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THE PROMISE OF
ANIMAL LANGUAGE RESEARCH

A DISSERTATION SUBMITTED TO THE GRADUATE DIVISION OF THE
UNIVERSITY OF HAWAII IN PARTIAL FULFILLMENT
OF THE REQUIREMENTS FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY
IN LINGUISTICS
AUGUST 1986

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ACKNOWLEDGEMENTS

This dissertation is the culmination of ten years of "higher" education, so I'll start my thank-you's from the beginning.

I would like to thank Joan Maling who taught my very first class in linguistics back in September, 1975, for filling my first exposure to the study of linguistics with the excitement of discovery. I thank Ray Jackendoff for sparking the fire that drove me to find out more. I recall fondly those sunny afternoons out on the grass at Brandeis. Ray set up an easle with a huge pad of paper on it, and proceeded to mark it up with X's with bars on top. He was teaching me X-bar theory before I even knew standard theory. At Tel-Aviv I had the privilege of being taught by Robert Lees, who never once broke down and called me "Esmé"--always "Miss Hoban", well, now you can call me "Dr. Hoban", thank you Dr. Lees.

I have been here at the University of Hawaii for five years now, studying in both the psychology and linguistics departments. There is one person to whom I am indebted for all her help: Dr. Martha Crosby--despite all odds, we made it!

Most of all, I would like to thank Moti Bordoley, who has been a strong motivating factor in my life since before I even started college. During the writing of this dissertation he suffered with me through those times when inspiration abandoned me, discussing the material and helping me work out my ideas. Ma agid lecha foof, ata gadol mikol.
ABSTRACT

Research over the past two decades has yielded some noteworthy results and a great deal of controversy in the field of animal language research (ALR). This dissertation critically reviews some of the recent reports of language acquisition by apes and dolphins and proposes that a closer relationship between ALR and linguistics is necessary for future progress in both fields. It is demonstrated that ALR sheds light on the process of language acquisition in humans: by comparing the results of language training in a chimp to language acquisition in a child we can see that both share a common non-linguistic stage of language learning. While the child goes on to acquire language, the chimp does not. Animals may be prevented from acquiring language by biological constraints, but this can only be clearly established by future work in which appropriate language models are employed. Current knowledge of linguistic processes in language acquisition and language change suggest new approaches for ALR. In particular, since recent work on creole languages has demonstrated their importance as a key to Universal Grammar, it is proposed that a creole-based model is the most appropriate language model for future research in ALR.
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PREFACE

When I was a small child, my father used to read to me from a book he had written and illustrated called *What does it do and how does it work*. The book was about big machines like bulldozers and tractors, in which I had little interest, and the pictures were large loose drawings in black and yellow. I had plenty of books about cuddly animals that were illustrated in full color, but I was drawn to that clunky, oversized book by the secrets it held. I have always been driven by a desire to learn "what it does and how it works", and after a wonderful experience learning Hebrew as a high school student in Israel, that interest turned to language.

While working on my bachelor's degree at Brandeis University, and later at Tel Aviv University, I became intrigued with the workings of language, how it came to be, and the questions of uniqueness and species specificity. I had to see first hand what happens when you try to teach animals language. My professors tried to talk me out of such foolishness, and Dr. Lees said that I should stick with apes-- at least they are evolutionarily related to us. But I didn't want to study apes, just *because* of the evolutionary relatedness. I wanted to study an animal that followed a completely different route to get where it is today. For dolphins that route included returning to the water and moving its nose to the top of its head-- now that's adaptation.

I came to Hawaii in 1981 to study dolphins. Before that I had only read about animal language research. Now I was involved in it first-hand. Better yet, I made friends with the dolphins at the laboratory.
They accepted me into their world and gave me and my swimming partner hours of swimming pleasure, gliding and turning in the sparkling sun dappled water. Sometimes we swam into the night. The water was cool but the dolphins' bodies kept us warm. There was something so relaxing in those hours of darkness, drifting peacefully with the dolphin, silent but for the explosive burst as they exhaled and quickly inhaled through their blowholes. Such a taking in of air can be a comforting sound.

Perhaps it was my attachment to the dolphins that prevented me from recognizing shortcomings in the language work until later. Like Terrace, who didn’t get to analyzing his tapes until Nim was gone, it wasn’t until I had left the laboratory that I could organize my thoughts about the dolphin project in particular, and animal language research in general. There were several facts which called out for explanation.

Why was it that the dolphins (and other animals in animal language research) always fell back on their natural behaviors to solve a new problem, never using their newly acquired "language skills" when it would have been advantageous to do so, but relying instead on basic perceptual strategies, like spatial orientation, sound, light and shape? If the artificial language used with the dolphins really required a big brain, then how could the "pea-brained" sea lions comprehend it?

My work on The Seth Project led to still further questions. Why is it that the child Seth’s utterances reminded me so much of Nim the chimp? Examining the documentation of a child’s language development from the age of one to four at weekly and daily intervals suddenly revealed that language acquisition was not so magical as it had seemed--
it develops rather slowly and by small steps. At the same time, it seems that actual linguistic development doesn't even start until sometime after the age of two.

Animal language research is a young field which has generated much activity (my bibliography contains over 200 entries). There is much more to animal language research than I could possibly have covered in the scope of this work, but I wanted to touch on what I consider to be the important issues. They range from some basic questions (Chapter I); an examination of the differences between ASL and what the signing apes have been taught (Chapter II); a comparison of child and ape (Chapter III); a critical review of work with dolphins (Chapter IV); to a proposal for a two-stage model of language acquisition (Chapter V) and a proposal for a language model for future animal language research (Chapter VI). These diverse issues were connected in my mind, and now here in this work. This dissertation is my attempt at a synthesis of these issues. I hope in the work to make the connections I see clear to the reader as well, and in this way provide a new perspective on a growing and controversial field.
CHAPTER I
INTRODUCTION

Probably every human who has language has wondered at one time or another about the possibility of talking with animals, communicating with them through language. Several comparative psychologists have taken this question seriously and designed experiments to find out about the possibility of teaching language to animals. These experiments have become part of a growing and controversial field: animal language research (ALR). ¹

As often happens when people have strong opinions about an issue, the controversy over ALR is dichotomized, with two extreme viewpoints. There is the "ALR is good" camp, whose members believe that with a little time and patience and plenty of hard work we will eventually break the language barrier and animals will achieve language too. This view is founded in the belief that language is an outgrowth of earlier call systems. This relates to what is known as "the continuity question", so familiar to comparative psychologists: Did language evolve in a continuous progression from earlier call systems, and is animal communication different from human language only in the variety of messages that can be conveyed (amount), or is there a difference in kind?² The "ALR is good" school of thought tends to go with the former: communication via call systems represents an earlier stage of a continuous progression towards language. Then there is the "ALR is bad" school: ALR is a waste of time and money because language is species specific: humans have it, animals do not. Animals will never learn
language and it is a waste of time to try to teach them, and we will gain nothing from this misguided endeavor.

Dichotomies are often useful for helping to organize ideas into two basic categories, and it seems to be human nature to do so, but all too often they are taken to extremes, as in this case. This dissertation will present an alternative approach, one in which ALR is viewed as a means of exploring where the human language faculty came from and what developments might have taken place in order for a language faculty to come into being. According to the present view, the goal in ALR should not be to teach language to animals, but to find out how language works-- what is required in order to learn language.

This dissertation will argue the following position: Not only is ALR not a waste of time, it can provide an important source of information about human language. ALR should be a vital area of linguistic research. By placing ALR in this position, the study of language will benefit from a new perspective, and ALR will benefit from the influence of current work in linguistics.

In the following chapters, this two-way exchange will be illustrated. I will show how comparative research using ALR can contribute to a better understanding of child language acquisition by allowing one to see the early stages of language learning from a new perspective; and how ALR can benefit by incorporating what we know from the study of creole languages. Some problems in current approaches to ALR will be discussed, and the path that future research should take will be laid out.
In this first chapter, some important issues for ALR are considered. The chapter begins by presenting three important questions which are often left unaddressed in the ALR literature, but which need to be considered by anyone undertaking such a project. From a brief overview of the issues, we will move to a more in-depth discussion of them.

Research over the past two decades has produced some noteworthy results in the area of animal language research. Although the researchers have attempted to cover all contingencies in terms of the experimental controls exerted in their studies, most have neglected some very basic questions about the prerequisites for language. This would seem to be a necessary area to discuss prior to undertaking a research project, merely because by addressing these issues it becomes more clear what the purpose of this research is, and how it should proceed.

Perhaps because of this avoidance of the basic issues, ALR researchers are also reluctant to reveal the purpose of their research--most are at best deceptive in their reports, claiming to be studying anything from social interaction to cognitive capacities, while blithely using complex linguistic terminology to describe their findings. They claim to be conducting comparative psychological research, while actually their goal seems to be to reach a point where they will be able to say that their animals "have language". Often, the very titles of their reports belie their claims of innocence: "Linguistic capabilities of lowland gorilla" (Patterson, 1978a); "Reading and sentence completion by a chimpanzee" (Rumbaugh, Gill & Von Glaserfeld, 1973), "Sentence
comprehension by bottlenosed dolphins" (Herman, Richards & Wolz, 1984); 
"Evidence for sentence constituents in the early utterances of child and 
chimpanzee" (Gardner & Gardner, 1975); "Language acquisition in apes and 
children" (Miles, 1976).

Basically, there are three questions which should be considered 
(not necessarily answered) before undertaking any such research project. 
1) Where did language come from? (i.e. did it arise out of earlier call 
systems?) 2) What are the differences between language and 
communication? And finally, 3) What, if anything, can ALR tell us about 
language development (ontogenetic) or language origins?

Although there may be no definition of language that is acceptable 
to all those currently involved in the ALR controversy (linguists, 
psychologists, anthropologists, and regular people) (if nothing else, 
ALR is showing us how difficult it is to define language), when the term 
"language" is used here, it will refer to systems governed by 
hierarchical structure, where symbols are combined to produce larger 
units, which are in turn structurally dependent upon each other. The 
linear strings which result from a translation of this hierarchical 
structure are then processable by both transmitter and receiver. The 
term will not refer to the communication that may take place without 
language. Undeniably, we use language to communicate, and the two 
systems are probably inextricably intertwined at this point in human 
evolution--most of us use a great deal of non-linguistic communication 
along with language when we wish to communicate our thoughts and
feelings— but in the discussion that follows, we will consider the two separately.

It is unlikely that anyone will be able to give a definitive answer to 1) above, but it would be nice to hear the opinions of those involved in ALR on the subject. This is not an uncontroversial point, although most of the animal language researchers choose to ignore it, apparently assuming that it is a given fact that language as we know it developed out of less sophisticated communication systems simply by some sort of complexification. In fact, Duane Rumbaugh (1977a) claims he is speaking for all when he says that the Gardeners' statement that "behavior that is at least continuous with human language can be found in other species" (Gardner & Gardner, 1971, p. 118) is "fundamental to the field of comparative psychology", and "is held in common by all investigators who study the potentials of the chimpanzee for language." (Rumbaugh, 1977a, p. 240). This relates of course, to the continuity question mentioned above. Apparently, according to Rumbaugh unless a researcher believes that natural communication behavior on the part of the ape represents an earlier stage of continuous development, he has no business doing animal language research. In fact, one does not need to begin a language project with the assumption of continuity, indeed, ALR may help us answer this very question.

The second important question raised above is: What are the differences between communication and language? Although there is clearly a distinction to be made, there is an alarming trend in ALR to assume that once communication between animal and teacher is
established, no matter what form it takes it can be called language. Researchers guilty of this leap (Gardeners, Fouts, Patterson) began searching for "communicative competence" (cf. Miles, 1976) when they were unable to demonstrate "syntax" in their animals' productions.3 Somehow, it seems that just those researchers who get emotionally closest to their subjects, (those who attempt to conduct research in as natural an environment as possible, even to the point of attempting to teach natural human language) are the ones who run into problems of overinterpretation based on their emotional attachment to their subjects. Being attached to an animal, to the point of being able to predict its behavior, can be an impediment in ALR: I also understand everything my dog wants and can predict and interpret her behavior, but that does not mean that what she is doing is language. Clearly, there is a need to distinguish between the kind of communication that goes on between me and my dog, and the kind of communication that is achieved through language.

Question 3) above is: What, if anything, can ALR tell us about language development and language origins? Judging by the paucity of linguists involved in such projects thus far, linguists have in fact not seen any reason to be a part of ALR-- has this been sore neglect, or justified rejection? Why should the linguist take an interest in this research? It seems as if researchers in ALR have tended to plunge headlong into the foray, without pausing to consider what questions should be raised, and which issues could be resolved by their work. Although many of these researchers are experimental psychologists, they
seem to have forgotten the fundamentals of their profession. One must first pose a question of interest and clarify all the underlying assumptions, and then go about designing an experiment to collect relevant data. The experiment must be designed in such a way as to provide data that will address the original question.

Having gone ahead on their own, without the aid of others outside their field, psychologists are now complaining that linguists have not been of much help (Premack, 1985a, b). But what are the questions being asked that are of interest to linguists? Partly through the fault of popularization of the field, the central question has become one of "do animals have language...yet?", and, contrary to popular belief, this is not a central concern for linguists. Linguists are interested in studying all aspects of language, from historical to psychological, to purely formal aspects, but not in arguing over who has it. Perhaps it is true that the popular press (and some researchers) have focused on this "intrinsically stupid, irrelevant, and actively misleading question", and busied themselves with "trying to get linguists to lower the target" (Bickerton 1981a, p. 217), but is this all ALR has to offer?

As a linguist who has been involved in an ALR project, I can say that despite frustration at the directions the field has taken, I continue to be interested in the field, and hope that interest will spread to the point where the questions asked will be of central concern to other linguists.

We return now to a closer consideration of the three questions posed above, beginning with the first question: Where did language
come from? As Bickerton (1981a) points out, most accounts of language evolution concentrate on the point of emergence of human language, to the neglect of what came before, and what happened after. It should be obvious that for our purposes (in ALR) what came before would be of particular interest, if only because language has yet to emerge in any of the animals studied. I believe a part of the problem is that the proper groundwork has never been laid, and without researching the foundation, there is little point in attempting to install some sort of language producing mechanism in an animal. But this is just what is being done. Where anthropologists are making the mistake of getting stuck at the point where language first emerged, and arguing what form it might have taken, vocal (Hill, 1972, 1974) or gestural (Hewes, 1973, 1977a, 1977b), in ALR researchers are concentrating on achieving that moment in animals. Once having gotten the animals to "say" something, they fail to follow up and recognize the distance between that and language as we humans use it, or for that matter, between that and what language might have looked like when it first appeared in our own species.

What we need to do instead is to pull back, away from language as it is today, back beyond language as it first emerged, and think about what the prerequisites for language might have been—what had to be in place before language could begin to develop. When we get back far enough, we can start to come forward slowly. Only in this way can we demonstrate that there is no "paradox of continuity" (Bickerton, 1981a) (that language must have evolved, like everything else, but at the same
time, language is so different from any existing communication system that it simply cannot have evolved from them). We will see that, as Bickerton explains, the apparent paradox is based on two false premises: 1) that language is a unitary phenomenon; 2) that language evolved out of prior communication systems. In fact, what I will propose is that communication is only a secondary use of language. The apparent paradox is there only when we compare language and communication systems, but what I am claiming is that language evolved as an integral part of our cognitive system, and the apparent leaps are actually logical outgrowths of combinations of cognitive abilities.

