishment, the ability to let others know not only that danger impends, but also something of the nature of that danger.

Alarm Calls

It was Tom Struhsaker who, in 1967, drew the attention of students of animal behavior to the vocalizations of vervets. These little "Old World" monkeys are found widely in savannah, forest, and semidesert parts of sub-Saharan Africa, where they form one of the most common primate species on the continent. They are rather small in size (adult males weight about ten pounds, females perhaps eight) and subject to a variety of predators. Fortunately for their survival, they have a corresponding variety of ways to protect themselves.

The animals that prey on vervets fall into three general classes. First are the large cats, primarily leopards, that chase the monkeys on the ground. The dexterity of the vervet provides a great advantage in climbing, and an effective escape strategy when pursued by a leopard is to climb a tree.

A second class of predator would be more than happy to see the vervet employ this strategy. The martial eagle (along with two or three other large birds of prey) is likely to swoop out of the skies to carry off a vervet for lunch. If the target were to climb a tree, that would make the eagle's task much easier. When an eagle is sighted, therefore, the best thing for a vervet in a tree to do is get down and hide in the bushes.

Finally, a variety of snakes, including pythons, mambas, and cobras, are dangerous to vervets, but only if they catch them unawares. In the presence of these it behooves vervets to pay constant attention to their location, which makes evasion relatively easy.

Struhsaker observed that when predators of one or another of these types are spotted by one of a group of vervets, different-sounding alarm calls are given depending on which type of danger is present. Leopards and other large cats elicit loud barks, or "leopard alarms"; upon seeing an eagle, the monkey gives a characteristic kind of cough or "eagle alarm"; while a third, acoustically distinct sound called a "chutter" or "snake alarm" is produced when a python or other dangerous snake is seen.

Even more fascinating than this differentiated calling is the response of the other vervets. When one of them gives a leopard alarm, the others bark loudly and run for a tree, if on the ground; or climb higher, if already in a tree—regardless of whether they themselves can see the leopard. Similarly, when one member of the group gives an eagle alarm, the others climb down from trees they are in and rush to the bushes. A snake alarm results in all of the other monkeys standing up on their back legs and looking around in search of the snake. They may even approach and mob it, giving further calls (from a safe distance) to ensure that everyone knows where the danger is.

In each case, the response to the alarm call is appropriate to the kind of danger the predator presents. The easy and obvious interpretation, for the human observer, is that the calling monkey is shouting "Leopard!" (or "Eagle!" or "Snake!") and the others are simply acting sensibly on the basis of this information. But is this anthropocentric story the right one? Is the monkey really "referring" to a specific animal? Does the call simply reflect a kind of fright? Do the other monkeys act on the information that a specific danger is at hand (to wit a leopard, an eagle, or a snake) or are they merely responding to their colleague's fright?

Philosophically, the difference between seeing the monkey's alarm call as having a specific external reference, on the one hand, or as simply reflecting the animal's internal state of fright, on the other, is a significant issue. To see that it is also behaviorally interesting, let us consider another, superficially similar case: that of ground-dwelling sciurids (chipmunks, ground squirrels, prairie dogs, marmots, and the like).

For a Belding's ground squirrel (*Spermophilus beldingi*), danger again comes in more than one form. An aerial predator such as a hawk may swoop down out of the sky; or a carnivore such as a badger, weasel, or coyote may stalk the animal on the ground. Like the vervets, apparently, a squirrel that detects one of these potential dangers gives one or the other of two acoustically distinct alarm calls, and the other animals respond in a more or less appropriate way (ducking immediately into a hole for the aerial predator and looking around watchfully for the other enemies).

When we look more closely, though, we see differences between the vervets and the squirrels. The vervet calls are all distinct from one another, but the squirrel alarms represent the two ends of an acoustic continuum, with intermediate forms also heard on occasion. More important, the squirrels often give what seem to be the "wrong" calls. When closely pursued by a carnivore, they may give an "aerial alert" call, and a hawk sighted at a great distance or standing on the ground may elicit a "ground alarm" call.

The consensus that has emerged about the interpretation of alarm calls by ground squirrels is that these vocalizations indicate not the specific nature of the potential predator, but rather the degree of urgency of response to the danger. Hawks can swoop out of the sky very quickly, and when one is around, it is essential to duck for cover immediately; the same is true when a fast-moving carnivore is in hot pursuit. When a coyote is simply stalking the neighborhood, the squirrels need to remain alert, but not necessarily to drop everything and run. The same is true if a hawk is barely in sight, not near enough to attack without warning.

Squirrels, it seems, have basically one thing to say ("Look out!"), and differences in the way they say it reflect their degree of concern. Vervets also reflect degree of urgency in their alarm calls, but they do so by calling longer and more loudly when the danger seems greater. A variety of ingenious experiments, primarily conducted by Dorothy Cheney and Robert Seyfarth, have established quite conclusively that vervets have (at least) three distinct things to say about clear and present danger, not just one.

Much of the work in support of this conclusion involves playing back recordings of naturally produced alarm calls to unsuspecting monkeys in the absence of any actual predator. When leopard, eagle, or snake alarms are played, regardless of the length or loudness of the call (and thus, the implied urgency of the danger), listening vervets climb trees, hide in bushes, or stand up and look around, as appropriate. Their activity suggests strongly that three distinct *categories* of danger are signified by different calls, not three degrees of imminence of impending disaster.

For a second line of argument in support of this conclusion, Cheney and Seyfarth ran a number of experiments in which they repeatedly played, for instance, the leopard alarm recorded from individual A to other members of the group in the absence of any leopards (or of A). After a certain amount of time, the other vervets stop running into the trees and begin to ignore these alarms. Vervet A has been shown to be unreliable as a source of information about leopards. Once this degree of distrust in A's leopard-calling behavior has been developed, other calls can be played, and several interesting results emerge. First, the monkeys respond to the leopard alarm calls of some other individual, B, quite normally. And second, even A's calls are responded to when they involve eagles or snakes. That is, the fact that A is demonstrably to be ignored on the subject of leopards does not in itself compromise his evidence about other predators, or other animals' calls indicating a leopard.