Let us begin at a very basic level, the level of our senses, which enable us to take in information about the world around us. We can see objects, feel their shape and what they are made of, smell and taste them, and hear them. Although normally we think of "perception" as referring to vision, let us extend the idea somewhat here to say that the various bits of information we receive from all our senses enable us to form a percept of whatever object we are examining. Where do these percepts go from here? In order for them to be useful to us in any way (one must be able to recognize a familiar object when one sees it) they must be stored away in memory for later reference. Further than this, we need to look at just how they are stored. There must be several levels of storage, from the level of the feature, or smallest bit of information perceived, all the way up to networks of percepts which form concepts.
The reader will notice a plethora of terms familiar from the study of cognition, and this is not an accident. What we will see here is that language could have evolved within cognition as a way of organizing thought. Therefore, speaking about information gathering and processing is only appropriate. The point is that language is not a unitary phenomenon since the capability for language is the result of the intersection of several component faculties. Yet since what is envisioned here is one point of intersection of such varied components as the particular arrangement of human memory and organization and processing of information in the brain, then that point of intersection would have produced one kind of language-- human language. So although the antecedents to language may have been many and varied, including the ability to create concepts from percepts, to distinguish real from imagined, to distinguish between a state and an ongoing process, to determine the order of events in ongoing and past action, to use symbols to refer to objects and events in the real world, and to organize information in a systematic way, the particular point of intersection of those capabilities could only have yielded one product: human language, and each member of the species would arrive at the same product, just as each member of our species develops the same kind of heart and lungs.

Perhaps it is necessary to say something more about the difference between a percept and a concept. As Bickerton (1981a) notes, "percept" and "concept" are so often used in everyday speech that we tend to lose the important distinction between them. Percepts stand alone as the direct input from our information gathering systems. As I sit here
looking out my window, I perceive a tree. After a certain amount of processing of the information that allowed me to form the percept "tree" from the combination of green and brown colors, curved and straight lines, swaying motion and rustling sound, I may arrive at my concept of a tree, or trees in general as a class of objects that grow long limbs and have branches and leaves. Some produce fruit and and so on and so forth, connecting into various networks of stored information which allow me to distinguish a tree from a bush, from the boy across the street waving his arms. This ability to distinguish the percept of a specific tree from the general concept tree is so basic, that it is not at all surprising that Bickerton has found the specific/non-specific distinction to be a fundamental one in language.

The move toward language capability (and from language emergence onward to further linguistic development) would most likely have been interactive. Capabilities which aided in survival survived to be passed on in the genetic pool. At each stage we need to ask not only where such a capability might have come from, but what it would have "bought" us. The ability to manipulate images at will allows us to play with time as we please. We can sit here right now and roll ourselves a screening of the deer we saw drinking at the water hole yesterday, or place the deer there for us to find tomorrow. We can juxtapose images that were not originally perceived together, so we can include ourselves in the picture with the deer. We can think about the past and the future, and based on our past experience even plan the future. The ability to juxtapose images at will (or juxtapose features, creating
entirely novel images) enables us to consider the past and ponder the future. Based on past events we may even make predictions about the future—predictions that cause us concern. If we have had trouble catching a deer at the water hole before, we may predict difficulty in the future. It seems to be human nature to desire to be in control—of our lives, of those around us, and of our future.

It may be possible for animals to store images of perceived objects and events in memory, but could the sort of "screening" described above be possible without a system for organizing and labelling images? It is difficult to imagine how one could manipulate images and create new ones without some sort of language capability. What is suggested here is that a language capability developed interactively with the ability to form representations in memory, in a sort of double helix, with each capability feeding into the other. The organization of information that the beginnings of a language capability would afford would make image manipulation possible, perhaps first at the level of objects and events actually perceived. This, in turn would stimulate further development of a language capacity, which would allow a second stage of image manipulation in which new images could be created, and so on and so forth.

Language may have been a useful tool for organizing and expressing one's thoughts to oneself, but whenever we (whatever stage of development we were in at that time) began to live and hunt together in groups, this capability would have come out of internal use as an indispensable tool. There is power in numbers, but the only way to
organize the numbers and harness the power to control events is by communicating the very thoughts and concerns of the individual discussed above, and language makes this communication possible.

Communication by definition evolved as part of the social development of getting along in groups. Precursors to language began to evolve in each member's mind, only later to emerge to be shared by members of a group. The particular sound-meaning relationships could only be worked out, through mutual agreement as it were, "let's call this a 'pug'", but language could not be used for communication until it emerged among conspecifics in order that members of the group arrive at mutually intelligible forms.

We have come full circle and returned to communication, but from a different direction. This is communication with language, and what is being communicated are thoughts and concepts as opposed to stimuli and percepts. Language eventually took on a communicative use, but its primary function was, and still is as a tool to organize storage and manipulation of images and concepts in memory.

Language did not replace basic "survival" communication, which remains as a separate capability to this day. As Hewes (1976) points out, spoken language is localized in an entirely different portion of the cortex from where we find the natural calls of non-human primates, "but the old limbic controls for involuntary human cries still exist" (p. 490). Hewes would take this as evidence against the vocal theory of language evolution, in support of his gestural origins theory: "if spoken language had evolved directly out of the primate call system, one
would not expect it to have become localized in an entirely different portion of the cortex" (p. 490). This is precisely the sort of evidence we need against the premise mentioned at the outset, that language evolved out of prior communication systems. If language had evolved out of prior communication systems, then we would expect it to replace them, or at least be localized in the same area of the brain. Yet we find the two systems localized in entirely different portions of the cortex, which indicates that they are not related, and most likely never were.

Language and basic communication evolved separately and remain separate to this day. Not only are these systems localized to separate areas in the brain, the sounds produced by each are not even in the same frequency ranges. While the cry of a newborn human infant and that of a baby chimp may be similar in range, both differ from the vocalizations produced in speech (Lieberman, 1975). The basic communication signals are still available to us, and in fact used involuntarily— we cry out in fear or hunger, bellow in anger, moan in pain and anguish, squeal with delight, sigh in contentment and yawn audibly in relaxation, just like other animals, and language has nothing to do with these non-linguistic sounds.

If language did not develop in stages from less sophisticated communication systems, then there must have been a jump someplace along the line from those to the first hierarchically structured symbolic language system. Perhaps we humans made the jump, leaving nonhuman primates on the other side. In that case, in attempting to teach language to animals we may be able to establish symbolic systems, but it
will take a big jump to get a system comparable to human language with
the hierarchical organization and structural dependency characteristic
of it.

My hope is that by standing as it were, on the far side of the
river, as close to the bank as we can get, we may be able to discover
something of what is involved in crossing the gap. If we can bring
animals to the limits of their capabilities we may be able to see if
what they are doing has anything to do with the precursors of human
language, and if so, what must have taken place in our evolution to
enable us to come so much further in our language capability.

Lenneberg (1967) recommended against attempting to discover
anything about language origins by studying animals, but he was
concerned with those who would look for the roots of language in the
communication of animals, particularly "if the basis of comparison is
pragmatic or 'logical' and without regard to the animals' phylogenetic
relation to man." (p. 265). The position presented here is in complete
agreement with Lenneberg "that there is every reason to believe that
animal communication is a discontinuous affair and that logical
commonalities among communication systems are not necessarily indicators
of a common biological origin." (p. 237). Lenneberg argued that it is
unreasonable to search for the "antecedents of the human propensity for
language" in animals because "no living animal represents a direct
primitive ancestor of our own kind and, therefore, there is no reason to
believe that any one of their traits is a primitive form of any one of
our traits" (p. 234-235), but at the same time he points out that "the
range of possible antecedents is vast" (p. 235). My point here is that it is just this fact which makes ALR a reasonable endeavor. If the antecedents to language include such prerequisites as the ability to form general concepts from specific percepts, categorization, recombination, reference, use of reference to achieve displacement, and so on, then it is perfectly reasonable to construct experiments within ALR to look for these antecedents-- not in an attempt to prove that animal communication represents an earlier stage of human language, but in order to discover just which antecedents of language are common to humans, non-human primates, and perhaps other mammals, and which are specific to humans. Since the only kind of natural language we presently recognize is human language, then this is parallel to saying that we are trying to find out which antecedents of language are part of general cognitive abilities, and which are specific to language.

This is just what was meant above when it was said that the proper groundwork must be laid and the foundation researched. We need to think about what the prerequisites to language might have been and look for those in animals, not try to teach apes to ape language (Sanders, 1985).

We return now to the second basic question posed at the beginning of this chapter: What are the differences between communication and language? Most early attempts at illustrating the difference between communication and language were bent on building the user into the system. That is, one of the defining characteristics of language was that humans use it. (Such an approach defeats the purpose in the case of ALR). Thus Hockett's (1960) design features of language were so
specific that even some humans (those who do not use the vocal-auditory channel) were excluded from "having language". In keeping out the animals, Hockett's original formulation allowed gibbons to attain all but the last elusive three design features: productivity, traditional transmission, and duality of patterning. Hockett has since revised the original list (Hockett & Altmann, 1968), and now deaf individuals are conceded language too, but in the meantime, the animals have been gaining as well.

Researchers in ALR have been determined to demonstrate that their animals can acquire any feature once considered uniquely human and specific to language, and for the most part they have been successful in unwittingly demonstrating the weakness of an approach in which language is treated as a list of features, rather than as an integrated system. Lists of features and characteristics may be useful as guidelines, but in general they have not been used only as such. The drawing up of lists has been akin to the lining up of armies, and has only served to further fuel the controversy between pro-ALR and anti-ALR positions. Armed with a list of his own (comprised of behaviors he has trained his animals to perform) the psychologist in ALR has tried to shoot down every soldier on the language list, placed there to protect language faithfully from the wild animals without. Again and again, as animal language researchers achieve each new objective, new soliders spring into the place of their fallen comrades. What has become clear is that the psychologists may kill all the soldiers, but they have yet to capture the flag. There is no way merely to list the necessary
ingredients of language, and, as Premack has discovered, one cannot break down the workings of language into the basic elements using a functional analysis approach (Premack, 1970) and then put them back together, installing them one by one in an ape (Premack, 1976). Lists are only useful if the researcher is willing to get to the bottom of things and can understand what is behind the superficial differences. One has to look for the underlying capabilities in animals-- not just try to achieve some sort of language-like performance on the surface.

How then, does one differentiate between communication and language? There is no single criterion to use as a measuring stick with which to assess a situation and determine, "aha, what we are dealing with here is communication and not language at all", instead, it is more a constellation of integrated qualities and characteristics which sets language apart. Perhaps all differences can be reduced to a question of content and form.

It is both what language can do and the formal mechanisms by which it is done that set language apart from communication. With respect to the former, Jacob Bronowski (1967) zeroed in on one important difference between communication and language: language allows one to express messages devoid of affect. If I want to communicate that I am angry I may frown and thump my fist on the table. At the same time, I can merely use language to say that I am-- or, even better, was angry, because the distancing from the emotional content of the message seems to have led to the ability to distance oneself from place and time as well. Words continue to have meaning even in the absence of their
referents. The best evidence of this referential quality of words is in displacement. Displacement works in both directions. In describing an event that took place in the past, we are able to evoke the very emotions felt when the event took place. Or, in describing a future event we may experience anticipatory emotions. In one sense then, displacement can be thought of as an affect switch. Language contains that switch, and the option to flip it, but communication does not.

Displacement is evidence of the ability to use a symbol to represent a referent when the stimulus is not immediately available (by available, I mean visible or otherwise tangible, by immediately, I mean it is simply not present, has not been recently, and there is no reason to expect it to be arriving soon). Displacement is the combination of symbolic representation plus distance (time/place), and as such is the best available evidence of the referential quality of symbols. Displacement allows the experience of the individual to be shared by many. The separate members of a group need not all experience being in a particular place, say, a shelter, or a place where food can be easily obtained, in order to understand one individual's description of such a place, go there, and recognize it when they arrive there. Displacement carries with it obvious survival advantages.

We can see that displacement is a logical outgrowth of a language capability which allows for symbolic representation. When abstract arbitrary symbols can stand for concepts and objects in the real world, one can refer to events and the objects involved in them, and even to one's impression of the event--all this at a time and place different
from that of the events themselves. Displacement is obviously only possible with the development of long term memory, and the ability to imagine things that may not exist at all, but long term memory alone does not yield displacement.

Displacement has been the focus of discussion in recent ALR projects, but none of the researchers seem to have discovered a procedure that can actually test the animal's ability to refer to objects which are not present. Perhaps this is because displacement and reference are inextricably intertwined. It is impossible to get displacement without reference-- how can one refer to objects that are displaced unless one is able to refer in the first place? At the same time, there is a difficulty in distinguishing between true reference and things that look like reference in ALR because it seems impossible to demonstrate reference without using displacement to do so. How can we show that when an ape produces "give orange" the symbol "give" actually represents the action, and the symbol "orange" refers to the object orange? The ape may have merely learned to associate the symbols with the outcome of receiving an orange. The same problem occurs in comprehension. If we tell a dolphin to jump over a ball and it does so, how do we know that "ball" actually represents ball for the dolphin? It is not enough just to demonstrate varied functional use of the symbols (Savage-Rumbaugh & Rumbaugh, 1978; Savage-Rumbaugh, Rumbaugh & Boyson, 1980). Although we are capable of conceiving of reference and displacement separately, it is difficult to separate them experimentally. It is impossible to refer to things not present without
reference, yet it is impossible to prove that A can represent B unless B is not present at the time. As long as A and B are both present we cannot show that the relationship is one of reference or representation as opposed to a paired association. Attempts at establishing displacement in animals involved in ALR projects are reviewed below.

Herman et al. attempted to demonstrate displacement in their dolphins (Herman, et al. 1984) using the following procedure. Normally, the dolphins are given commands which require them to take simple actions (jump over, swim under, fetch, etc.) to objects floating about in their tank. On "displacement trials" these objects have been removed from the tank, and are tossed in simultaneously from "random" locations around the tank only after the command sequence has been given. Herman et al.'s attempt to demonstrate displacement in this way has shown nothing more than that the dolphins have the ability to hold a simple command in memory for a brief period of up to about 30 seconds. Essentially, the dolphins are merely withholding a simple physical response until given the go ahead. This is a far cry from displacement, and is, in fact, simply the wrong way to approach the problem.

Savage-Rumbaugh and her colleagues (Savage-Rumbaugh, Pate, Lawson & Smith, 1983) have used a better approach. As a first step in "briding the gap from the immediately present pointable world to the removed 'remembered' or 'imaged' world of time and space" (p. 468), they used color video with their chimpanzees. The chimps Austin and Sherman could see things on the video screen which were located elsewhere. The task required that they request these objects, which would then be
brought to them in a matter of seconds. Supposedly, their ability to request objects on the basis of viewing their video image demonstrates displacement. There are several problems with this method. First of all, how do we know the chimps were actually requesting the objects? Perhaps they were merely indicating them on the screen, with no expectation of receiving them. How do we know they know the object is not actually on the screen? The chimps may actually use the video images to represent the real objects, but like the dolphin study, the time/distance ingredient is missing. We have no reason to believe that they are referring to objects which are not present. Perhaps Sherman and Austin were really requesting the objects, but then they only had to wait a matter of seconds before receiving the requested object, which they would learn would soon follow. Again we are dealing with something other than displacement. The chimps are able to press a key on a computerized keyboard in response to an image which appears on a screen. This may be one way to test the ability to use symbols to represent objects in memory, but the design of the present experiment does not isolate this sufficiently.

Premack (1976, Premack & Premack, 1983) has gone much further in the right direction, which is to study an animal’s ability to use a cognitive map before attempting to teach that animal to use symbols in that mapping. In one experiment the chimp is shown a miniature representation of a real location in the form of a dollhouse room. The chimp is allowed to observe while the room is "baited"—food is placed somewhere in the dollhouse room. Later, the chimp is taken to the real
location where his task is to find food he saw being placed in the
dollhouse room. As opposed to the Savage-Rumbaugh et al. experiment,
in which Sherman and Austin might be requesting objects without any
conception of the fact that what is on the screen is meant to represent
a remote location, in Premack’s study, either the chimp must realize
that the dollhouse represents the real room, or he will not have much
luck finding the food. According to Premack’s report, their success
rate is not high.

In another experiment (Premack & Woodruff, 1978a), which truly
incorporates the element of distance, the chimp Sarah viewed video tapes
of people with a variety of problems (unable to reach food; shivering in
a room with the heater turned off; locked out of a room; etc.) and had
to choose an appropriate solution. Should Sarah’s ability to choose the
right answer (by indicating one of several photographs of possible
solutions) be interpreted as a demonstration that she can view a video
image as a representation of the real world, and can manipulate a
problem without the elements which make up the problem being present?
Savage-Rumbaugh and Rumbaugh (1979) think not. They remark that
Premack and Woodruff do not present any evidence, independent
of Sarah’s choice of photographs, to indicate (i) that she saw
the videotaped sequences as problems to be solved, (ii) that
she perceived her choice as representing a solution to the
portrayed problem, and (iii) that she understood either the
elements of the problems or the nature of the solutions or if
faced with the problem that she could solve it herself. (p. 1201)
Lacking such independent evidence, the Rumbaugh's argue, there is no way to determine that Sarah's choices of "solutions" were actually solutions, and not merely based on match-to-sample strategies, none of which would require an understanding of either the problems portrayed in the videotape or that her choices represented solutions. (p. 1201).