The possibility that alarm calls from specific individuals can be identified as unreliable is not limited to vervets. Similar results have been obtained in work on Richardson's ground squirrels (*Spermophilus richardsonii*), a species whose alarm calls do not appear to depend on the identity of the predator as opposed to the urgency of the threat posed. C. N. Slobodchikoff has claimed that the alarm calls of another sciurid, Gunnison's prairie dog (*Cynomys gunnisoni*), not only vary depending on the kind of predator (hawk, coyote, human), but also that "within a predator category, the prairie dogs can incorporate information about the physical features of a predator such as color, size, and shape." If more research confirms these remarkable claims, it would mean that these prairie dogs have the most sophisticated system of vocal expression in the animal world, apart from human language.

Further research in vervet communities turns up other alarm calls as well. There may be one for mammals that are less obviously dangerous than leopards, but worth keeping an eye on; one for baboons, in places where those apes prey on vervets; or one for "unfamiliar humans," especially the Maasai tribesmen of the area. In some (savannah) areas of Cameroon where vervets are hunted by feral dogs, the dogs are treated as "leopards." Elsewhere, however (in forests), the dogs that might be encountered are working for human hunters; shouting a lot and climbing into a tree would not help in evading the real danger. What this situation requires is sneaking off into the bush. And indeed, these monkeys have developed another call, a "dog alarm." It is short and quiet, and on hearing it the other vervets sneak off into the bush where hunters cannot follow.

Sharing the News

A common feature of alarm calling is that when multiple species inhabit the same area, they are often able to make use of one another's alarm calls. This is nicely illustrated by the vervets, in areas where they share territory with animals that have their own systems. For instance, superb starlings also have aerial and ground predator alarms, and vervets in these areas respond to starling alarm calls in ways appropriate to the birds' own categories. A starling's aerial alarms may make the vervets climb down and head for the bushes, while terrestrial alarms may send them into the trees.

The predators that pose a danger to starlings are not all sources of concern to vervets. As a result, it is reasonable that the extent to which vervets pay attention is related to the closeness of the starlings' categories to their own. Vervets seem sensitive to the fact that starlings' calls do not have quite the same "meaning" as their own. For instance, the starlings' aerial predator call is given for a broader range of raptors and other birds. When they hear it, vervets tend to look up, but not to automatically run for the bushes. The starlings' terrestrial predator call is much more comprehensive than that of the vervets, and is given not only for leopards and snakes but also for other dangers that vervets do not worry about (like other vervets!). While they pay attention to this call when they hear a starling produce it, they do not treat it in exactly the same way they would treat the leopard call of a conspecific.

These facts confirm the impression that vervets have categories for different sorts of predators, and their calls reflect these. They also show that some form of learning is relevant in relation to these systems. That must be the case if the calls of other species (which bear no particular acoustic resemblance to the vervets' own calls) can be associated with the same categories.

This conclusion is also supported by research on quite different animals. Among lemurs, species such as the ring-tailed lemur (*Lemur catta*) and Verreaux's sifaka (*Propithecus verreauxi verreauxi*) have alarm call systems in which the individual calls can be shown, by arguments like those above for the vervet, to designate particular types of danger. Typically, they distinguish aerial as opposed to terrestrial predator types. Other kinds of lemur, such as the ruffed lemur (*Varecia variegata variegata*), have alarm calls that differ, like those of the ground squirrel, only in terms of urgency.

Both ring-tailed lemurs and sifakas have distinct calls for two categories of danger, although the calls themselves are not acoustically similar in the two species. Nonetheless, ring-tailed lemurs from areas where the two are found together can be shown to respond appropriately to the calls of a sifaka when these are played back to them. On the other hand, ring-tailed lemurs from a group raised entirely in an animal park (in Japan) where there were no sifakas did not respond to these calls. They had never had the opportunity to learn that these other animals know how to say "eagle," too—although they say it differently.

Another kind of learning must be involved to the extent that particular groups of vervets come to produce new, contextually appropriate calls, like the baboon and dog alarms mentioned. In the case of feral hunting dogs, all that is involved is learning a new condition of use for the leopard call, but the other two alarms present a different problem. If alarm call systems are innate in origin—as I argue below—we need to account for the appearance of novel calls on the basis of environmental factors. One possibility is that the vocalizations are part of the animal's innate repertoire, and the innovation consists in conditioning their use under certain circumstances of danger. This suggestion is consistent with what is known about the acoustic structures of the calls concerned, but further speculation will have to await the collection of additional data.

Glimmerings of Syntax

Among the monkeys that make use of one another's alarm calls are two species that live together in the Tai forest in Côte d'Ivoire, Diana monkeys (*Cercopithecus diana*) and Campbell's monkeys (*C. campbelli*). Each has discrete alarm calls for leopards and for eagles. Male and female Diana monkeys have calls that are acoustically quite different, but each sex responds similarly both to its own calls and to those of the other sex. The difference between one call and the other depends universally on the nature of the predator, not on some other factor such as the urgency of the threat or the direction from which the predator is approaching. The alarm calls of the two species are distinct from one another, but all of the monkeys respond in essentially the same way to (a) an alarm call of their own species, (b) an alarm call of the other species, or (c) the vocalizations of an actual predator. They reply with their own corresponding calls and take evasive action appropriate to the predator in question. If this were all there were to the situation, we would simply have an unambiguous example of the sharing of alarm calls across species.

There is a further twist, however. In situations where the danger is not very great, male Campbell's monkeys emit a pair of low, resounding "boom" calls before their alarm calls. The boom calls may be given when a large branch breaks or a tree falls, or when the monkey hears a distant alarm call, or when the predator is sighted at a distance. In each instance the level of danger is low or completely inferential, as opposed to the direct threats that elicit normal alarm calling. And as opposed to what happens when they hear a normal Campbell's monkey leopard call, when a Diana monkey hears a "boom"-introduced leopard call, they do not respond to these more complex vocalizations with calls of their own or evasive action. Apparently they know that this sequence indicates the mere possibility of danger, not its imminence.