Premack's earlier work with Sarah perhaps came closest to examining displacement capability in the chimpanzee. In the feature analysis study described in Premack (1976), Sarah was able to "describe" objects which were not present using symbols for attributes which she had learned. The drawback of this (and some of Premack's other designs) is that it must be extended beyond a two choice situation (Apple: is it 1) red or 2) green?) in which the subject is assured a 50% chance of being correct on any one question, and which may in fact encourage a wrong approach in the animal. Another problem which must be avoided in a feature analysis experiment is the possibility of the animal learning the attributes in some kind of stimulus-response (S-R) chain, triggered by appearance of the target object. Both of these problems are simple to overcome once one is aware of their existence, and the approach is worthy of application in other projects.

For example, Schusterman's sea lions (described in Schusterman & Krieger, 1984) now have three different attribute symbols in their vocabulary: color (black/white), location (on land/in water), and size (large/small). It would be interesting to see if they could be taught to use these symbols to describe an immediately present situation, for
instance, the presence of the small white ball in the water. Or could they describe objects which are not present?

To summarize briefly here, if the first important difference between communication and language is found in the content of what is expressed, then displacement is what makes all the difference in that content. Displacement allows: 1) separation of affect from description of the event, 2) separation of body in time and space, 3) separation from the real world. I believe that any differences in content can eventually be traced to the existence or absence of displacement in the system.

The other main difference between language and communication is form. If displacement was the big jump in content, then hierarchical structure is the key here. Not only does animal communication lack displacement, it also lacks syntax. Contrary to popular belief, but known to any linguist, syntax neither means word order, nor need its application be limited to language. A grammar of a factory, for instance, might include an inventory of stock; the syntax would tell us which items belong together; and finally, how to put them together. Or perhaps a better example might be a recipe for baking cookies. First we need a list of ingredients, including dry and wet types. Second, we need to know, for example, that the butter and sugar will be creamed separately as a unit. Finally, the recipe tells us that the way to put the ingredients together is to add the eggs to the sugar/butter mixture, the flour to that, and that the chocolate chips are only added at the very end when the cookie mixture is done. So an absolutely minimal
grammar of a language would have to include at least the following: a list of building blocks for that language (classes of lexical items); rules of coocurrence (for example, that adjectives go with nouns, and not prepositions); and finally, the information necessary for putting them all together: rules of order and movement (for example, in English the adjective precedes the noun; and in every language there are rules as to what constituents can move where in the sentence and how).

It is this building-block characteristic of language which sets it apart from communication. Language makes use of a system of hierarchies: phonemes are put together to form morphemes and words, words are put together to form phrases, phrases are put together to form sentences, and sentences can be put together to form conversations, or paragraphs. Several levels are active at once, and it is always possible to add new elements at the base structure level, which are integrated immediately into the system and used productively.

Cheney and Seyfarth (Cheney, 1984; Cheney & Seyfarth, 1982; Seyfarth, 1984; Seyfarth, Cheney & Marler, 1980a; Seyfarth, Cheney & Marler, 1980b) in their studies of the alarm calls of vervet monkeys have found that the calls are not as limited in scope as researchers had previously thought. These researchers have recently discovered that specific calls of vervet monkeys are correlated with particular flight behaviors in conspecifics, followed usually by the appearance of a specific threat. That is, there seem to be specific calls for when the threat is a leopard, an eagle, or a python, and other monkeys respond to these vocalizations with appropriate escape strategies: scampering up
trees in response to the "leopard-warning", darting into bushes in
response to the "eagle-warning", and scanning the ground in response to
the "python-warning" (Seyfarth & Cheney, 1982).

It has been suggested that this behavior be interpreted as evidence
that some vocalizations of non-human primates in the wild may function
in an analogous manner to human symbolic use of words (Seyfarth &
Cheney, 1982; Snowdon, 1982), but are these screams really referential
as are words in human language? Do the monkeys, thus warned, carry
with them in their flight a concept of "eagle", or "flying predator",
complete with images of impending attack, talons ripping at fur,
screeches of terror, blood flowing, and so on?

At present, a more parsimonious explanation would be that the
warning monkey perceives the threatening predator and screams. Not a
word-like referential scream, as in "Help! Eagle!", but a scream
associated with that particular situation. This triggers the
appropriate response in other monkeys which may not have seen the
danger. It should be stressed that a sound may be paired with a
stimulus without coming to represent that stimulus symbolically. For
example, I may scream explosively when terrified of an intruder, and
inhale audibly in surprise when I drop a pot of boiling water. Each of
these sounds is associated with the particular stimulus, but not
necessarily exclusively, and certainly not as a referential symbol.

It is doubtful that these calls of the vervet monkeys are truly
part of a productive hierarchically structured system in the sense
discussed above. Can they be broken down into separate recombinable
elements, and can those elements once put into new combinations be understood immediately? For instance, if a new sort of threat happens along the trail can one vervet monkey name it and will the name be understood by the rest of the group? This would truly be impressive.

Herman et al. (1984) and Schusterman (Shusterman & Krieger, 1984) working with marine mammals have been able to demonstrate this kind of generalization to a certain degree, but only in the comprehension of commands. Both teams place particular emphasis on training for this kind of comprehension, and it seems to have paid off. Herman's dolphins demonstrate a certain degree of accuracy when presented with new commands made up of old lexical items, or when a new symbol is put into an old frame. The fact that Herman has not correlated the dolphins' responses to new commands with their previous experience with the symbols in the commands or the objects they represent makes it difficult to interpret their ability to respond correctly to novel commands. It would be interesting to see if prior exposure to a symbol or object is a good predictor of accuracy in novel commands.

This is a unique opportunity that researchers have with animals that is not usually available to researchers of child language acquisition, in that in both the Herman and Schusterman projects the researchers have records of all of the input to the animals. In our research with Seth here at the University of Hawaii (see Chapter III) we are tracing the emergence of syntax from the age of one and a half to approximately two and a half years of age. It is only because we have an hour per week of audio tapes of Seth interacting with his father that
we have been able to note the vast importance of the father's input. This is only one hour per week of the child's life. In ALR we have records of every interaction. It is a shame not to examine the data more carefully.

In the preceding sections, we have been able to delineate some important aspects of language which set it apart from communication, but it remains the case that the whole is simply not merely the sum of the parts. True, the form and content of language are different from communication, and I have attempted to provide some of the details as to what these differences are, but training these aspects separately and putting them together will not yield language. This is precisely the problem with drawing up lists mentioned earlier. It almost seems as if there is a mysterious chemical reaction that takes place when these elements are combined and left to incubate in the human vessel. But perhaps there is no mysterious chemical reaction. Perhaps there is simply a missing ingredient in animals. If Chomsky (1980) is right, there certainly is a missing ingredient— the main one: a language organ. Without this organ there is no hope of ever acquiring language.

Before returning to our third question, it might be a good idea to recapitulate here. In asking where language came from, we were led to a need to differentiate between language and communication. We found that the main differences can be reduced to those of content and form, and these in turn boil down to reference and syntax. If we can explain where they came from, we will have a better idea where language came from.
What does one need to get reference? Imagination, long term memory and the rich conceptualization system that they allow; a system for orientation in time and space, and a system for organizing these things, that is, a "syntax" for reference/displacement. What does one need to get syntax? To begin with, the brain must have the neural connections necessary for such a hierarchical organization of memory storage. But there must also be a use for syntax-- there must be a sufficient amount of information present, and requirements for the processing of that information such that there is an advantage in having a system to organize the information efficiently. This system would enable the user to relegate simple syntactic manipulations to automatic processing, and leave capacity free for research and development as it were.

Thus syntax could have evolved as follows: a massive increment in neural pathways created a processing capacity capable of handling expanding information storage. As with any evolutionary change, the processes of natural selection allow some variations to remain while others disappear. In this case, the change remained because an enhanced processing capacity was advantageous to the survival of the species. Once the processing capacity was available, information could be stored and manipulated more efficiently, allowing for further expansion of storage capacity.

How would reference and syntax interact in their development? As mentioned above, reference/displacement requires an organizational system of its own, and further, the growth of information that needs to
be stored would be greatly increased by a displacement capacity. Once the neural connections for syntax were in place, this would allow for the development of the syntax for reference, and could in this way handle the increase in information storage and manipulation necessary for displacement. As we have just said, in order to get displacement one needs a rich conceptualization system and a system for orientation in time and space. Syntax would allow displacement to develop by enabling the intersection of the orientation system and the conceptualization system to form a displacement component.

There are many questions that have yet to be answered about "animal cognition" (see Roitblat, Bever & Terrace, 1984 for a recent consideration of such questions), and researchers can address these questions through their work in ALR. But what do researchers want from the animals in their ALR projects, and what in fact is being asked of the animals in terms of evolutionary leaps? Is too much being asked of them? It would seem that these researchers are asking animals to achieve through training what our own species only achieved through countless years of evolution. They would do better to look first at where these animals' evolution has taken them and what they can do with what they have than to require of them that they take an immense evolutionary step in our direction.

Returning finally to the third basic question: What might ALR tell us about language ontogeny or phylogeny; what is there to interest the linguist in the field of ALR and what benefits might there be for linguistics? Chomsky (1979) likens the study of apes to find out about
human language to studying human broad jumping in an attempt to find out about bird flight. Is it really so far fetched? Perhaps the most general question that intrigues people about ALR is the question of the species specificity of language. Are we humans the only creatures who have language? How does our currently existing non-definition of language have to be modified in light of recent achievements of animals involved in ALR projects?

Originally, I too was drawn to the field of ALR out of curiosity about the question of specificity. The more one learns about language, the more wondrous it seems, and the more difficult it becomes to imagine life without it. As linguists, we are interested in language because there are intriguing patterns. We are interested in just those phenomena for which there is no immediate logical explanation—those things that did not have to be the way they are, but conform to certain rules nonetheless. We look for patterns, and when we find them we describe them, and hope eventually to explain their bases.

Are we the only ones who have language? One soon tires of arguing who has got it, especially since we have no definition of "it". Rather, one should ask how we got it, and since language is not a unitary phenomenon, what the different parts are and what different combinations of these parts might yield. I have no desire to tutor the animals. Dr. Herman speaks of "educating" his dolphins. I hope, instead, to find out how they use their cognitive tools. Do they have tools similar to our own that they use differently? Has the evolutionary route that they followed caused them to develop as great an information processing
capability as our own and apply it elsewhere, or even in the same place, i.e. linguistically?

Chomsky (1980) has noticed a clustering of beliefs: those who believe that language is innate tend to adopt a modular theory of language, i.e. that language is made up of a set of intertwining and integrated but nonetheless separate modules; whereas those who believe that language is a learned skill with nothing given at the outset tend to think of language as a unitary phenomenon. Where do animal language researchers and their opponents stand on these issues? Generally, they fit right into the scheme. Those who believe that animals can learn language consider language to be a single entity, a wholly learned skill, with no innately endowed component. Those who do not believe animals can learn language tend to cluster with the nativists, assuming not only that the ability to acquire language is innate, but that only humans have this innate ability because it requires some module or modules which animals lack.

There is an additional position between these extremes which must be considered. It may be that the ability to acquire language is part of the genetic make-up of humans, but that language is a product of the intersection of several modules, not all of which are specific to language, and some of which may be common to animals as well as humans. These modules can be thought of as molecules in a chemical compound. There may be a difference of only one molecule, yet the resulting compound is an entirely different substance, in this case, language in humans. This is the quality/quantity paradox encountered
earlier: do animals lack certain components, or simply have a
different way of combining them (or both)? ALR does not have to be
divided along the innate/learned boundary: the finding that apes can
learn some aspects of language does not require assuming that nothing
is innate, but it might require a re-examination of models of language
acquisition, as we shall see in Chapter III.

Through the modular approach we may be able to identify the general
cognitive as opposed to the specific linguistic modules by isolating the
former in animals. It is more informative to say the the "computational
component" (Chomsky, 1980) (better yet, part of it) is unique to humans,
and to begin to answer why, than to have to say something so general as
"language is unique to humans". It is not enough to know that only
humans use language. I want to know why, and for that matter,
how and what if.

According to Chomsky (1980) (whose views have been strongly
influenced by the work of Lenneberg on the biological foundations of
language), Universal Grammar is a mental organ. Did this language
faculty spring at once, fully blown? It may be that it did emerge
somewhat suddenly as the result of a combination of properties, but the
question is, what were the "molecules"? There may be specifically
linguistic developments that the human mind underwent, but there are
probably some capabilities that are required for language that are
shared by several species. Through description of animal capabilities
we may begin to arrive at explanations of our own capabilities. ALR
needs to concentrate more on how animals do what they do than on
training better performance. Researchers tend to lose track of the goals of the research (or perhaps some have different goals from the start). They try to correct errors in their animals' responses with "remedial training" when these very errors may provide crucial information as to how the animal is processing the input. Errors should be treated like little windows into the workings of the mechanism-- not blemishes to be covered up and done away with.

Research on aphasics and cases like that of Genie (Curtiss, 1977), a child who was both traumatized psychologically and deprived of linguistic interaction until she was discovered at the age of 13, may also help us to define the modules that comprise a language capability. There are many unanswered questions, for example, aphasics may be able to think without speech, but is it without language? How is it that aphasics with severe language deficits can use systems like those that have been taught to apes (Davis & Gardner, 1976; Gardner, Zurif, Berry & Baker, 1976; Hughes, 1974; Velletri-Glass, Gazzaniga & Premack, 1973)? And how was Genie able to acquire a certain amount of language after the age of 13? These cases provide the basis for the idea that not only may there be separate components that make up a language capability, but perhaps there is a "language faculty" and a "conceptual component" (Chomsky, 1980). The conceptual component alone may not be at a total loss when confronted with language data, which would explain the abilities of aphasics and the accomplishments of Genie. What implications does this have for ALR? Can we say that apes are like humans without the language faculty (Chomsky 1980)? And what about
marine mammals? Chomsky (1979) wonders what significance any of these analogies might have. The answer is that by studying how apes acquire the systems taught to them we may be able to isolate just what capabilities are specifically linguistic, and what may be handled by a more general cognitive capability. Perhaps Chomsky finds the analogy of little use because for him it is enough to assume the existence of a language faculty which only humans have, period, without looking for evidence of what it is made up of, how it works, and where it came from.

Aphasia research is one area where we may look for analogies; another fertile region is child language acquisition. (This subject will be dealt with later in Chapter III). The more closely we examine the acquisition process in children, the less magical acquisition seems. In a project at the University of Hawaii (mentioned earlier) data have been collected on the child Seth on a weekly basis from a period of one to four years of age. The kind of closely spaced examination which this makes possible (many child language studies look at the child in monthly or several month increments) begins to approach the kind of thoroughness and continuity found in some ALR data collection. The more we look at the child data, the more it seems that up until about two to two and a half years of age the child's verbal behavior, particularly in form, if not content as well, looks an awful lot like the apes'. After this age, the similarities disappear. Is this change really sudden? Or is it more like the sprouting of seeds which have been germinating until then? If Chomsky (1980) is right, and language simply grows in the human
individual when provided with "sun and water", are those who would attempt to instill language in animals sowing in infertile soil?

By grouping together different sources of information, data from aphasics, children, second language learners, pidgins, creoles and ALR, similarities may be revealed that would otherwise remain obscure. By examining these similarities we can see new connections, which lead to new questions, and eventually, one hopes, some answers.