A series of playback experiments made it possible to explore this alternative a bit further. The normal eagle and leopard alarms of Campbell's monkeys were played through speakers, and the Diana monkeys responded as expected to each. When a boom sequence preceded the exact same call by about twenty-five seconds, however, the Diana monkeys simply went about their business. It seems they interpret the boom as something like a modifying "maybe," so "boom—leopard" means "maybe a leopard." Furthermore, the Diana monkeys recognize this message specifically in the context of Campbell's monkey calls. Playbacks of Diana monkey alarm calls always elicited responding calls and evasive action, even when these were preceded by the same boom that mitigated the force of the Campbell's monkey calls.

The situation is most unusual, not only for alarm calling, but more broadly for signals in animal vocal or gestural communication. What is striking is that the Campbell's monkey calls, at least as interpreted by the Diana monkeys, involve the combination of two distinct signals (the boom and the normal eagle or leopard call) to create a new unit that means something other than what either signal means by itself.

The amount of "syntax" we can see in this example is extremely limited. The combinations consist of exactly two elements, one of which is fixed (the boom) and the other of which is taken from a restricted set (other alarm calls, for eagles or leopards). As I argue in Chapter 8, such a principle of combination displays none of the major syntactic properties of human natural language; in particular, it is neither hierarchically organized nor recursive in nature. Nonetheless, it is the closest we have come to finding a system involving anything like syntax in animal communication.

A final area of interest in the acoustic communication system of nonhuman primates is the possible role of learning. To what extent is relevant experience necessary for the animal to come to control the set of calls it eventually displays? As we have seen in the case of birds, quite similar animals can have systems that vary greatly in the relative importance of nature and nurture.

Although the issue is complex, it seem reasonable to conclude that no learning is necessary for a monkey or ape to produce its calls in roughly the appropriate circumstances. Monkeys have fully mature and accurate calls at a very early age (twelve weeks). Animals raised in isolation, or those deaf at birth (either naturally or as a result of experimental mutilation), nonetheless produce appropriate calls. It seems evident that auditory experience does not play a crucial role in the development of a production capacity.

Contradictory evidence hints at the possible influence of environmental input on call learning in macaques. Rhesus and Japanese macaques have somewhat different vocal repertoires, and one experiment suggested that rhesus macaques raised by Japanese macaque mothers sound more like Japanese macaques than they would otherwise, and vice versa. But another experiment showed no such effect. A group of rhesus babies all came out sounding like rhesus, not like their Japanese foster parents. In any case, these two species are too similar to provide a valid test, since their natural vocal repertoires already overlap.

Basically, there is no evidence that learning in a strict sense is involved in the production of monkey vocalizations. This thesis is consistent with the suggestion of considerably less cortical involvement in sound production in monkeys than in humans, as noted above. Instead, subcortical structures in the limbic system are more fully involved. That basic architectural point need not mean that experience has no role in development. Field studies indicate that the monkeys do learn a certain amount about when and under what circumstances to call. That is, while they do not need to learn the actual calls, experience seems to play a role in refining their notion of the circumstances under which the calls are, as it were, called for.

As an example, in Cheney and Seyfarth's work with vervets, it was obvious that infants had only a vague notion of what constitutes an appropriate stimulus for, say, an eagle call. Young monkeys gave this call for a variety of large birds, not only for the limited number of species that actually prey on vervets. On at least one occasion, an eagle call seems to have been triggered by a falling leaf! By the time vervets reach maturity, however, their sense of what constitutes an eagle, and when it might be appropriate to point one out, has been substantially refined.

This result shows that monkeys' ability to use calls in appropriate ways develops, within narrow limits, in interaction with experience. There is little flexibility in this kind of learning, however. When Japanese macaques are brought up with rhesus macaques or vice versa, the animals do not adapt to their environment by using their innately provided repertoire of calls in a way appropriate to the expectations of the other animals around them. Some learning does go on in these experiments as far as what the calls of others mean: the cross-fostering mothers learn quickly what their adoptees' calls mean, even if it is not what members of their own species usually mean by a similar-sounding call.

There is another sense in which learning takes place in the vocal communication systems of monkeys. To some extent, local dialects develop: monkeys who are around each other tend to sound alike. This kind of fine-tuning is not the same as if the calls were learned from experience in the first place, but it does illustrate some modification of the production system on the basis of experience.

In vervets, lemurs, and other primates, alarm calls are far from the end of the story when it comes to communicative vocalization. These animals call when they find food (perhaps differently depending on whether the food is rare and desirable or just ordinary), in aggressive confrontations, during sexual activity, and at other times as well. The number of calls that can be differentiated depends on the species (and on the auditory acuity of the investigator), but does not seem to exceed one or two dozen. Some have been the subject of investigations that reveal a good deal about the mental life of the animals involved.

Like lemurs, vervets have a number of other vocal signals that play roles in the day-to-day social interactions of members of a group, with one another and with other groups. For instance, when one vervet group encounters another, a characteristic vocalization, a "wrrr," is given upon sighting. If the outsiders get close enough and actually make contact, the wrrr may be replaced by a chutter.

Within the group a lot of grunting goes on. This happens under four general circumstances: (a) when submissive meets dominant, (b) when dominant meets submissive, (c) when one monkey goes out into an open area, and (d) when a monkey encounters another monkey not of his own group.

Cheney and Seyfarth studied these intragroup grunts in considerable detail. They had the impression that they could more or less distinguish one kind from another, but failed in their initial attempts to find gross acoustic features distinctive of each. As a result, they considered the possibility that vervet grunting is always the same, the difference being purely contextual. A submissive animal knows his relation to a dominant, that is, as does the dominant, so there is no need for them to have different ways of expressing

that relationship. Perhaps a grunt is taken one way or another depending on the characteristics of the context.

But this is not the case; acoustic distinctions must exist among the grunts that are used under different circumstances. Cheney and Seyfarth recorded a number of grunts by different individuals in different situations, then played them back through hidden loudspeakers to others in various contexts. The four grunt types (based on the circumstances under which the original recordings were made) elicited qualitatively different responses. For instance, the "submissive-to-dominant" grunt caused the hearer to gaze toward the speaker, while the "other-group" grunt caused him to look out at the horizon. There must, then, be differences in the grunts, differences in the sounds that are interpreted by the monkeys, even if human listeners performing a detailed acoustic analysis find it hard to identify what those features might be.