What is broad jumping going to tell us about flying? That all depends on how capable one is of making connections. In the particular case of human broad jumping versus avian aviation, we know the behaviors are not homologous, and in fact not really analogous either. But what about a case in which we do not know the origins of the behavior, something like the apparent deceptive behavior by baboons (Kummer, 1982), or by Premack’s chimp Sadie (Premack & Woodruff, 1978b; Woodruff & Premack, 1979)? Is this behavior homologous to deception in humans, as when Daddy demands of Seth, "Did you toot?" (i.e. "fart"), and Seth replies with an emphatic "No!"? Perhaps physical and physiological similarities are easier to trace, and thus the example of broad jumping/flying can be easily dismissed, but in the case of some of the capabilities demonstrated by chimps in ALR programs, dismissal should not be so quick. Determining whether the behaviors are analogous or homologous to similar behavior in humans will be very informative and helpful in understanding where human language came from, and how it got where it is today. The first step is to determine if these behaviors are even similar on the surface. Then we can start to ask if this is
due to convergence under similar pressures, or due to homologous evolution.

Why should the linguist spend time considering ALR? Perhaps it is better not to ask why, but which linguist should take a closer look at ALR. My answer is: the linguist who is interested in going beyond the mere characterization and description of human language to the level of explaining and understanding why human language is the way it is and how it got to be that way.

The following chapters will present a critical review of some of the recent research in ALR, putting it into the larger perspective of the questions asked in this chapter: 1) where did language come from; 2) what is the difference between communication and language (and why what the animals in ALR projects are doing can not be considered language); and if they are not doing language, then 3) what does ALR have to offer in the way of contributing to our knowledge of the phylogenetic and ontogenetic origins of language?
Chapter I Notes

1. The title "Animal Language Research" implies that these people are studying animal languages-- that is, studying existing languages, as in, "Semitic Language Research", whereas in most cases the researchers are attempting to teach the animals language, either natural (human) or artificial.

2. If the former were true, that language is the result of gradual complexification of call systems, then this would support an argument for continuity. If there were some sort of inexplicable gap between this sort of communication and language, then a hypothesis of discontinuity would be supported.

3. Again and again in the pages that follow we will see that most researchers in ALR incorrectly assume that "syntax" is synonymous with "word order". Even using this incorrect definition of syntax these researchers were unable to establish regularity in their chimps' use of word order.

4. This, and other procedures used with the dolphins will be discussed further in the chapter on marine mammals.

5. If it were that simple, that apes were just like humans deprived of their language faculty, the implications for aphasia research would be fantastic. Animals could be used as the model for aphasia research and testing in the laboratory, and the results could be applied directly to human patients in the clinic. Obviously, the situation is not so simple, but the analogy is nonetheless helpful.
CHAPTER II
AMERICAN SIGN LANGUAGE AND APES

The purpose of this chapter is twofold: on the one hand, I will be attempting to dispell the myth that any animal has acquired American Sign Language (ASL) and uses it in its communication—whether with humans (Fouts, 1973, 1974, 1975, 1977, 1978; Gardner & Gardner, 1969, 1971, 1975, 1980; Miles, 1983; Patterson, 1978a, 1980a, 1981; Terrace, 1979), or with conspecifics (Fouts, 1983, Fouts, Hirsch, & Fouts, 1982; Fouts, Fouts & Schoenfeld, 1984; Gardner & Gardner, 1985). The second purpose of this chapter is to plant the seeds for an idea which will be elaborated upon in Chapter VI.

ASL is a language used by many deaf individuals in the United States. Nearly a decade ago, it was estimated that there were approximately 500,000 deaf users of ASL in this country (Wilbur, 1979). ASL is a bona-fide language, with the grammatical complexities and expressive capability of any spoken language, but it was not always considered as such. The road towards acceptance as a "real" language has been long and hard, beginning only as recently as the sixties, with Stokoe's seminal work (Stokoe, 1960; Stokoe, Casterline & Croneberg, 1965), and followed by no less important contributions by the group at the Salk Institute and their colleagues (Klima & Bellugi, 1979).

What these studies revealed is that ASL is very similar to spoken languages, and at the same time different from them. It is similar in that the same processes are in evidence, yet the mechanisms by which they are carried out are different.
In spoken languages, "phonology" is the term used to refer to the system of sound units which are the building blocks of words. ASL has an analogous system, first proposed by Stokoe (1960) when he analysed the form of ASL signs into three separate parameters: 1) handshape or configuration, 2) place of articulation, 3) movement. Stokoe and his colleagues (Stokoe et al., 1965) used these parameters to create a dictionary of signs in their citation form. Several researchers have since argued that an additional parameter, orientation, is necessary for a more precise and accurate description of ASL (Klima & Bellugi, 1979; Battison, 1974; Freidman, 1975; Frishberg, 1975). It is these parameters of sign which are combined simultaneously to produce the words of ASL. This concept of simultaneity in signing will be discussed further below, and as we shall see in the section on ALR, it has been thoroughly misunderstood by some of the researchers of animal language capabilities.

It should be noted that although given the components described by Stokoe and others the number of possible signs which can be produced may be infinite, just as in spoken languages, there are limitations on the forms words can take in ASL. Various constraints on the formation of signs in ASL have been proposed by Battison (1974) and Frishberg (1976). Siple (1978) provides a concise description of ASL signing:

American Sign Language utterances occur within a space in front of the body consisting of an area bound by the waist and the top of the head and extending a few inches to each side of the body. The end of an ASL utterance is signaled by a pause.
as the hands return to a position of rest near the bottom of
the signing space. This signal can be modified when a question
is asked: At the end of the last sign in the utterance, the
hands either remain in their final position or reach outward
toward the addressee before returning to their rest position;
at the same time, the face assumes a questioning look. (p. 11)

As discussed above, ASL moved from "non-language" status to being
recognized as a full-fledged language because of work revealing
complexities of structure governed by grammatical rules. The following
is a brief description of the structure of ASL, a basic knowledge of
which is necessary in order to appreciate the fact that apes have not
acquired ASL. This description will include a discussion of the
following aspects of ASL: iconicity, simultaneity, word order,
morphology, and non-manual signs.

Characteristics of ASL

Iconicity

In considering the question of similarities and differences between
ASL and spoken languages, the two most obvious differences are the
iconicity and simultaneity characteristic of ASL signing. Iconicity
refers to the situation in which the meaning of a sign is more
predictable from its form than in other cases— that is, the physical
characteristics of the form imitate aspects of the meaning. Thus signs
may iconically represent the objects and actions they stand for. For
example, the ASL sign for 'tree' is made by holding the forearm bent
upward at the elbow with the fingers of the hand spread like the
branches and leaves of a tree. The iconicity that can be found in ASL was one of the first obstacles that had to be overcome on the road to "real" language status, because it seems not to conform to Hockett's design feature of "arbitrariness":

In a semantic communicative system the ties between meaningful message-elements and their meaning can be arbitrary or nonarbitrary. In language the ties are arbitrary. (Hockett, 1960, p. 6)

It was inferred from this that if the signs in ASL are nonarbitrary symbols, then ASL must not be a language. By now researchers in ASL have established that this simply is not so. Although some signs are indeed reminiscent of the objects which they represent, the number of such signs is not great, and the resemblance is not always apparent. In tests where non-signers of ASL were shown ASL signs and told to guess their meanings, the results revealed a performance level well below chance (Bellugi & Klima, 1976). Returning to the 'tree' example above, the connection is only obvious once the observer has been informed of the meaning of the sign. Like onomatopoeia in spoken language, the connections are not always immediately apparent. For example, in Hebrew 'bakbuk' is the word for 'bottle', because of the sound a liquid makes when poured from the bottle. It is doubtful whether one could figure out the meaning of the word even if told it is onomatopoeic. Another, different kind of example, again from Hebrew, is the etymology of the word for 'spoon'. The word for 'hand', or more specifically, the palm of the hand, is 'kaf'. The cupped hand may be used for scooping, and
thus the word for 'spoon' is also 'kaf' (Evan Shoshan, personal communication). The point is that there are many cases of lack of arbitrariness in language symbols, but they are the exception rather than the rule, and as a rule, the connections become obscured as the use of the word develops and changes over time. Although some signs may be iconic in origin, in ASL the tendency of historical change is toward increased arbitrariness (Frishberg, 1975).

A further point that should be stressed, as Newport & Meier (1985) and others have noted, is that there are many possible forms even iconic signs might take. The sign for tree is iconic in several sign languages (Bellugi & Klima, 1976), yet takes a different form in each. Although the sign itself may not be arbitrary in form, the choice of which form it will take does seem to be arbitrary.4

In addition, as most authors are quick to point out, it is seldom necessary to refer to iconicity in order to describe the grammar of ASL, the rules of which are similar to those of spoken languages. (But see Cohen, Namir & Schlesinger, 1977; DeMatteo, 1977; Mandel, 1977; for an opposing point of view).

Does iconicity play a role in the acquisition of ASL? Although native signers have learned to use the iconic character of some signs to advantage in teaching ASL to non-signers, recent research has provided evidence that iconicity does not play a role in the acquisition of ASL by deaf children (Bonvillian, Orlansky & Novack, 1983; Meier, 1981; Newport & Meier, 1985). This question has yet to be answered for apes learning the signs of ASL. Although Fouts (1973) found clear
differences in chimpanzees' ease of acquisition of ten signs, this was not necessarily correlated with iconicity. The chimps had great difficulties with the signs for 'hat' and 'look', and these signs happen to be rather iconic in nature. Fouts suggests that other factors are involved, including the chimps' natural aversion to bringing anything close to the eye (the sign for 'look' is made by touching the index finger near the corner of the eye). At the same time, Patterson (1978a) reports that the gorilla, Koko, acquired the 'look' sign quite readily, and her descriptions of signs which she claims the gorilla invented indicate that the signs are iconically motivated (Patterson, 1980a). Were it to be discovered that iconicity plays an important role in an ape's ability to learn signs, this would have interesting implications for the positing of underlying processes involved in ape sign acquisition.

Not only is iconicity not important in the child's acquisition of ASL, apparently it is also of little importance in the adult's use of the language. Psycholinguistic studies have found that the effects of iconicity are not very evident in sign language processing (Bellugi & Siple, 1974; Bellugi, Klima & Siple, 1975; Bowe, 1976; Siple, Fischer & Bellugi, 1977).

Simultaneity

The second obvious difference between ASL and spoken languages mentioned above is the simultaneity which is characteristic of ASL. This feature, and that of word order, are two aspects of ASL which are most often discussed in connection to ALR. In spoken languages, information
is broadcast on various levels (for example, segmental and supersegmental, so that intonation and stress are superimposed on the words of a sentence and on the sentence itself), but generally speaking, grammatical information is dispensed in a sequential fashion. Morphemes or grammatical markers are often tacked on to words in the form of affixes. In ASL much of the grammatical information is incorporated into the signs themselves, in the form of modulations, so that a change in movement is correlated with a change in meaning or grammatical function. Thus ASL may express lexical and grammatical information simultaneously. The process is the same as in a spoken language in that ultimately the lexical item will be marked grammatically for case, gender, or agreement etc, but the process unfolds at once rather than bit by bit. But see Supalla, 1982 as cited in Newport and Meier (1985) for some evidence which may indicate that even some elements of sign modulation may be sequentially ordered). This concept of simultaneity is probably one of the most misunderstood characteristics of ASL by researchers of animal language, as we shall see below.

Word Order

Word order in ASL is the second controversial aspect of the language to be frequently discussed in connection with ALR. Common knowledge holds that word order is completely free in ASL, but careful research has demonstrated that although the rich morphological system in ASL allows for more freedom than in a non-inflecting language like English, there most definitely are constraints on the order of elements in an ASL sentence (Wilbur, 1979). Although there is a great deal of
word order flexibility, ASL has a basic canonical word order. Most verbs in ASL are inflected to agree with one of their noun arguments; some verbs optionally agree with a second argument as well (Meier, 1981, 1982; Newport & Meier, 1985) (see following discussion on inflectional morphology in ASL). When this is the case, since the inflection identifies the relationship between the verb and the nouns in the sentence, word order is flexible. For those verbs which do not inflect (especially those which are signed on the body), word order is used instead to mark the relations among elements in the sentence (Newport & Meier, 1985). It should perhaps be re-emphasized here that "syntax" and "word order" are not synonymous, as this seems to be a constant source of confusion in ALR, and the basis for the controversy about ASL word order that is found in the ALR literature.

As mentioned above, word order is not nearly as important to the grammar of ASL as is the internal composition of the word, i.e. its morphology. Traditionally, there are two branches of morphology: inflectional and derivational. Inflections are grammatical markers added to words which indicate grammatical categories such as tense, aspect, person, number, case, and gender. Inflectional systems are regular and productive in that new words will be inflected according to the existing system, but no new words will be produced by that system. Derivational morphology, on the other hand, is the system whereby new words are formed by regular processes from existing lexical roots. Both branches of morphology are richly grammaticized in ASL.
Inflectional Morphology

The inflectional morphology of ASL is the most studied aspect of the language, probably originally because it was perceived as a way of establishing legitimacy (by demonstrating grammatical complexity), but eventually because it is the richest area of ASL grammar. ASL is not at all like English in this respect, and has in fact often been compared to Navaho (Newport & Meier, 1985). Unlike spoken language inflections, which generally take the form of separate morphemes concatenated with other morphemes (by affixation to a stem), in ASL inflection is produced by systematic modulation of one parameter of the sign (movement), so the information is simultaneous, rather than sequential (Klima, Bellugi, Newkirk & Battison, 1979; Newport, 1981; Wilbur, 1979).

An example of such modulation can be found in Bellugi & Klima's (1979) description of the varying forms of the ASL word SICK. Handshape and place of articulation for the sign remain constant, but by modulating only one parameter, that of movement, it is possible to modify the sign to yield different aspectual meanings such as when the state or action began, how long it lasted, how often it occurred, how strong it was. By such mechanisms it is possible to express the following meanings: "is sick", "was continuously sick", "became sick", "was intensely ill", "was repeatedly sick", "becomes sick easily", and others. In ASL it is possible to inflect for temporal aspect by modulating the temporal patterning of the movement of the sign. This modulation is done by varying such aspects of the movement as tension, rate, evenness, length, and manner. On the other hand, modulation of
spatial patterning is used to inflect for number and distributional aspect. Bellugi and Klima (1979) posit eleven such spatial and temporal dimensions of sign modulation.

One of the more interesting of these processes, particularly because of its relevance to ALR, is deixis:

The meaning of the term 'deixis'-- a loan word from Greek that means essentially "pointing" or "indicating"-- makes it a particularly apt expression for describing the special way many ASL verbs reflect their arguments; for the deictic function is marked in ASL directly by indexing locations in space and by changes in the form of the verb-- in the direction of its movement and in orientation-- so as to point to such spatial loci. (Bellugi & Klima, 1979, p. 276)

Referential indexing is used to express pronominalization, displacement and anaphora. For signers of ASL, there is a horizontal plane of signing space in front of the signer in which target loci may be established and later referred to. For example, the signer may wish to refer to someone more than once in the course of a conversation. Let us say I am discussing what a friend said. When first referring to the friend, perhaps by name, she is assigned a point, or locus in the horizontal plane. In English, when referring to this person again I would say 'she', or 'her' rather than the person's name. In ASL this is done by simply pointing to the target locus. As we can see, this same sort of use of space is employed to achieve displacement:
In discourse that extends beyond the speaker, the addressee, and the here and the now to objects, events, and persons not present, there are a variety of conventions for establishing indexical loci. The signer as narrator can use the indexic plane as a kind of stage on which indexical loci are created by indexic signs alone, or in conjunction with noun signs, or by positioning certain noun signs or classifier signs at particular locations on the indexic plane (Bellugi & Klima, 1979, p. 277).

In addition, verb signs can interact with these loci by moving between, toward, or at them, to express anaphoric reference.

Thus we see that pointing is an important grammatical device in ASL which yields not only structural complexity (pronominalization and anaphora), but conceptual complexity (displacement) as well. It is such an integral part of the ASL inflectional system that it would be difficult to imagine how one could claim to have acquired ASL without having acquired its special use of pointing. As we shall see below, such claims are made for the apes being trained in sign "language" projects.