In addition, these differences have an importance for vervets in their relations with one another. When some new males joined a group, for the first few days they and the members of their new group made other-group grunts to one another. After a while, though, the newcomers began to make submissive-to-dominant grunts to the group's dominant male, and the females of the group started making intragroup grunts to the newcomers as well. These were either submissive-to-dominant or dominant-to-submissive, depending on the individual's relative place in the group's pecking order—a matter that seems to have been resolved rather quickly. The different grunts do convey different sorts of information, then, and are not merely a single signal that can be interpreted in different ways depending on the context.

Proceeding on the assumption that the various calls really are different, we can ask whether they "mean" for the vervet what they seem to mean to us. One possibility is that these vocalizations are purely automatic, involuntary reflections of something—either of the vervet's internal state (perhaps alarm) or of some action the vervet is about to take.

In the case of the alarm calls discussed earlier, this last possibility is disconfirmed, because what the vervet actually does after giving the call varies. The monkey may do nothing at all, or may climb up a tree, or may climb down from a tree, without any necessary and inflexible relation between call and action.

If the call is not a reflection of impending action, what about the possibility that it reflects the animal's internal state fairly directly? This thesis would initially seem to be supported by the neurobiology: recall that vocalization in at least some monkeys appears to be initiated primarily in the subcortical limbic system, rather than in the cortex. But that notion will not suffice, for several reasons.

First of all, solitary vervets do not give alarm calls. While one might imagine that the presence of an audience affects the animal's internal state, this possibility somewhat reduces the attractiveness of our hypothesis. Studies of a variety of primates show that under some circumstances, the animals can apparently suppress their calling behavior—even while making the facial expression that would normally accompany it! Some element of voluntary control must be involved.

Second, dominant animals call more than submissive ones do. Why should this be? Perhaps because dominants are the ones doing most of the breeding. It is their genes that are more at risk to predation, so they care more about the group. In general, too, more alarm calling takes place in the presence of close kin or offspring than in the presence of peripherally related group members. This sensitivity to one's own relation to other nearby animals is seen in a great many other animals as well (witness the food and alarm calls of chickens).

In the particular case of vervets, all of these features demonstrate that alarm calling is sensitive (at least) to the audience, which means it cannot simply be a direct reflection of the monkey's internal state of alarm. Rather than being merely expressive, vervets perhaps produce alarm calls in order to influence the behavior of the other vervets, to get them to take appropriate evasive action with respect to the specific threat that is at hand.

Furthermore, the calls are sometimes given under circumstances that appear have the character of deception. On occasion when two groups were fighting and one group was clearly losing, some member of that group gave a leopard call. All of the monkeys ran for the trees and the fight stopped—but there was in fact no leopard, as least as far as the observers could determine. The calling monkey cannot have been frightened by a leopard if there was none. We might interpret his call as an attempt at deception, but remembering the cautions of earlier chapters, we must stop at the conclusion that the caller was attempting to get the other animals to disperse.

We can at least conclude that alarm calls are not just an automatic reflection of internal state, but are intentional. The monkey says "Leopard!" not because he is himself about to run into the tree, or because he has seen a leopard and is scared, but because he wants the others to take cover. Thus, the vervet is what Cheney and Seyfarth (following Daniel Dennett) call a *first-order intentional system.* The animal produces a signal by which he intends to influence the behavior of others.

Can we go beyond this, to establish that the calls constitute what Cheney and Seyfarth call a *second-order intentional system*? To show that, we would need evidence that a monkey intends to affect not just the behavior of another, but that animal's knowledge or state of mind. Here the evidence is disappointing. While the monkey's tendency to call depends on the audience, it does not seem to depend on the state of knowledge that members of that audience can be inferred to have. If the point of calling "Leopard!" were to make sure that everyone knew there was a leopard, one monkey would not need to call if other monkeys had already called, or could perfectly well see the leopard. That is not what happens. When one calls, the others also call, regardless. There is no evidence that they take one another's state of awareness of the danger into account in signaling.

Still, they clearly do identify the calls they hear with a specific calling individual. By repeatedly "crying wolf" with recordings played through hidden speakers, the experimenter can habituate members of a group to monkey X's leopard call, for instance; but monkey Y's leopard call will send them off to the trees. Reliability (or the lack of it) is associated with specific individuals, which means the animals must be able to make identifications on the basis of the voices they hear.

What should we conclude that the calls "mean"? We have already seen that they do not mean simply "I'm scared!" Should we say, then, that they actually refer to something in the world? On that interpretation, a leopard call would mean "There's a leopard!" or perhaps "There's a predator that attacks in a particular way!" The facts are certainly consistent with such an interpretation, but there is no evidence that forces that interpretation as opposed to the more conservative view that the call means "Go climb a tree!"

One consideration is more consistent with the "Go climb a tree!" interpretation than with "Leopard!" If the calls really refer to objects (predators) in the world, they are completely arbitrary. The leopard call bears no relation at all to real leopards. It does not resemble in any way the sound of a leopard or evoke one other than by convention. The same is true for the eagle and snake calls. Words in human languages are arbitrary in this way, of course, but the relationship in primates is something of a mystery.

No complete explanation of the form of the calls is available, but seeing them as attempts to influence the actions of the other monkeys (rather than referring to something in the world) helps somewhat with the problem. The acoustic structure of all three alarm calls is similar. All are broadband sounds, with very sudden onset and a rapid rise time. They are thus perfectly structured to evoke the "startle" reflex in hearers, to get their attention and provoke them to interrupt what they are doing. This general response is known in a variety of monkeys and apes. Correspondingly, the acoustic analysis of alarm calls shows that they usually have just the kind of structure that could elicit it.

Among the vervets, we find the exception that proves this rule. Recall that in some areas vervets have developed an alarm call for dogs that might well be in the company of human hunters. Unlike the leopard, eagle, and snake calls, this call does not have a sharp and sudden character, but is rather soft and acoustically diffuse. It is not nearly as easy for a listener to localize, and it is similar to calls in other species that evoke caution rather than sudden abrupt action.

If we think of the alarm calls in terms of their intended effects, their acoustic structure makes sense. If we insist on seeing them as referring to particular predators directly, however, that structure remains arbitrary and inexplicable. Further, we see that when calls are generalized, the generalizations make sense in these terms. In areas where the leopard call may also be a response to sighting a feral dog, that apparent ambiguity is because the appropriate evasive response in the two cases is the same. "Leopard" does not refer to a species in anything like the way we think of one, but rather in a more functional sense, in terms of a class of appropriate reactions.