Derivational Morphology

In addition to these inflectional processes, ASL also makes use of a number of derivational processes. In ASL, as in English and other spoken languages, there are many noun-verb pairs which are related in form, like 'record'-'record'. In English these pairs are differentiated by stress: #re-cord (noun), re-#cord (verb) (the symbol "#" is used
here to indicate primary stress). In ASL they may be differentiated by using Supalla & Newport’s (1978) distinctions of manner of movement (continuous, hold, restrained) as follows: while hand configuration, place of articulation, and movement path are the same, nouns are characterized by restrained and repeated movement, while verbs have a continuous or hold manner, and a single (for punctual) or repeated (for durative) motion (Newport & Meier, 1985). Supalla and Newport (1978) found that this relationship held for 100 noun-verb pairs where the noun denoted a concrete object involved in the action of the verb, such as 'airplane/fly', or 'seat (chair)/sit'.

Bellugi and Klima (1979) go beyond the Supalla and Newport (1978) data, which dealt only with concrete nouns, and argue that the same type of process is active in deriving related abstract noun-verb pairs such as 'comparison/compare', and even provide an example of the productive use of the mechanism by which native signers in their laboratory derived a sign for 'a derivation' from the sign QUOTE-FROM. In addition, they provide examples of derived predicates which result in the meaning 'to act, appear like': e.g. from GIRL to 'to act or appear girlish' to 'effeminate'. They also describe a fourth way in which these derivational processes add to the lexicon by forming a sign to represent the activity of the verb, somewhat like gerunds in English: IMPROVE becomes 'the act of improving'. Some derivational processes are less productive and the resulting changes in meaning are more idiosyncratic, such as the process by which COMMAND becomes 'bossy', DRESS becomes 'clothes conscious'. The same is true of spoken language, as for
instance in Hebrew from the root YSHV, 'sit', we get the noun 'yashvan', which should mean "one who sits", but actually takes on the meaning of the part of the human anatomy that one sits upon. The same process would seem to be applying in the case of signs which take on extended figurative meanings in ASL.

Although by far the most regularly productive area of derivational morphology is that of concrete object-action noun-verb pairs, it should be clear by now that ASL makes use of derivational morphology in a variety of ways to expand the lexicon by regular, systematic changes in the movement component of lexical forms, the result of which are related lexical items.

Non Manual Signs

Before moving on to the discussion of apes and ASL, there is one other area of the grammar which should be mentioned, as it will be relevant in the following discussion: the grammatical use of nonmanual signals such as bodily posture, eye gaze and facial expression. In ASL these are used to indicate clausal subordination (Liddel, 1978), topicalization (Fischer & Gough, 1978; Friedman, 1976; Liddel, 1977; Padden, 1983), questions, and negation (Liddel, 1977). Apes and ASL

Considering the long struggle to achieve language status, that is, to convince linguists that ASL has the defining characteristics of human languages, it is surprising that researchers in ASL have not been more outspoken in refuting the claims that have come out of ALR. Klima and Bellugi (1972) in a reply to the Gardners wrote:
It is clear from this experiment that Washoe not only has learned to make manual gestures, but makes them in ways that clearly refer to aspects of her external environment. Her ability to name shows a development that in many respects is similar to that of a young child. (p. 75)

Stokoe, the first to provide a linguistic description of ASL is even less critical in 1983:

The chimpanzee with signing human companions or teachers may be in reach of a real language.... Thus the language the chimpanzees may be in reach of may be ASL or, much more likely, it may be English expressed in manual signs. (p. 156)

Later he writes:

However, I find that the critics who attack the experiments have failed to provide any solid basis for denying what the animals have demonstrated. (p. 157)

In this section, we will discuss why ASL was used as a "target" for "language"-learning chimps in the first place, and how neither the goal of modelling ASL nor that of chimps acquiring it has been met.

How did it come about that ASL was chosen as a target for teaching apes language? The first serious attempts to teach apes language involved raising an ape in a human home environment, and hoping that the ape would learn the spoken language of its surrogate parents. These attempts were doomed from the outset to meet with failure for one very simple, and another less obvious reason. Although these studies continued as late as the fifties (Hayes & Hayes, 1952; Hayes & Nissen,
it wasn’t until later that Leiberman (1968) published his findings on relevant anatomical evidence, confirming that apes are simply physically incapable of vocalizing in such a way as to allow them to form the sounds of human spoken language. Quite simply, they lack the appropriate vocal apparatus. Recognizing this limitation, the Gardners (Gardner & Gardner 1969, 1971) noted perhaps an even more important constraint on the apes’ ability to learn to produce spoken language-- a behavioral limitation:

More important, the vocal behavior of the chimpanzee is very different from that of man. Chimpanzees do make many different sounds, but generally vocalization occurs in situations of high excitement and tends to be specific to the exciting situations. Undisturbed, chimpanzees are usually silent. Thus, it is unlikely that a chimpanzee could be trained to make refined use of its vocalizations. (R. Gardner & B. Gardner, 1969, p. 664)

Most of the existing information that we have for chimpanzees indicates that vocalization is highly resistant to modification in this otherwise highly educable species (Hayes & Hayes, 1950, 1951, 1952, 1955; Kellogg, 1968; Yerkes, 1925, 1943). (B. Gardner & R. Gardner, 1971, p. 120)

Yerkes (1925, 1929) suggested that a gestural language might be a more appropriate medium:

I'm inclined to conclude from various evidences that the great apes have plenty to talk about, but no gift for the use of
sounds to represent individual, as contrasted with racial, feelings or ideas. Perhaps they can be taught to use their fingers, somewhat as does the deaf and dumb person, and thus be helped to acquire a simple, nonvocal "sign language". (Yerkes, 1925, p. 180)

Influenced by Yerkes' suggestion, the Gardners chose ASL as the medium, and Washoe chimpanzee as the subject. It is important to note that prior to the Gardners' pioneering work with Washoe, very little was known about the grammar of ASL. As they admit in 1971 (Gardner & Gardner, 1971), they should have had a proper description of the grammar of ASL before beginning the project. They cannot be blamed for what they did not know about ASL when they began their project in 1966, but one would hope this lacuna would have been remedied by 1985. It has not. The Gardners (B. Gardner & R.A. Gardner, 1985) still insist their chimps knew ASL:

In our laboratory, infant chimpanzees were reared under human conditions that included two-way communication in American Sign Language (A.S.L.), the gestural language of the deaf in North America. A large body of evidence from five chimpanzees demonstrated stage by stage replication of the basic aspects of the acquisition of speech and signs by hearing and deaf children.... (i) the chimpanzees communicated information in A.S.L. to human observers; (ii) independent human observers agreed in their identification of the chimpanzee signs, (iii) the chimpanzees could use the signs to refer to natural

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language categories: DOG for any dog, FLOWER for any flower, SHOE for any shoe. (p. 159)

This is perhaps the most serious criticism I have for the Gardners and the projects with signing aces which followed (Fouts, 1983; Miles, 1983; Patterson & Linden, 1981). It seems none of the researchers took the time to really understand the workings of ASL. In 1971 the Gardners were sufficiently unsure of ASL's status as a bona-fide human language to feel the need to assert that "there is no reason to doubt that ASL is a sufficiently powerful linguistic system to test the capacities of a young chimpanzee" (B. Gardner & R. Gardner, 1971, p. 123). By now they should realize that it is too powerful a system to use as a model for ALR. Asking an ape to learn ASL is no more reasonable than asking it to learn English (which, presumably, even they realize is ridiculous), or for that matter Navaho, the inflectional system of which has been frequently compared with that of ASL.

In going through these reports, I was amazed by two observations. These scientists take a shortcut in their reports by simply invoking ASL without providing any background information about the language. Given that ASL is not exactly a language with which most readers will be familiar, it is surprising that they do not take the time to tell the reader something about ASL. What is more surprising is that when they do provide some background on ASL it is either erroneous or accompanied by unsupported claims of acquisition of these same properties of ASL by their animals. We are told that the formal teaching of structural aspects of ASL was deliberately avoided (Gardner & Gardner, 1978), and
in fact Washoe was not exposed to a model which included ASL inflections, yet at the same time we are also told that Washoe had acquired ASL inflections by the end of her last year with the Gardners.

In following the trail of invocation of ASL looking for other than superficial descriptions of the language, one finds that Fouts is the most remiss in this respect in that not only does he fail to provide a description of the language he purportedly taught his apes (Fouts 1973, 1974, 1975, 1976, 1977, 1978, 1983), but what little he does say (Atkinson Gorcya, Garner & Fouts, 1982; Fouts, Shapiro & O'Neil, 1978), only serves to demonstrate his lack of knowledge of grammar in general, and that of ASL in particular.13 The same is true of Patterson (1978a, 1978b, 1980a, 1980b) except that her claims for Koko gorilla's accomplishments are so wild and completely unsupported by any kind of scientific evidence as to render her work beyond the scope of scientific discussion (Patterson & Linden, 1981) (but see Terrace, 1982 for a valiant attempt).

Having provided somewhat of a guide to the workings of ASL in the first part of this chapter, perhaps the clearest way of demonstrating the disparity between what the signing apes are doing and ASL is by going through the discrepancies one by one. (For a discussion of the general lack of linguistic abilities demonstrated in signing apes, see the excellent review articles by Laura Petitto and Mark Seidenberg (Petitto & Seidenberg, 1979; Seidenberg & Petitto, 1979, 1981.)
Word Order

There have been several cases of profound misunderstanding of the grammar of ASL on the part of animal language researchers. Perhaps the area of most confusion is that of word order. Virtually all of those who have attempted to teach ASL to apes have remarked that word order is free in ASL. As we saw in the first part of this chapter, this is simply untrue. The preoccupation with word order arose as a result of early criticisms of the work. Bronowski & Bellugi's (1970) reply to the Gardners indicated that Washoe had yet to demonstrate that she had "acquired the use of formal sentence structure or the ability to analyze and synthesize complex messages." This was soon reduced to "apes haven't got syntax", which in turn was somehow reduced to "word order is not important to apes". Researchers on both sides of the controversy lost sight of the fact that "word order" is not synonymous with "syntax". As discussed in Chapter I, a syntax of a language must provide two things at least 1) how lexical items cooccur with each other to make up units, 2) how to order these units to make sentences. Word order is only one part of syntax.\(^{14}\) Those on the ape side have argued that since word order is not important in ASL, there is no reason to look for word order consistency in the apes' signing. The Gardners, Fouts and Patterson have criticized Terrace and his colleagues (Terrace et al., 1979) for examining word order regularity to the exclusion of context, whereas Terrace et al. accuse the others for failing adequately to document word order at all.

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The solution to the word order controversy is as follows: if the apes have actually been trained in ASL, then indeed, inflection should play greater role than word order in indicating who did what to whom. In order to claim that an ape has acquired the inflectional system of ASL it must be demonstrated that he both uses and understands it in a consistent and productive way. It is not enough simply to state, as have the Gardners (1978), Fouts (1978) and Patterson (1980a,b; Patterson & Linden, 1981) that the ape uses ASL inflections without supporting the claim with substantial evidence. The literature is plagued with examples based on anecdotal evidence. Providing an illustrative example is one thing. It is quite another to base a claim of acquisition of the inflectional system of ASL on one or two examples of an ape changing a particular sign in a particular context.

On the other hand, as Sanders (1985) notes, if what has been modelled to the apes, and what they have subsequently learned is not ASL at all, but Pidgin Sign English (PSE) which retains the canonical word order of spoken English (Terrace et al., 1979; Terrace, 1979), then order should be important for them. Again, word order itself should not be the issue, rather the question is one of being able to differentiate the semantic roles and syntactic functions of words in a sentence. Thus far no animal has been shown capable of doing this.

Word Function

Another common misunderstanding deals with the form and function of words in a sentence. The Gardners (1975) make the erroneous statement that "In Ameslan [ASL], as in most natural languages, many
signs can be used either as nouns or verbs" (p. 251). As we saw in the preceding section on morphology, this is certainly not true for ASL as there are clear derivational rules for relating object-action noun-verb pairs. Fouts (1975; Fouts & Rigby, 1975) gives a demonstration of the problem of overinterpretation (and lack of metalinguistic knowledge on his part) when he credits Washoe with making an adjective out of a noun in calling him "dirty Roger" (the sign DIRTY had heretofore been used by the trainers and the chimp in the context of feces). Again, I question the regularity and productivity of this process, if it was a process at all.

Simultaneity

As discussed above, simultaneity refers to the characteristic unfolding of information in the visual mode of ASL. Sign morphemes (the base sign plus inflections) appear together rather than sequentially, one after the other. This is not the same as saying that the base signs themselves appear together, although this does occur in the deliberate use of wit in puns (Klima & Bellugi, 1979). Patterson (1978a) and the Gardners (R.A. Gardner & B. Gardner, 1985) think that conflating signs is an inflectional device of ASL:

Another aspect of ASL which has no parallel in spoken languages is that certain signs may be produced simultaneously, thus leaving word order indeterminate. Koko began signing words simultaneously during the first few months of the project... and now frequently signs as many as three or four
words at the same time, for example, "ME-UP-HURRY"... and "HURRY-POUR-THERE-DRINK". (Patterson, 1978a, p. 91)

In her later work, Patterson tells us more about this simultaneous signing which Koko uses very frequently according to her:

Koko uses three different methods to generate simultaneous expressions: executing one sign with each hand... adding the motion or configuration of one sign to the place of articulation of another... and adding a facial expression or head movement to a sign. (Patterson, 1980a, p. 531)

By 1980 Patterson considered Koko’s combinations of signs by combining the place of articulation of one sign with the shape of another (as an example, she cites the C handshape articulated on the nose, which she glosses as "Chuck-toilet") to be slips of the hand; and she acknowledged that not all of Koko’s "modulations" are used by signers of ASL (they may learn something from Koko yet). As an example of these innovations on Koko’s part, she tells us that Koko pluralizes nouns by repeating the gesture for the object name (one wonders if she counts like Clever Hans), and that Koko indicates possession by signing signs simultaneously, as in BABY-HEAD. ASL does not make use of simultaneity in this way, and, as Petitto and Seidenberg (1979) point out, it is near impossible for a signer to make three or four signs or parts of signs at once, or on both hands, and one wonders how Patterson was able to distinguish which signs Koko was combining (since only parts of signs were combined) and how to interpret the outcome.
Patterson also states (1978a,b) that from the outset, she spoke English and signed ASL simultaneously (as did Fouts) when addressing her ape. As others have noted (Petitto & Seidenberg, 1979), considering the differences between the two languages, this would be impossible for a normal human being to accomplish since it requires simultaneous translation of two structurally different languages across modalities in the same person, as opposed to what we normally think of as simultaneous translation, which involves one person translating another's speech. More likely then, Patterson and Fouts spoke English and provided sign for content words as they said them— which would mean that they used English word order and "frozen" citation forms of ASL. This is not the same as signing ASL. In fact, this is what is referred to as PSE (see p. 59).

Repetition

For some ASL signs, repetition is part of the basic sign. Seidenberg and Petitto (1979) give the example of the ASL sign EAT, which requires the hand to be held in its particular shape and orientation while the movement (moving towards the mouth) is executed exactly twice. This is not the same as reduplication, which is used as a means of indicating temporal aspect in ASL (as discussed in the first section). As Bellugi and Klima (and others in their 1979 book) describe, by changing the movement of a sign, repeating, enlarging, making more or less tense, it is possible to cause changes in meaning which may indicate continuous or punctual action, or intensity of the activity, or even tendency to engage in the activity. Van Cantfort and
Rimpau (1982), students of the Gardners, imply that the Gardners' chimps (Moja, Dar, Pili, and Tatu) used repetition as a grammatical device, but in their discussion of this topic they merely refer to the researchers' habit of deleting repetitions as being unimportant. At any rate, there is no reason to assume that the apes could use repetition as a grammatical device.

Non Manual Signs

It is interesting to note that facial expression and bodily posturing are a part of the natural behavior of the ape (Hewes, 1976; van Lawick-Goodall, 1968), and these behaviors have carried over to the signing context in certain cases. Patterson (1980a), the Gardners (1978) and the Fouts project (O'Sullivan, Fouts, Hannum & Schneider, 1982) report that their apes use these expressions as grammatical devices in their signing: "Washoe used the manual activities and facial expressions that signal the prosodic features of American Sign Language" (Gardner & Gardner, 1978, p. 307). The same claim is made for Moja, Dar, Pili and Tatu: "These new subjects began to use the questioning function by the tenth month, and used three major contrasts--statement, question, and emphasis--regularly, before the end of their first year" (p. 307). It is not clear how the Gardners' apes used nonmanual signals to indicate emphasis, and in any case, emphasis in ASL involves a change in the movement parameter not mentioned by the Gardners. As for the questioning expression, as this is a natural facial expression which often accompanies the natural gesturing of apes (even my dog has a questioning facial expression), it is not surprising that they continued
to display facial expression while signing. The question is whether the apes are able to use this expression to modulate meaning systematically. Evidently the Gardners think so. From my own experience of viewing films of signing apes, I have wondered at the apparent lack of attention on the part of the apes because they rarely look at the person with whom they are supposed to be engaged in conversation, or the objects about which they are signing. This apparent indifference is characteristic of the signing apes, and indicates the need to further examine their natural use of facial expression and eye gaze. While humans engage each other's gaze (indeed, the recipient of a signed message monitors the signer's eyes and face as well as observing the hands) while carrying on a conversation, this does not seem to be natural to chimps, and may even be counter-indicated by their social rules. As the Gardners originally noted about natural vocal behavior, it may not lend itself well to behavioral modification.

At the same time, Van Cantfort & Rimpau, in describing how the chimps used the sign THAT with contrasting facial expression to indicate statement or question, say that for questions the chimps do make eye contact consistently:

... the distinction between the two forms was eye contact and the length of time that the sign formation was held. In Tatu's use of the interrogative form THAT? eye contact was made an maintained while the sign duration was longer than in the declarative form. (Van Cantfort & Rimpau, 1982, p. 36-37)

Of course, as noted earlier in the section describing characteristics of
ASL, the use of nonmanual signals in ASL goes beyond contrasting questions with declaratives to marking subordinate clauses in complex sentences. Van Cantfort and Rimpau caution against comparing adult human use of ASL with infant chimps'. If an ape could learn to create a subordinate clause, his use of facial expression to mark that clause would not surprise me in the least.

Time Line and Deixis

It becomes obvious as we continue that there is no reason to search for grammatical marking devices in the apes' signing, since there are no grammatical structures to be marked. There is no use of the time line and no use of deixis. This is not surprising because it has yet to be established that apes are capable of conceptualizing past and present (Koko's discussions of death notwithstanding (Patterson & Linden, 1981)). That is, the conceptual underpinnings are not there. Deixis is a perfect example of how the two must go hand in hand, and any superficial resemblance between ape pointing and indexical referencing in ASL is just that—superficial. Deixis enables at once expression of displacement (by establishing loci it is possible to refer to referents which are not present) and the complex structure of anaphora.

Van Cantfort and Rimpau (1982) report that the young chimps studied by the Gardners and themselves showed early development of establishment of loci. Like Patterson, they report that the chimps often signed on (i.e. touching) the object of their request, as do children. In the adult form of using loci, these are abstract points in space, and not actual objects in the room. Again, it can be argued that the chimps are
still young and we should not expect abstract assignment of loci, after all, this use of loci must develop in human children— as mentioned, there is a period when children sign on objects as well. More likely, though, it is not a question of an immature system that is developing (Washoe is now going on 20, and Koko 15), but one lacking an ability to abstract that is beyond the cognitive capacity of the ape. The apes’ signing behavior may resemble that of the immature human signer, but this is exactly the point: it never leaves this stage of immaturity.

Conclusion

It is not surprising that we find no evidence of grammatical devices in the apes’ signing since they were probably also lacking in the model, because the model provided to the apes was not ASL. Although the Gardners’ original intention was that the apes be exposed to ASL exclusively, and that all those who worked with them be fluent in ASL, this turned out not to be feasible: "...the human experimenters, themselves, were only slowly mastering the inflectional peculiarities of Ameslan during the course of Project Washoe" (Gardner & Gardner, 1978, p. 308).

Perhaps the greatest discrepancy between Project Washoe as we originally conceived it and as we were able to execute it lies in the weakness of the adult models that we could provide...

For the most part, Washoe’s human companions and adult models for signing could be described as hearing people who had acquired Ameslan very recently and whose fluency, diction, and grammar were limited because they had very little practice
except with each other and with Washoe. (Gardner & Gardner, 1978, p. 293)

Their successors were less concerned with this obvious drawback. This resulted in a spiralling effect which has not been recognized as such. The apes are taught at best a pidgin sign, but more likely merely separate signs in isolation. They learn to make some signs which superficially resemble the signs of ASL, and then the critics come along and say that what the apes are doing could at best be described as a pidgin sign (Terrace, 1983, 1984). If the Gardners' original intent in choosing ASL was to use a ready made human language to avoid the problem of having to invent their own language and then defend its status as a language, by failing to model ASL properly, they lost the privilege of invoking ASL with all of its considerable grammatical machinery.

What we find as we go through the literature on signing apes is that the resemblance to ASL is only on the surface. The signs produced by the apes may look like ASL, but there is only sparse anecdotal evidence of grammatical use of signs, and no evidence of comprehension of grammar on the part of the apes. Even the form of the signs is not identical to ASL—variations are permitted which would change meaning or render a sign uninterpretable in ASL. In the Gardners' reports we are told that when deaf signers of ASL observed Washoe they had no difficulty in interpreting her behavior. Although the Gardners would take this as proof that Washoe was signing in ASL, Seidenberg and Petitto (1979) suggest that any sensitive observer could interpret her behavior. I am not certain the same would be true of some of the
combinations of signs which Patterson attributes to Koko. One wonders how much of Patterson's own expectations she imposes on Koko's behavior, and how much could be interpreted from a faithful transcript of Koko's signing alone. The empirical question of how dependent interpretation is on context and on knowing the ape well, is one which needs to be addressed in all of the ape signing projects. (See works by Sebeok in the bibliography in Appendix C for some overly zealous attempts at addressing this issue).

In sum then, we may say that at best the apes have been shown to be capable of responding to a pidgin-like system employing "frozen" citation signs of ASL, and they are also capable of producing many of these signs. Apes have yet to learn those very characteristics of ASL which reinforce the concept of universal grammar. ASL has the same kind of analytic character and hierarchical structure (discrete units inside of discrete units) as spoken languages. As Newport (1982) has suggested, "it therefore appears that language demands this type of organization, even when the modality would permit other quite different organizational possibilities" (p. 477).

The Gardners originally chose ASL as the language they intended to model for their chimps because it obviated the need to invent an artificial system, and then prove that that system was a language. It is not sufficient to prove that the input is a language, in addition, and even more important, it is necessary to demonstrate that the animal is able to understand and use the linguistic properties of the model.
Apes have not learned ASL, and it should never have been expected of them— it is as absurd as expecting them to learn Navaho, Russian or English. The Gardners did the apes an unintentional disservice by choosing ASL as the target language. Instead, they should have searched for a model which provided a simplified form of language, but one which contained the basic grammatical baggage necessary to warrant inclusion in the family of human languages. It is not an insignificant observation that what the apes do looks more like a pidgin than anything else. This fact can be used in planning further study, as we will see in Chapter VI.
Chapter II Notes

1. ASL is not to be confused with Signed English and other non-natural sign systems (Seeing Essential English, Signed Exact English) which may be used in teaching deaf students English, or for simultaneous translation (Siple, 1978; Wilbur, 1979).

2. That these processes are similar in both signed and spoken languages is not surprising when we consider that as a language ASL has the same job as any spoken language, and since the study of universal grammar has shown us that humans seem to have a prescribed way of doing language, it should be of no surprise that ASL has the same processes as any other human language. In effect, this is further support for a language bio-program (see Chapter V) which is not modality-dependent, as one would not expect it to be.

3. These differences are to be expected. ASL is a visual-gestural language, as opposed to an aural-vocal one— that is, these differences in mechanisms would seem to be modality-specific. But this is an empirical question. Until the same kind of thorough study is done of the grammars of other sign languages of the world, we will not know if sign languages are typologically similar in the way their grammars are manifested.

4. It would be interesting to see how often it is the case that an iconic sign will be so across different sign languages. That is, is there a set of particular signs that are always iconically represented, and if so, what else do they share in common?

5. It is interesting that although iconicity is available to children
acquiring ASL, they seem not to take advantage of it. Why this should be the case is an intriguing question. One might venture to speculate that since non-arbitrariness is generally not a characteristic of human language, the human does not come into the world equipped to perceive iconicity. Or perhaps there is a linguistic faculty that searches for forms within the language to analyze as elements in a system, and this overrides any tendency to notice the relationship between the individual form and its referent. Apes may lack this faculty, but at the same time it would be interesting to find out if they could do the cognitively complex mapping between form and referent required to utilize iconicity in their signing. (See discussion in Chapter V.)

6. Fischer (Bellugi & Fischer, 1972) suggests that one reason for the simultaneous character of ASL may be to compensate for the fact that signing takes longer than talking.

7. Verbs are not inflected for tense in ASL. Time is indicated with lexical items which fall along a "time line" (Friedman, 1975; Frishberg, & Gough, 1973). Rather than marking tense on verbs in the sentence, or even on the sentence itself, ASL (like Chinese) indexes time for whole stretches of discourse by marking the time of occurrence at the beginning of the conversation, and only indicating time when there are changes (Studdert-Kennedy, 1980; Wilbur, 1979). Many time signs are morphologically related, and are articulated along an arc which runs from a point in front of the signer's dominant side at face level (for future), touching the cheek in the middle, and to a point behind the head (for past).
8. By common convention, all caps will signify ASL signs.

9. Although these are considered grammatical markers "Grammaticized facial expressions are used as obligatory markers..." (Newport & Meier, 1985), this designation is not entirely satisfactory. Obligatory use does not mean the grammar cannot be described without referring to these elements, and one wonders whether facial expression or bodily attitude are actually called in to play to differentiate between sign meanings. They may be more along the lines of intonation and stress (which may be necessary to differentiate meaning of words in isolation, or between sentences like "It is?" vs. "It is.") (Covington, 1973).

10. Although the Gardners began their work with Washoe in 1966, evidence was already beginning to accumulate against the appropriateness of spoken language as the medium. In their 1969 report, they cite Bryan's (1963) comparison of chimp and human vocal anatomy, and the failure of the Hayes' experiment with Viki.

11. Note that of the four signing projects which followed the Gardners, the two most recent, that of Miles (1983) and Terrace and his colleagues (1979), recognized the fact that the system they were modelling was in fact Pidgin Signed English (PSE), but both groups failed to recognize the significance of this fact (see discussion below).

12. Although Fouts and Patterson evidently do not consider it so ludicrous: both claim that their apes comprehend spoken English. (Patterson, 1978a,b, 1980a,b, Patterson & Linden, 1981; Fouts, Chown & Goodin, 1976.)
13. In Atkinson Gorcya, Garner & Fouts (1982), the authors demonstrate a misguided conception of the sociolinguistic notion of "variation", and the linguistic term "inflection", not only misunderstanding both, but confusing them with each other. The term "variation", used in the context of sociolinguistics, refers to the differences which may occur across various dialects of the same language (as used by different individuals), or it may refer to stylistic differences in language use in varying social contexts (within one individual). Variation does not change the words beyond the point of intelligibility, yet the "variation" these authors describe, "such variation can be produced by changing the hand configuration, the sign in relation to the body, or the direction or intensity of the movement associated with the sign" (p. 220) would not just vary the sign, but would change its very meaning. In fact, the very changes in movement they describe are those which ASL employs as meaningful inflections (discussed above).

14. Order can be singled out and drilled as we will see in Chapter IV, and the result is that order does become important in the animal’s response. This does not mean that the animal is aware of the function of word order. (see also Hoban, 1983 and Peters, 1985).

15. There are two possible reasons for this. One, as discussed above, is the fact that the trainers did not know enough ASL to fully demonstrate its use. They themselves used citation forms (i.e. uninflected forms) of ASL in English word order. Another reason that we find no evidence that the apes were capable of comprehending ASL grammar is that these studies concentrated almost exclusively on
production of signs. Of course the ultimate explanation-- that they are simply incapable of learning grammar-- has yet to be established.
 CHAPTER III  
SETH AND NIM  

It is common in the ALR literature to compare apes' learning of systems in which they are instructed, whether in sign as described in the last chapter or in artificial systems (Premack, 1971a, b, c.; Rumbaugh, 1977a, b, c) to the acquisition of language by children. In fact, the Gardners stated at the outset that this was their intended purpose in designing and carrying out the Washoe Project, and in their choice of ASL in particular:

Thus it should be possible to compare the performance of a chimpanzee in ASL with the performance of deaf human children, and the performance of either with the performance of hearing children in English. (Gardner & Gardner, 1971, p. 123)

It is always possible to compare, the question is whether the comparison is valid and interesting. It is not sufficient to merely juxtapose two instances of a phenomenon, begin listing the components of each and performing an item by item comparison. First we must establish a basis for the juxtaposition. The case of bat wings and bird wings is familiar to students of evolutionary biology. Although their respective functions are similar, the relationship is one of analogy rather than homology— they are not related in origin. There is no a priori reason to avoid comparison based on analogous behavior or performance, many important contributions to the natural sciences can attest to this fact, the problem arises when the relationship is mistaken for what it is not. In the case of chimps and children, we are not always in a position to
choose between the two alternatives. On the one hand, anthropologists are not all in agreement as to how to draw the evolutionary family tree. Recently it has been suggested that man is more closely related to the chimpanzee than the chimpanzee is to the gorilla. This would seem to warrant suggestions of homology in comparing chimps to children, but then where does this leave Koko the gorilla, and Chantek the orangutan?

On the other hand, we are still trying to understand language acquisition in our own species. Is it premature to draw parallels between processes we are unsure of in child language and processes we are still less sure of in apes' behavior? Based on the impact such comparisons have had thus far on the fields of psychology and linguistics, such parallel-drawing has been a fruitful practice, although more in terms of questions generated than answers provided. By examining the language acquisition process (such as it may be in animals) and comparing it with that of humans, we may be able to decide if they are so different as to be unrelated, or if they share common processes, and if so, to what degree, keeping in mind at all times that having established a correlation does not imply causality, and that what appears similar may only be superficially so. With these caveats in mind, we now turn to a comparison of Nim--Chimpanzee, and Seth--little boy.

The Nim Project

Complete background details of the Nim Project are available elsewhere (Sanders, 1980; Terrace, 1979; Terrace et al., 1979; Terrace, Petitto, Sanders & Bever 1980). A brief sketch will suffice here. In
an attempt to replicate the work of the Gardners, Herbert Terrace at Columbia University undertook a similar project with a male chimpanzee which he obtained at the age of two weeks and trained in sign language until he was about four years old. Terrace and his colleagues correctly make the distinction between ASL and what their chimp, Nim, was taught:

In Pidgin Sign English (PSE), ASL signs together with some of its expressive devices in English word order without the grammatical morphemes of English, are used....In this and all other studies of signing behavior in apes, PSE, not ASL, was used. In this sense, it is misleading to term their signing "ASL" since it does not exhibit the grammatical structure of that language. (Terrace et al., 1979, note 7)

The initial goal of the Nim Project was to determine whether a chimp could learn to use grammatical rules. In order to answer this question, the researchers collected a large corpus of approximately 20,000 utterances over the course of Project Nim, and examined them for sign-order regularities. As discussed in Chapter II, researchers on previous chimp projects had considered the question of word order to be irrelevant, and they neglected to gather data on the subject. The Gardners, for example, recorded sign combinations using conventional English word order. An examination of their data will not reveal whether Washoe signed, for instance, MORE TICKLE or TICKLE MORE. that information, not having been recorded, is lost forever. Terrace and his colleagues in turn have been criticized for having paid attention exclusively to word order, to the point of disregarding the
context of the utterance within the discourse (Gaustad, 1981; Gardner & Gardner, 1985; O'Sullivan et al., 1982; Patterson & Linden, 1981; Van Cantfort & Rimpau, 1982). The work culminated in the now famous question "Can an ape create a sentence?" (and an article by the same name, Terrace et al., 1979) where "sentence" was defined as follows:

A sentence characteristically expresses a complete semantic proposition through a set of words and phrases, each bearing particular grammatical relations to one another. (Terrace et al., 1979, p. 891)

Terrace et al.'s answer to the question, "can an ape create a sentence?" was a singularly final: "No".