The referential nature of vervet calls is suggested more strongly by another of Cheney and Seyfarth's experiments, one that did not involve alarm calls. We have seen that two distinct calls can have more or less the same reference. The wrrr and the chutter both refer to "another group." Cheney and Seyfarth played misleading wrrr calls long enough that some listener became habituated: he learned that the caller was giving a false alarm, because no other group was coming into view. The researchers then played a chutter and discovered that the listener ignored this call too — because both wrrr and chutter refer to the coming of a new group, and the calling monkey had already proved unreliable in that respect. Contrast this with the fact that an individual's unreliable eagle calls do not result in the other vervets' ignoring his later leopard call. These experiments are suggestive of a "referential" interpretation, since two distinct calls with essentially the same reference are seen to be equivalent, while two calls with different reference are not.

Much can still be learned about the social and cognitive life of monkeys from a close study of their communicative behavior, as Cheney and Seyfarth show. These animals have a clear sense of individual identities, social and family relationships, and much else, and act on the basis of a highly structured view of the world and their own places in it.

In our efforts to understand the nature of communication, the principal point seems to be that vervets have a system which they use with the apparent purpose of influencing one another's behavior. There is no evidence that they (or any other monkeys) have a theory of mind in the sense of an understanding that other monkeys have their own knowledge of the world, that this knowledge plays a role in determining their actions, and that one can influence another's behavior by affecting that knowledge. As a result, we can conclude only that vervets intend to modify one another's actions, not that they try to deceive or otherwise shape one another's beliefs.

Communication among the Higher Apes

Prosimians (such as lemurs) and monkeys communicate vocally using elements from a limited set of calls. These number up to perhaps a few dozen and are largely innate; their conditions of use can be modified by experience, however. The calls are only partially voluntary, although the brain regions under whose control they lie are not well understood in most species. At least some of these animals (lemurs in particular, and many monkeys as well) also transmit and receive information through scent marking and other olfactory channels.

The primates of greatest interest to us, *Homo sapiens,* make use of open-ended vocal (or manual) systems based on the recursive combination of discrete units into hierarchically structured forms with complex semantic interpretations. These systems are acquired from within an innately specified range on the basis of experience and (apart from the occasional "Ouch!") are used under the voluntary control of cortical mechanisms. The perfume industry notwithstanding, we make only minimal communicative use of the information that can be derived from chemicals in our environment.

Given these rather sharp differences, we might expect that our nearer relatives among the primates, the great apes, represent some sort of intermediate position. We might look to them for communicative means that, while perhaps not really *language* in our sense, would still be more like language than the calls of lemurs and monkeys. In this we will be disappointed. From all existing studies, communication among the great apes seems qualitatively little different from that among the other nonhuman primates.

Vocalizations

Although there have been a few studies of communicative behavior among gorillas, orangutans, and bonobos, the great bulk of the literature concerns chimpanzees—specifically, Jane Goodall's extensive study of the behavior of these animals in the wild. An understanding of natural communication among chimpanzees is also important as background for the laboratory studies we look at in Chapter 10.

Chimpanzees seem to have a vocal repertoire roughly comparable to that of vervets, but it does not include a system of alarm calls differentiated in terms of the predator. Basically, only one vocalization indicates the presence of predators such as leopards, pythons, or human hunters: the *wraaa*. This lack of differentiation is surely connected with the fact that chimpanzees are larger, less vulnerable animals than vervets (or ring-tailed lemurs), and basically have little to fear in their environment (or at least, that was the case prior to the arrival of humans). When danger does appear, chimpanzees tend to respond in a uniform way: by climbing up into the trees. There is no particular pressure on chimpanzees to develop a set of finely differentiated alarm calls.

Another area where we might look for an attribution of reference in chimpanzee vocalization is food calls. Chimpanzees give a *food grunt* or a *food aaa* call when they find a source of food, and that attracts the others in the neighborhood to come and share the find. They are likely to call more extensively when the quantity of food discovered is greater. A number of different kinds of food might be found in the wild, with different ways of gaining access to it, so we might imagine that different calls would be made for different foods, but nothing of the kind seems to happen.

The one place where we can be sure that variations on a single theme make a difference is the chimpanzee's famous *pant-hoot.* This call is given by animals traveling or nesting alone, or between nesting families at night. One chimpanzee makes a series of pant-hoots and waits. Others respond in kind, and they alternate their calls. Since the pant-hoot of each individual is distinguishable from that of the others (presumably to the chimpanzees as well as to human observers), it serves to identify animals who may or may not be in sight of one another.

Pant-hoots are used too to identify oneself to one's fellows. We might almost think of them as individual "names," except that no chimpanzee ever produces any other's pant-hoot. That is, while we can think of Panzee's pant-hoot as something like an announcement "I'm here!" no one ever asks "Is that you, Panzee?" or "Where's Panzee?" Nor does anyone try to deceive the others by claiming falsely to be Panzee. While chimpanzees are certainly aware of the identities of the members of their group and others, and attend to differences in pant-hoot calls for information about the individuals around them, we have no reason to think of these calls as referring to individuals in any more interesting sense.

Chimpanzees make a number of other vocal calls to each other within groups. Some are the greeting of a subordinate to a dominant member of the group (*pant-grunt*) and, conversely, from the dominant to a subordinate (*soft bark*). At least three different sorts of *scream* are given by animals who are attacked, or upset, or copulating. The acoustic differences among these screams are not well understood, but other animals react differently to them, so there must be a distinction. A few miscellaneous calls of distress or surprise round out the inventory.

Chimpanzee vocalizations may be under the control of the animal's limbic system, rather than the cortical regions we associate with voluntary activity, although (as in the case of most monkeys) this is an issue that needs further research. Calling is by no means entirely automatic and involuntary. Numerous observations attest that chimpanzees can suppress their calls. They walk very quietly together when hunting other animals, for example, and a female suppresses her copulation screams when she is with someone other than the dominant male (who would probably respond by attacking her partner if he became aware of what was going on).