What they had discovered, not from the corpus itself, but from subsequent analysis of video tapes of Nim interacting with his teachers, was that Nim often imitated what his teacher had just signed. This finding caused the researchers to conclude that the word order regularity which had been previously noted in the data (and led to attributing to Nim the ability to structure his utterances grammatically) could no longer be the basis for inferring a productive competence in Nim. The earlier conclusion that Nim was structuring his utterances grammatically was now considered invalidated by the fact that his utterances seemed overly dependent on those of his teachers. Not only had much of what he signed just been signed by his teachers, but just as likely as not, Nim signed at the same time as his teachers. Terrace et al. concluded that Nim's signing required no "linguistic" explanation, i.e., it did not require attributing to Nim any grammatical
competence. Instead, the parsimonious explanation was that Nim had learned that a set of vocabulary items was appropriate to various contexts, and he simply used these signs, adopting a strategy of "copy the teacher" in order to obtain objects and food which were withheld from him until he signed for them.

This conclusion was based in part then, on the character of Nim's "conversations" with his trainers, particularly his discourse strategies of imitating and interrupting. The Terrace group claimed that Nim imitated and interrupted out of all proportion in comparison to children, and initiated conversation, or produced spontaneous utterances less often than a human child would. Thus, a linguistic explanation of Nim's output was ruled out partly because it could be explained by imitation, and partly because his performance was considered too different (in terms of discourse style) from that of a normal child (based on comparisons with children studied by Bloom, Rocisanno & Hood, 1976).

Imitation has been the focus of recent studies of child language acquisition, and some data are available (Bloom, Hood & Lightbown, 1974; Bloom, et al., 1976; Moerk & Moerk, 1979; Snow, 1983), but there are no figures available on interruptions by the child in a conversation with an adult. The assumption here was that discourse strategies play a key role in the acquisition of language, so that an indication that a child is indeed acquiring language is the fact that she is able to create sentences spontaneously. The basis for this inference goes back to the Chomsky (1965) "how else" approach: how else can we account for the
fact that children are able to create novel sentences other than to assume that they know a grammar? Thus ape language researchers look for the creation of novel sentences as a sign that the ape knows a grammar. It has been assumed (as did Terrace) that the frequency of interruption is negligible in adult/child discourse, and that spontaneity and initiation make up the greater part of a child’s interactive behavior in conversation. Is this really so? An ongoing study at the University of Hawaii provides some data which address these questions.

The Seth Project

A project is currently underway at the University of Hawaii on the acquisition of language by a blind child. The data for this project were collected in the form of one to two hours of audio tape per week (plus diary notes) by the child’s father, who was the primary care-giver from the age of 19 months. He began recording his son at the age of 15 months, and the corpus covers the period from that time until the age of 48 months.

Each tape is transcribed by a member of the project in broad phonetic transcription. The transcripts include comments, timing, stress, intonation, and a gloss for every utterance. (See appendix A for a complete set of transcription conventions for the Seth Project). The transcript is then checked by a second member, reviewed by the project director, checked by the father, and finally reexamined by the project director, Dr. Ann Peters.
The Subject

The child, Seth, was born blind (due to hypoplasia of the optic nerve), but by the end of his first year was able to perceive light through tunnels of peripheral vision (particularly in his left eye). This enabled him to avoid objects when crawling, and later to discriminate letters (if they were large enough and close enough), shapes, and colors. Children who are thus denied eye contact have been found to be more dependent on verbal expression to communicate (Dunlea, 1982). In addition, they are more likely to imitate their caregiver than sighted children (although one of Bloom et al.'s subjects imitated up to 25% of the time, and obviously there is a great deal of individual variation).

In addition to his blindness (and perhaps even more importantly), Seth is an only child, being raised primarily by his father, who also happens to be a linguist. The father not only encouraged the child's linguistic development, but took a professional interest in it as an object of study. These factors, combined with his blindness, make Seth a special subject. As with any case study (including that of Nim), caution must be advised in generalizing to a larger population. Seth may not be a prototypical language learner (if there is such a thing), but at the same time, the corpus is so rich that it provides us with the opportunity to examine aspects of his conversational strategies which have not been discussed in the literature thus far. In particular, it allows us to compute percentages of interruption and initiation, and compare these with those found for Nim.
The Data Sample

Chronological age can be misleading in terms of gauging language development, and MLU (mean length of utterance) is generally accepted to be a more reasonable basis for comparison. Nim’s MLU during the period studied fluctuated between 1.1 and 1.6 signs. Eleven of the tapes transcribed thus far in the Seth Project were recorded during a period when Seth’s MLU was similar to that of Nim, ranging from 1.2 to 1.6. These tapes were chosen for the analysis.

Following the method of the Nim Project, each of Seth’s utterances was first classified as usable—intelligible and unambiguous (gloss), or unusable—ambiguous or unintelligible. Since the original Seth transcripts were of one-half hour portions of one-hour recordings, the number of utterances often reached up to 400 per tape, while the entire corpus analyzed by Sanders (1980, 1985) for Nim consisted of 1176 utterances. It was therefore decided to restrict the number of utterances analyzed here to a more manageable number. This was done by selecting the first 50 unambiguous, intelligible, usable utterances from each tape. Table 1 shows the breakdown of utterances for each transcript.

Classification of Utterances for the Discourse Analysis

As discussed above, a linguistic explanation of Nim’s behavior was rejected by members of the Terrace team, in part, because of the results of a discourse analysis. Data for Nim are available in the form of several published reports (Sanders, 1985; Terrace, 1979, 1981, 1984; Terrace et al. 1979, 1980) and a dissertation by Sanders (1980). In
<table>
<thead>
<tr>
<th>TAPE #</th>
<th>16</th>
<th>17</th>
<th>18</th>
<th>19</th>
<th>21</th>
<th>22</th>
<th>23</th>
<th>25</th>
<th>27</th>
<th>29</th>
<th>33</th>
</tr>
</thead>
<tbody>
<tr>
<td>AGE</td>
<td>19.1</td>
<td>19.2</td>
<td>19.3</td>
<td>20.0</td>
<td>20.2</td>
<td>20.3</td>
<td>21.0</td>
<td>21.2</td>
<td>22.0</td>
<td>22.2</td>
<td>23.2</td>
</tr>
<tr>
<td>MLU</td>
<td>1.4</td>
<td>1.2</td>
<td>1.4</td>
<td>1.2</td>
<td>1.3</td>
<td>1.3</td>
<td>1.2</td>
<td>1.2</td>
<td>1.4</td>
<td>1.4</td>
<td>1.6</td>
</tr>
<tr>
<td>TOTAL UTTS+</td>
<td>76</td>
<td>54</td>
<td>65</td>
<td>56</td>
<td>58</td>
<td>63</td>
<td>59</td>
<td>67</td>
<td>60</td>
<td>55</td>
<td>63</td>
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<tr>
<td>INCLUDED</td>
<td>50</td>
<td>50</td>
<td>50</td>
<td>50</td>
<td>50</td>
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<td>50</td>
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<td>50</td>
</tr>
<tr>
<td>DISCARDED</td>
<td>26</td>
<td>4</td>
<td>15</td>
<td>6</td>
<td>8</td>
<td>3</td>
<td>9</td>
<td>17</td>
<td>10</td>
<td>5</td>
<td>13</td>
</tr>
</tbody>
</table>

- Age is in months and weeks.
+ Equals total number of utterances examined.
their discourse analysis, the researchers on the Nim Project divided the corpus into adjacent and non-adjacent utterances. Adjacent utterances were defined as those that followed an adult utterance without a definitive pause (Bloom, et al., 1974). Non-adjacent, or initiating utterances are those which spontaneously initiate a conversation--utterances which are neither immediately preceded by an adult utterance nor are repetitions of the previous child (or chimp) utterance.8

Adjacent utterances were further broken down into three subcategories: 1) imitation 2) expansion 3) spontaneous. Those utterances which were comprised solely of words which appeared in the model in the prior context were classified as imitations.9 This was spontaneous imitation, as opposed to requested, as in "say 'please'" (the latter being deleted from the corpus), and if it differed at all from the model, it was by reduction or simplification, but never addition. If, on the other hand, some material was added, the utterance was classified as an expansion. A spontaneous utterance was an utterance originated by the child immediately following an adult utterance, in which no words imitated the adult model. If an utterance were repeated by the child, it was classified according to the classification of the first instance of the utterance, as was the procedure on the Nim project. Thus, successive imitations were all classified as imitation, and if a spontaneous utterance were repeated, all of the repetitions were classified as spontaneous.

If an identical adult utterance intervened between the child's repetitions it was difficult to determine whether the following child
utterance was a self-repetition or an imitation of the adult. On the Nim project these were classified in the same manner as the first instance of the sign in the series. For example:

Nim: HUG (spont.)
Teacher: HUG
Nim: HUG (spont.)

This procedure was followed in the present analysis as well, in order to render the data comparable to those from the Nim Project. This occurred only eight times in the entire Seth corpus analysed. From the reports of the Nim Project, it is not possible to determine how often this occurred in the Nim corpus. The implications of this treatment of repetition are discussed below.

Results and Discussion

Imitation

Seth frequently imitated some or all of what Daddy said. There was a total of 122 imitated utterances in the corpus of 550 utterances (22% of the total corpus). As can be seen in Table 2, the relative frequency of imitation fluctuated in the first four tapes, reaching as high as 62% at age 19.2, but began to decrease steadily towards the end of the period analyzed. The Terrace group observed Nim’s imitation over a period of two years (notice that the data for Seth represent a four month period from 19.1 (months.weeks) to 23.2 because that was the period during which his MLU was like Nim’s) and found that not only did it not decrease over time, but actually began to increase toward the end of the period he was with them. Table 3 shows that Nim’s overall
## TABLE 2
SUMMARY OF SETH TRANSCRIPTS

<table>
<thead>
<tr>
<th>ACE</th>
<th>19.1</th>
<th>19.2</th>
<th>19.3</th>
<th>20.0</th>
<th>20.2</th>
<th>20.3</th>
<th>21.0</th>
<th>21.2</th>
<th>22.0</th>
<th>22.2</th>
<th>23.2</th>
<th>MEAN</th>
</tr>
</thead>
<tbody>
<tr>
<td>% ADJACENT</td>
<td>80</td>
<td>98</td>
<td>72</td>
<td>92</td>
<td>96</td>
<td>70</td>
<td>86</td>
<td>74</td>
<td>80</td>
<td>78</td>
<td>66</td>
<td>81</td>
</tr>
<tr>
<td>% IMITATED</td>
<td>22</td>
<td>62</td>
<td>18</td>
<td>52</td>
<td>26</td>
<td>12</td>
<td>14</td>
<td>14</td>
<td>12</td>
<td>6</td>
<td>6</td>
<td>22</td>
</tr>
<tr>
<td>% EXPANDED</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>4</td>
<td>0</td>
<td>0</td>
<td>4</td>
<td>0</td>
<td>6</td>
<td>4</td>
<td>2</td>
</tr>
<tr>
<td>% SPONTAN.</td>
<td>58</td>
<td>36</td>
<td>54</td>
<td>40</td>
<td>66</td>
<td>58</td>
<td>72</td>
<td>56</td>
<td>68</td>
<td>66</td>
<td>56</td>
<td>57</td>
</tr>
<tr>
<td>% INITIATED</td>
<td>20</td>
<td>2</td>
<td>28</td>
<td>8</td>
<td>4</td>
<td>30</td>
<td>14</td>
<td>9</td>
<td>26</td>
<td>20</td>
<td>22</td>
<td>34</td>
</tr>
<tr>
<td>TAPE NUMBER</td>
<td>1</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>5</td>
<td>6</td>
<td>7</td>
<td>8</td>
<td>9</td>
<td>10</td>
<td>11</td>
<td>12</td>
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<tr>
<td>-------------</td>
<td>----</td>
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<td>----</td>
<td>----</td>
<td>----</td>
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<td>----</td>
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<tr>
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<td>76</td>
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<td>74</td>
<td>86</td>
<td>84</td>
<td>91</td>
<td>95</td>
<td>82</td>
<td>92</td>
<td>75</td>
</tr>
<tr>
<td>% Imitated</td>
<td>14</td>
<td>35</td>
<td>32</td>
<td>21</td>
<td>18</td>
<td>32</td>
<td>33</td>
<td>48</td>
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<td>22</td>
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<tr>
<td>% Expanded</td>
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<td>11</td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% Spontan.</td>
<td>60</td>
<td>41</td>
<td>53</td>
<td>68</td>
<td>66</td>
<td>44</td>
<td>41</td>
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<td>68</td>
</tr>
<tr>
<td>% Initiated</td>
<td>25</td>
<td>24</td>
<td>24</td>
<td>37</td>
<td>26</td>
<td>14</td>
<td>16</td>
<td>9</td>
<td>5</td>
<td>18</td>
<td>8</td>
<td>25</td>
</tr>
</tbody>
</table>

(Data from Sanders, 1980)

- These scores are based on frequency of spontaneous plus initiated utterances in the back-three context. (see text)
average frequency of imitation was 35%. Supporters of ape-language (those who believe that apes are capable of learning language) have argued that Nim was an extraordinary chimp in this respect. Four transcripts of "conversations" with two chimps from the Fouts' project appended to Miles' (1978) dissertation provided Terrace (1981) with the basis for performing a similar discourse analysis of their signing. The data, from Terrace 1981, are presented below in Table 4.

<table>
<thead>
<tr>
<th>TAPE NUMBER</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>MEAN</th>
</tr>
</thead>
<tbody>
<tr>
<td>% ADJACENT</td>
<td>76</td>
<td>94</td>
<td>77</td>
<td>86</td>
<td>83</td>
</tr>
<tr>
<td>% IMITATED</td>
<td>14</td>
<td>23</td>
<td>8</td>
<td>8</td>
<td>13</td>
</tr>
<tr>
<td>% EXPANDED</td>
<td>8</td>
<td>8</td>
<td>14</td>
<td>4</td>
<td>8</td>
</tr>
<tr>
<td>% SPONTAN.</td>
<td>55</td>
<td>63</td>
<td>56</td>
<td>74</td>
<td>62</td>
</tr>
<tr>
<td>% INITIATED</td>
<td>24</td>
<td>6</td>
<td>3</td>
<td>4</td>
<td>17</td>
</tr>
</tbody>
</table>

(Adapted from Terrace, 1981, Table 4)

When we compare the chimp data with that of the child, we see that in terms of frequency of imitation, the child falls in between the two sets of chimp data. The respective frequencies are: Booee and Ally 13% overall, Seth 22%, and Nim 35% overall. Terrace et al. compared Nim with the children studied by Bloom et al. (1976) whose imitations accounted for 18% of the utterances at the age of 21 months (MLU 1.3) (averaged across children). What do these numbers mean? Is Nim an exceptional chimp? Is Seth an exceptional child?
The first factor to keep in mind when considering these results is the sampling techniques involved. The sampling methods were very different in each project. Terrace had only nine video tapes (Sanders used an additional 6) filmed over a period of two years. These videos were recorded on a regular basis during Nim's lessons in his special classroom at Columbia. The Seth tapes, aside from being more numerous (per time period), were less regular in their sampling (and thus did not constitute a random sampling of Seth's speech in all contexts, but rather a sample of his speech at those times that his father decided to turn on the tape recorder). Seth's father generally turned on the tape when it was convenient for him (often during a structured activity like mealtime) and when Seth seemed to be in a talkative mood. The father was not interested in obtaining a random sample which would reflect an average amount and type of speech on Seth's part. Instead, he wanted to collect data on what Seth said when he did speak. At the same time, recording sessions were relaxed as Seth and Daddy went about their daily routines. Seth was not even aware of the function of the tape recorder, although he was allowed to play with it and did speak of it often. The Fouts' chimps were filmed during relaxed outdoor playtime with their trainers.