Other observations show chimpanzees can behave in ways that seem deceptive. They may give alarm calls when no danger is present so as to get another animal to go away, or they may not give food calls when there is a likelihood that such a call would cause others to come and take all the food. The former possibility is particularly important to explore further, as Goodall says that "the production of a sound in the *absence* of the appropriate emotional state seems to be an almost impossible task for a chimpanzee." By and large, their calls are closely bound to, and expressive of, these internal emotional states, rather than referring to things outside of themselves.

Gestures

Calls are not the only way (or perhaps even the most important way) chimpanzees communicate with one another. Many of their gestures are expressive too—apparently intentionally so. Goodall reports more than a dozen distinct gestures that the animals she observed use for social purposes. Probably there are many more, as confirmed by other observers of animals both in the wild and in captivity. Several of the signals are used to initiate play, while others solicit food, grooming, or other personal attention.

These gestures are more likely to be under conscious control and not automatic. The vocal signals discussed above seem to be largely an expression of the animal's internal state, and the information they convey to others is more or less a by-product of that. Manual gestures, in contrast, are apparently intended (in at least some cases) to produce a particular effect in others. Chimpanzees can be seen to make a gesture, and then to pay close visual attention to the other to see if the right reaction is forthcoming; they often repeat the gesture more emphatically if it is not.

Some, but not all, of these gestures are apparently quite natural and could be argued to be innate. In some cases, animals have obviously made up new gestures on the basis of novel circumstances. For instance, throwing wood chips at another chimpanzee to initiate play is unlikely to be an innately specified behavioral routine. An individual was observed to devise a strategy of passing by another while offering up one limp leg, inviting the other animal to grab it and play—all the while alternating his gaze between the potential playmate and the leg. Chimpanzee gestures seemingly are flexible in formation and use.

Not only manual gestures are used in this way. While chimpanzees' facial expressions are nowhere near as controllable as are those of humans, some — such as a fear grimace — are characteristic. Anecdotal reports tell of animals concealing their faces with their hands so as (apparently) not to reveal their fear. One gorilla covered its "play face" with its hand, presumably so as not to give away its playful intention. Goodall reports a particular gait that male chimpanzees use when leaving camp, a way of walking that conveys determination of purpose and invites others to follow. Reported instances of apparently deceptive chimpanzee behavior involve the use of this special gait to induce others to go off in one direction while the animal himself goes elsewhere.

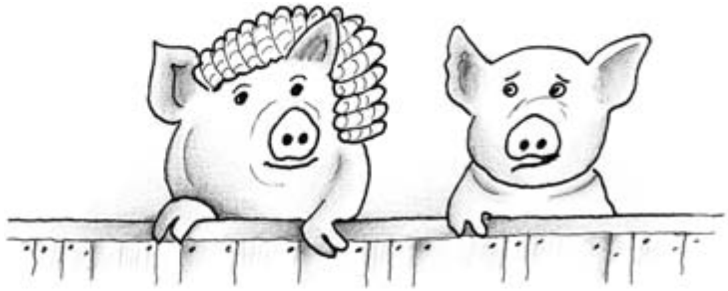
Finally, there is evidence for the use of objects as symbols. Chimpanzees in some groups are observed to trim and groom leaves in the presence of a desired female, as an invitation to mate. And bonobos traveling in the forest along networks of trails have been observed to stamp down vegetation or leave large leaves pointing in the direction they have taken at forks in the road, from which others in their group can follow them.

Except in the limited sense that all are means of communication, these gestures have little in common with human language. But the fact that chimpanzees and bonobos do make flexible use of gesture in the wild provides an important bit of context for the laboratory studies we will consider in Chapter 10. That nonhuman primates can be trained to use manual gestures in a meaningful way is to some extent a natural extension of normal behavior.

This is not to deny that it *is* an extension, especially to the extent that we can see the meanings of their learned gestures as arbitrary, and not fully iconic. At least some of the most widely reported "signs" that turn up in the vocabularies of language-trained chimpanzees and bonobos (such as COME-GIMME and MORE) turn out to deviate markedly from the American Sign Language signs that are their presumed models. Significantly, the deviations are in the direction of naturally occurring manual gestures with very similar content, documented by Goodall and others for animals in the wild. The question arises of whether those signs actually form part of a language-like system being learned, or whether they are simply adaptations of something quite different. To understand that issue, we need to appreciate something of the richness of the animals' natural gestural communication.

10

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 multisign 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.)

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 multi-lexigram 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.

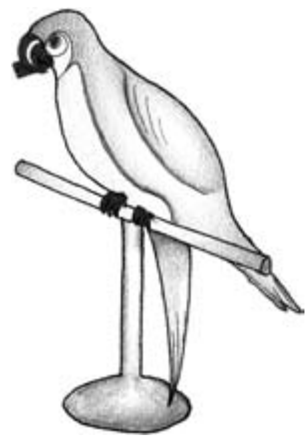
The last two are perhaps obvious requirements for treating gestures or lexigrams as "words." To see the importance of noninstrumentality, imagine what happens when I go to the vending machine in the basement, insert money, and press the buttons A-0-9 in sequence to receive a package of M&Ms. This is 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*

Language Instruction in the Laboratory

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.

Notes

1 Animals, Language, and Linguistics

Page **3**—Review of books about apes: Douglas H. Chadwick, Our unfortunate cousins, *New York Times Book Review,* 11 December 1994.

Page **4**—Response to Chadwick's review: David Pesetsky, How to tell the apes, *New York Times Book Review,* 25 December 1994, p. 23.

Page **7**—Actual number of human genes: *Science* 300:1484 (2003)

Page **7**—Proportion of genetic material devoted to the brain: *Science* 291:1188 (2001).

Page **14**—Russell on dogs and language: Russell 1948, p. 74.

2 Language and Communication

Page **16**—Communication among bacteria: Andrew Pollack, Drug makers listen in while bacteria talk, *New York Times,* 27 February 2001.

Page **18**—Evolution of communication: For an extensive and detailed discussion, see Hauser 1996.

Page **21**—Design Features for Language: Hockett 1960. Hockett's paper circulated in a number of different versions, with the precise list of design features changing slightly over time. The precise set discussed here is not identical to any single version of the paper; it is a starting point for discussion of interesting issues, not an exposition of Hockett's specific views.

Page **21**—Evolution of physical specialization for speech: Lieberman 1984.

Page **21**—Less-common sensory modalities: Hughes 1999.

Page **27**—Language-particular forms of animal sounds: See www.georgetown.edu/cball/animals/.

Page **31**—Language as made for lying: Sturtevant 1947, p. 48.

Page **32**—The piping plover's trick: Ristau 1991.

Page **32**—Machiavellian intelligence: Byrne and Whiten 1992.

Page **35**—Signature characteristics: Bradshaw 1993.

3 On Studying Cognition

Pages **41ff.**—The story of Clever Hans: Pfungst 1911.

Page **43**—Clever Hans's real abilities: Cited in Pfungst 1911, p. 5.

Page **44** — Morgan's canon: Morgan 1894, p. 53.

Page **47** — Theory of mind in apes: Povinelli 2000.

Page **48** — Deceptive watchbirds: Munn 1986.

Page **52** — See Anderson and Lightfoot 2002 for a more comprehensive discussion of the components of human language and their relation to human biology and cognition.

Page **53** — Nicaraguan Sign Language: Kegl, Senghas, and Coppola 1999.

Page **54** — Spontaneous development of signing in deaf children: Goldin-Meadow and Mylander 1990.

Page **54** — Creole development: For extensive discussion, see the papers in DeGraff 1999.

Page **55** — Dissociation of language and general cognitive development: Curtiss 1988.

Page **55** — Specific Language Impairment: Leonard 1998.

Page **55** — Williams syndrome: Bellugi et al. 1993.

Page **55** — Christopher the linguistic savant: Smith and Tsimpli 1995; learning BSL: Morgan et al. 2002.

Page **59** — Japanese advertising copy: Examples are collected at www.engrish.com.

4 The Dance "Language" of Honeybees

Page **64** — Bee dances as a language: Gould and Gould 1995, p. 59.

Pages **65ff.** — The facts discussed in this section derive from several sources. Although the classic description is that of von Frisch 1967, several recent works are highly readable and update his account in important ways. Notable are Gould and Gould 1995, Seeley 1995, and Wenner and Wells 1990. The last takes a highly skeptical view of von Frisch's results (and methods), a matter discussed from the other side in chapter 4 of Gould and Gould 1995. My discussion cannot possibly do justice to all that is known about bees and their dances. Dyer 2002 provides an updated and broader survey, with references.

Page **65** — Aristotle citation: Von Frisch 1967, p. 6.

Page **66** — Bee dance as a language: Lubbock 1874, p. 160.

Page **66** — Odor as the key to foragers' success: Maeterlink 1901; Lineburg 1924.

Page **67** — Mystery of communication in the dance: Francon 1939, p. 143.

Page **68** — Von Frisch and the Nazis: Gould and Gould 1995, p. 58. Actually, von Frisch's relations with the Nazi authorities and his expressed opinions during the period 1941–45 were nowhere near as unambigiously heroic as his postwar rehabilitation would suggest. These matters, explored in some depth in Deichmann 1996, pp. 171–200, do not bear directly on the scientific issues at stake here.

Page **70** — The economics of energy and foraging: Seeley 1995.

Page **70** — Information transfer in the dance: Michelson 1999.

Page **73** — Role of sound in the dance: Michelson 1999.

Page **75** — Trembling dance: von Frisch 1967, p. 282, quoting from one of his earlier papers.

Page **76** — Cricket chirping not "language": Gould and Gould 1995.

Page **80** — Discrete combinatioral systems: Pinker 1994, p. 84.

Page **83**—Critics of the dance language hypothesis: Wenner and Wells 1990. Wenner 2002 attempts to revive earlier criticisms of the claim that dance (as opposed to odor) communicates the location of a food source to other potential foragers. However, he does not really rebut the arguments that have been offered in favor of this view since the appearance of the earlier book.

Page **88**—A thorough discussion of some of the issues in insect navigation, not limited to bees and their dances, is Gallistel 1998.

5 Sound in Frog and Man

Page **92**—Bug detectors in the frog's visual system: Lettvin et al. 1959.

Page **94**—South African clawed frogs: Kelley and Tobias 1999.

Page **98**—Gerhardt and Huber 2002 provide further information about the precise mechanisms by which frogs of various kinds produce sound.

Page **98**—Wilczynski, Zakon, and Brenowitz 1984 detail the vocal communication of the spring peeper, with attention to the underlying neurophysiology.

Page **100**—A summary of this early work on the bullfrog is in Capranica 1965.

Page **107**—Those interested in delving deeper into acoustic phonetics are referred to Ladefoged 1996.

Page **110**—Speech is special: Liberman 1982. Papers by Liberman and his colleagues relevant to the material of this section are collected in Liberman 1996.

Pages **115ff.**—Motor theory of speech perception: Again, see the papers in Liberman 1996.

Page **124**—Cognitive importance of distinctive features: Jackendoff 1994.

Page **125**—Two olfactory systems in mice: *Science* 299:1196–1201 (2003).

6 Birds and Babies Learning to Speak

Page **133**—Chickadee call structure: Hailman, Ficken, and Ficken 1985.

Page **135**—Song versus calls: Marler 1999. Points cited later in the chapter also derive from this classic discussion.

Page **142**—Neurophysiology of birdsong: Nottebohm 1999.

Page **143**—Motor theory and birdsong perception: Williams and Nottebohm 1985.

Page **145**—Notions of song "learning" across a variety of species are surveyed in Boughman and Moss 2003.

Page **147**—Brain nuclei common to oscines, hummingbirds, and parrots: Jarvis et al. 2000, p. 632.

Page **155**—Relation between seasonal learning and neurogenesis: Hauser 1996, pp. 144ff.

Page **157**—Human language organ: Pinker 1994; Anderson and Lightfoot 2002.

Page **161**—Acquisition of human language: Our knowledge of the earliest stages of the child's path to language has expanded greatly since the early 1990s. The account here is drawn from Anderson and Lightfoot 2002 and relies heavily on Jusczyk 1997, de Boysson-Bardies 1999, and Kuhl 1999. De Boysson-Bardies's book, in particular, provides a readable introduction to much of this research.

Page **161**—Manual babbling by deaf babies: Petitto and Marentette 1991.

Page **164**—Genie: Curtiss 1977. Curtiss 1988 provides a somewhat broader survey of classic cases relevant to language learning when exposure to language is lacking during the sensitive period for acquisition. Emmorey 2002 surveys the literature dealing with the special case of hearing-impaired children who gain access to (signed) language relatively late in life.

7 What Primates Have to Say for Themselves

Page **169**—First observations of alarm calling in vervets: Struhsaker 1967.

Page **171**—Squirrel alarm calls based on urgency: Macedonia and Evans 1993.

Page **171**—See Cheney and Seyfarth 1990 for a wealth of information about communication and much more in vervet monkeys.

Page **172**—Unreliable signals in ground squirrels: Hare and Atkins 2001.

Page **172**—Prairie dog alarm calls: Slobodchikoff 2002.

Page **175**—Combining of calls in forest monkeys: Zuberbühler 2002.

Page **180**—Limbic system in primate vocal production: Deacon 1992.

Page **182**—The vocal repertoire of the ring-tailed lemur, based on studies of lemurs in a study colony at Duke University, is described in Macedonia 1993. Included are details of the acoustics of these calls and the circumstances under which they have been observed.

Page **184**—Chemical signals in lemurs: Kappeler 1998.

Page **185**—Chemically assisted theft in tropical ants: Breed et al. 1990.

Page **191**—Functional interpretation of primate calls: Owren and Rendall 2001.

Page **195**—Chimpanzees' inability to produce deceptive vocalizations: Goodall 1986, p. 125.

8 Syntax

Page **214**—Requirements of verbs for specific arguments: Pinker 1994, pp. 112ff.

9 Language Is Not Just Speech

Page **234**—The linguistic analysis of signed languages, especially ASL, has generated a huge literature. An early introduction to this work, which retains much of its value, is Klima and Bellugi 1979. A more recent collection updating many of their points is Emmorey and Lane 2000. The impact of modality on the structure of language in general is addressed in Emmorey 2002 and Meier, Quinto, and Cormier 2002. I have also benefited greatly from Perlmutter 1991 and a 1996 presentation at the American Association for the Advancement of Science by Richard Meier, many of whose examples I use here.

Page **234**—First treatment of ASL in linguistic terms: Stokoe 1960.

Page **236**—Martha's Vineyard signing community: Groce 1985.

Page **257**—I am grateful to Susan Fischer for suggesting these examples and for confirming the judgments here with native signers.

Page **260**—Home sign systems: Goldin-Meadow and Mylander 1990, Goldin-Meadow 2003.

Page **260**—Development of creoles: DeGraff 1999, among others.

Page **260**—Emergence of Nicaraguan Sign Language: Kegl, Senghas, and Coppola 1999.

Page **260**—Effects of modality in signed languages: Meier, Quinto, and Cormier 2002.

10 Language Instruction in the Laboratory

Page **269**—The initial substantive report on the Gardners' work with Washoe is Gardner and Gardner 1969. Subsequent papers expand the picture of this project and its results, and a collection of relevant papers is found in Gardner, Gardner, and van Cantfort 1989. Wallman 1992 analyzes the ape language projects dealt with in this chapter through Savage-Rumbaugh's early work with Kanzi, and he finds serious defects in all of them. His negative assessment may be overstated at times, but from the point of view of experiment psychology his objections are generally cogent.

Page **269**—Washoe's later years: Fouts 1997.

Page **276**—Terrace 1979 gives a full report on the Nim project.

Page **277**—Grammatical sophistication in early childhood: Hirsch-Pasek and Golinkoff 1996 discuss some innovative experiments on the grammatical abilities of very young children.

Page **281**—Nim's signing as imitation: Terrace et al. 1979.

Page **282**—Nim's more conversational signing: O'Sullivan and Yeager 1989.

Pages **283ff.**—Chantek project: Miles 1990.

Page **286**—Plural of "anecdote": Bernstein 1988, p. 247.

Page **287**—Sarah project: Premack and Premack 1972.

Page **287**—Lana project: Rumbaugh 1977.

Page **288**—Sherman and Austin: Savage-Rumbaugh 1986.

Pages **289ff.**—Kanzi: Kanzi and his accomplishments have been described at length in two books addressed to general audiences: Savage-Rumbaugh et al. 1986 and Savage-Rumbaugh, Shanker, and Taylor 1998. In my opinion, the two books suffer from a combination of exaggeration and defensiveness, but both offer fascinating information and perspective.

Page **291**—Kanzi's development compared to that of a human child: Savage-Rumbaugh et al. 1993.

Pages **300ff.**—Alex: Pepperberg 2000.

Page **301**—The mechanisms of sound production in mynah birds, and the acoustic relations between their sounds and the human speech that it sounds like to us, are described by Klatt and Stefanski 1974. Pepperberg 2000, chapters 15 and 16, provides details about the corresponding issues in the speech of parrots such as Alex.

11 Language, Biology, and Evolution

Page **307**—Chomsky's argument about flightless birds: Chomsky 1980, p. 239.

Pages **309ff.**—Most of the discussion in this section derives from work by linguists such as Carstairs-McCarthy 2001 and Jackendoff 2002, or from scholars who have collaborated actively with linguists. Hauser, Chomsky, and Fitch 2002 presents many of these matters from one particular point of view.

Page **311** — Vocal tract shape as a preadaptation: Carstairs-McCarthy 2001.

Page **311** — Lowering of the larynx in other species: Fitch 2002.

Page **312** — Vocal abilities of Neanderthals: Lieberman 1984.

Page **314** — Bickerton's account of the emergence of human language: Bickerton 1990, 1995, 2000.

Page **315** — Baldwinian evolution is the enhancement through natural selection of the ability to learn certain advantageous skills or behaviors. The advantage conferred by useful behavior produces selectional pressure in favor of whatever genetic basis supports it. This differs from standard Darwinian evolution not in terms of mechanisms, but because what evolves is the tendency to acquire a behavioral trait (flying, speaking) rather than a physical characteristic (wings, shape of the vocal tract).

Pages **315ff.** — Relation of protolanguage to modern linguistic forms: Jackendoff 2002.

Page **319** — Sapir on the essence of language: Sapir 1921, p. 11.

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