When examined in this light, a correlation between type of situation when the recording took place (classroom lesson, daily activity, playtime) and frequency of imitation is revealed, namely: the more relaxed the situation, the less frequent the imitation. It has been suggested that this correlation indicates a causative relationship
(Miles, 1983; Sanders 1985 and others), and it should be of no surprise that general mood of the interaction should affect the behavior of the participants in terms of interactive strategies. For example, Sanders (1980) provides an excerpt from a transcript of a lesson in which Laura Petitto is teaching Nim the sign LISTEN. Nim frequently imitates her, as he obviously must in order to learn the sign. The same is true of Seth in situations where he is specifically being taught, and his father even tells him to imitate, using the "say X" routine, e.g "Say 'geese'."

Another example of the influence of the character of interaction itself on the behavior of the participants can be found in the frequency of repetition in the Seth data. After having listened to countless Seth tapes, one gets the impression that Seth’s repetition is often an inverse function of how attentive his father is (see the conversation below).

What about the numbers themselves? What is the effect of classifying successive repetitions as members of the same category as the first instance of the utterance? The actual effect of this practice is an inflation of the frequency of whatever the subject tends to repeat most. For Seth this had the effect of inflating the number of utterances counted as spontaneous. Seth’s interactions with his father tended to display an alternating pattern of Daddy, Seth, Daddy, Seth, Daddy, Seth, until Daddy fell silent for a period, during which Seth would continue to repeat his last utterance, as can be seen in the following excerpt from transcript 23 (age 21.0), (phonology normalized to English orthographic conventions. S = Seth, D = Daddy):

90
D: What da the chickens say?
S: gobble-gobble.
S: tape.
D: What does the turkey say?
S: gobble-gobble
D: What does a duck say?
S: (makes duck noises)
D: What does the geese say? (sic)
S: honk. (pause)
D: Honk.
(5 sec.)
S: hair.
D: (no response)
S: hair.
D: (no response)
S: hair.
D: (no response)
S: no?
D: (no response)
S: no?
D: (no response)
S: no?
D: (no response)
S: hair.
D: (no response)
S: no?
D: Daddy's hair.
Data on repetition are not presented in any of the reports of the Nim project, but all of the signing projects report that their apes repeated much of the time. Based on the few excerpts from transcripts presented in the various reports by Terrace and his group, it would seem that in the case of Nim, he repeated more imitative utterances than non-imitative. If this is true throughout the data, it may be an important difference in the interactive strategies of Seth and Nim. Nim may have repeated what he imitated based on the results he obtained. Since his imitations of the model would tend to be appropriate to the situation, he would be rewarded more frequently for those, and thus would tend to repeat them more often. This could explain his increasing use of imitation. Seth, on the other hand, repeated himself when he was ignored, in order to draw Daddy’s attention. This actually reflects again a difference in the interaction itself. Whereas Seth’s father often "tuned out" and became preoccupied (reading the paper; eating his own food), Nim was never ignored, and never had to try to get his teacher’s attention.

Spontaneous and Initiated Utterances

Seth’s spontaneous speech ranged in frequency from 36% to 72%, averaging 57% (Table 2). For Nim, the percentage of spontaneous signing ranged from 22% to 68%, with an average of 42% (Table 3). The Booee and Ally data reflect a consistently high rate of spontaneous utterances, ranging from 55% to 74%, averaging 62% (Table 4). Again, Seth falls in between the two sets of chimp data. One reason for the low frequency of spontaneous signing for Nim is that Sanders used a "back-three" (see
note 9) context in calculating the scores for non-imitative signing. Since he counted back three steps, there was more likelihood that Nim's utterance would contain some element of a prior utterance, and thus not qualify as a spontaneous or initiated (non-adjacent) utterance. In fact, the use of this procedure of back-three context yielded only 34 initiations in the entire Nim corpus (3%) and Sanders therefore combined spontaneous and initiated utterances (back-three context) into one category, the frequency of which is reflected in the category "spontaneous" in Table 3. Using the back one context, Nim actually initiated 17% of the time, which is similar to the frequency of Seth's initiating (19%) and the same as that of Booee and Ally.

Expansion

Interestingly enough, neither Seth nor Nim expanded on prior utterances very often, as opposed to an average of 21% expansion for the children at MLU 1.3 in the Bloom et al. (1976) study. Details were not provided for Nim, but Sanders reports that overall he expanded in only 9% of his utterances. (The three statistics shown in Table 3 were the only ones available). Booee and Ally expanded about 8% of the time, but Seth rarely expanded, and his average frequency of expansion totalled only 2%. For Seth, expansion was simply not a part of his discourse strategy during this period (see below in the section on continued development for a discussion of the kind of expansion which occurred later). Although he imitated quite often, he rarely expanded on his father's utterances.
Adjacency and Interruptions

When two people are engaged in a conversation, an utterance by one tends to be followed by an utterance by the other. This should be reflected in the frequency of adjacency, which is quite similar for Seth (81% overall), Nim (83%) and Booee and Ally (83%). There is also a certain amount of time when simultaneous utterances may occur in the course of a conversation. This is not necessarily a sign that the participants are not listening to each other, and is often just the opposite—a sign of increased attentiveness to the point of anticipation of the other's utterance (cf. Tannen's 1984 work on New Yorkers' "high involvement" style). Interruption can be interpreted in a number of ways, depending on where it occurs, and how often.

Because the original Seth transcripts were so complete in terms of vocalizations and context, it was possible to go through the entire eleven transcripts and tally the instances of interruptions and simultaneous speech (see Table 5). In examining the transcripts, it was found that although Seth sometimes interrupted Daddy, and Daddy sometimes interrupted Seth, for the most part it was impossible to lay the blame on either because both began at the same time. Even so, the total frequency of simultaneous speech in Seth and Daddy's interactions (7% overall) was nowhere near as great as the frequency with which Nim interrupted his teachers (44% overall).

What might be the explanation for this difference? Sanders (1980) hypothesized that perhaps Nim was signing in parallel with the teacher, as opposed to in response to him/her, in essence, paying no attention to
**TABLE 5**

**ABSOLUTE (AND RELATIVE) FREQUENCIES OF INTERRUPTION**

<table>
<thead>
<tr>
<th></th>
<th>19.1</th>
<th>19.2</th>
<th>19.3</th>
<th>20.0</th>
<th>20.2</th>
<th>20.3</th>
<th>21.0</th>
<th>21.2</th>
<th>22.0</th>
<th>22.2</th>
<th>23.2</th>
<th>MEAN</th>
</tr>
</thead>
<tbody>
<tr>
<td># UTTERANCES</td>
<td>344</td>
<td>411</td>
<td>246</td>
<td>252</td>
<td>549</td>
<td>287</td>
<td>395</td>
<td>515</td>
<td>439</td>
<td>487</td>
<td>356</td>
<td></td>
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<tr>
<td>SETH</td>
<td>6(2)</td>
<td>8(2)</td>
<td>5(2)</td>
<td>13(5)</td>
<td>30(8)</td>
<td>1(.3)</td>
<td>1(.2)</td>
<td>5(1)</td>
<td>9(2)</td>
<td>5(1)</td>
<td>2(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>DADDY</td>
<td>11(3)</td>
<td>3(.7)</td>
<td>0(0)</td>
<td>2(.7)</td>
<td>6(2)</td>
<td>0(0)</td>
<td>0(0)</td>
<td>3(.5)</td>
<td>4(1)</td>
<td>2(.4)</td>
<td>1(.2)</td>
<td>(1)</td>
</tr>
<tr>
<td>BOTH</td>
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<td>31(8)</td>
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<td>10(4)</td>
<td>38(10)</td>
<td>3(1)</td>
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<td>24(4)</td>
<td>5(1)</td>
<td>34(7)</td>
<td>6(2)</td>
<td>(4)</td>
</tr>
<tr>
<td>TOTAL</td>
<td>28(8)</td>
<td>42(10)</td>
<td>14(6)</td>
<td>25(10)</td>
<td>74(13)</td>
<td>4(1)</td>
<td>4(1)</td>
<td>29(6)</td>
<td>18(4)</td>
<td>41(8)</td>
<td>9(3)</td>
<td>(7)</td>
</tr>
</tbody>
</table>
what the teacher was saying. His subsequent analysis of the data provided no support for this hypothesis, indicating instead that Nim "employed deliberate conversational strategies." Why then, if he were truly engaged in a conversation with the teacher, would Nim tend to interrupt or overlap so frequently? The answer to this question may provide the key to the differences between the structure of chimp and child's utterances which will emerge only later.

For Seth, interruption was sometimes a result of a "broken connection". Daddy is talking to Seth, and Seth is talking to Daddy, but each is more concerned with what he has to say than with the other (cf. Ochs, 1979). In the following excerpt, Daddy and Seth have just gone outside to look at trees. (Age 22 months) (Seth has learned to associate the words "bark", "leaves", "rough", and "tree" with this activity, and has been saying "bark" and "tree").

D: There. It's an *oak tree.
S: *bark.

(* indicates point of interruption)

Sometimes, Seth may have deliberately interrupted Daddy in a repeated request. In the next example, they have been talking about putting shoes on, and Seth has just requested "shoes'on" (all run together) twice.

D: Yeah, *put shoes on.
S: *shoe shoe.

Often, the interruption was in anticipation of Daddy's next utterance. Daddy and Seth have gone into the bedroom where Seth is staying on a
visit. They come upon a box in the room.

D: Here's a *box.
S: *box.

Most of the time though, Daddy and Seth just happen to start talking at the same time:

S: *tape.
D: *There's the-- there's the tape.

The times that Seth's utterances actually intruded into the middle of Daddy's were infrequent, and when they did, it seems to have been either a deliberate attempt to draw attention to his own utterance, or a result of lack of attention to Daddy. If Nim's interruptions were not caused by either of these reasons, how might they be explained?

Sanders reports that Nim often began to imitate the teacher's signing before the teacher had completed his/her utterance. This may have been due to processing limitations: Nim might have had to begin imitating the sign before he forgot what to sign. Indeed, as he got older (and presumably his memory capacity expanded) the chunks he imitated grew longer, requiring, it would seem, that he wait longer (for the teacher to provide more of the model to imitate) before beginning the imitation. The inordinate amount of interruption as Nim grew older would seem to indicate not that Nim had no conversational manners, but that he never learned to parse the signing stream which he received as input. He was never able to conceive of utterances as being made up of combinations of separate elements which had a beginning and an end.
Instead he was limited to perceiving and imitating unsegmented chunks of elements.

Alternatively, Nim's interruption may have been in response to the training paradigm, which required that he sign in order to get what he wanted. In such a situation, not signing would be counterproductive, so it is not so surprising that Nim signed along with the teacher. At this time, it is impossible to choose between the above two alternatives. We must await future experiments that test an ape's ability to conceive of the language stream as elements within a system.

Structure

The following are conversations between Nim and his teachers, and between Seth and his father:

T: DICK EAT--
N: BERRY
T: ME--
N: DICK *EAT.
T: *EAT.
T: NIM--
N: BERRY NIM--
T: EAT.
N: EAT.
T: NIM EAT YOGURT, *YOGURT--
N: *EAT
T: YOGURT, *YOGURT--
N: *YOGURT NIM.
       (Sanders, 1980, p. 47)
***************
T: SEE RAIN OUTSIDE?
N: AFRAID.
N: HUG.
T: YOU AFRAID NOISE?
N: MARY, AFRAID.
N: HUG.
T: WHAT YOU THINK ABOUT NOW?
N: PLAY.
T: WHAT PLAY?
N: PULL, JUMP.
T: (later) YOU TIRED NOW?
N: TIRED.
N: SLEEP, BRUSH-TEETH.
N: HUG.
       (Terrace, 1979, p. 186)
***************

(Phonology normalized to English orthographic conventions.)

S: Tonya
S: baby, uh Seth.
D: Baby Seth?
S: tape
D: Who is Seth?
S: *Seth
D: *Who is Seth W.?
S: me-ee!
D: Awright! I thought so. (5 sec.)
D: Tonya has babies.
S: ha' baby.

(Seth, 19.2 months old)

***********************
D: Is that your brush?
S: brush teeth.
D: Uhuh.
S: brush teeth?
D: Did you wake up this morning?
S: wake up?
D: (later) You want any more oatmeal?
S: no.
D: Okay, you want down?
S: please. (means yes)
D: You want more milk?
S: jump?
D: You want jump down?
S: milk? milk?

(Seth, 21.2 months old)

***************************
D: Daddy's gonna go fix your breakfast.
S: Oatmeal.
S: Oatmeal.
D: Yeah. Yeah. Let's go do that.
S: Hungry?
D: Ya hungry?
S: Please. (means "yes")

(Seth, 22 months old)

We can see from this and other samples that the utterances of Seth and Nim seem to have similar structure: for both of them, utterances of one word (or chunk) in length are mostly either noun, verb, or adjective; and utterances containing two words are mainly composed of nouns and verbs. Examples of the four logically possible combinations of noun and verb can be found in the speech of both Seth and Nim. Both frequently use combinations of noun-noun, as in Nim's "berry Nim" and Seth's "baby Seth"; both frequently produce noun-verb combinations, as in Nim's "Dick eat" and Seth's "geese say"; both frequently produce verb-noun combinations, like "eat Nim" or "brush teeth"; and both occasionally juxtapose two verbs, (usually separated by a pause) as in Nim's "pull, jump".

It is difficult to differentiate structurally between the output of Seth and Nim based on such a comparison. There is no basis for calling these utterances "linguistic" because there is no reason to believe that they are governed by grammatical rules. They are either imitations of a model which is governed by grammatical rules, or they are simply
combinations of units. The non-imitative utterances, as opposed to being hierarchically structured sentences, seem to be a simple linear stringing together of units. Peters (in press; cf. also Brain, 1976 and Ewing, 1983 for discussions of the question of syntax in early utterances) points out the important distinction between "syntactic constructions" and "combinations":

It is quite possible that early multi-unit productions are not governed by any ordering constraints that we would wish to call syntactic. That is, such productions may either be unordered, or they may have fixed but non-contrasting orders. Therefore, until we find evidence in children's productions for contrasting order patterns (e.g. subject + verb vs. verb + object or agent + action vs. action + goal) we will call them combinations rather than syntactic constructions. (p. 11)

It is important to note here, that for Seth, this is just a stage in his development from which he will soon depart, whereas for Nim this is as far as he got.

Continued Development

Perhaps one of the important differences between Seth and Nim is that later (after the period discussed above) Seth's frequency of expansion greatly increased. There were several ways in which Seth expanded utterances. Sometimes he expanded an imitation of Daddy, other times he added to his own repetition, as in:

S: let's go
S: let's go now
There is also another sort of expansion device which we call "frame inserts". This is where Seth inserts words into slots in a stock frame, systematically substituting one element for another, for example:

- S: big tree
- S: big doggie
- S: nice doggie

This kind of expansion does not happen until Seth begins to analyze units into separate elements, so he is able to expand by substituting and adding elements.

This is probably the first capability to differentiate between the child and the chimpanzee: the ability to perceive utterances as being made up of separate units, which are in turn made up of separate elements. In this first stage of recognition of units, children may be confused about what the units are. They know to look for units, but only experience shows which are the units of their language (Peters, 1983). Imitating, as Seth does, may initially lead the child astray so that his perception of boundaries does not match that of the adult (Peters, 1985). In the case of Seth, this unit parsing began somewhere between the age of two and two and a half (as evidenced by the rapid increase of MLU-morphemes shown in Figure 1), and by the age of three he had already been lead astray once (according to Wilson & Peters, 1984), as evidenced in his consistent use of anomalous wh-questions such as "what are you cooking on a hot?", and "what's Katherine is my?" In Seth's case, there is evidence that as he got older he used imitation to learn about his language, acquiring new words and learning their
idiosyncratic lexical properties. For Nim, the failure to increase utterance length suggests that imitation led nowhere.

Nim’s MLU never rose above about 1.6 at the age of about four years, when the project was terminated. As can be seen in Figure 1, Seth’s MLU hovers just above one from the age of 19.1 until 23.2, the period during which his output is similar to that of Nim. In the five-month period between 23 and 28 months, however, Seth’s MLU increases rapidly from just above one to 4.4. It is at this point when the MLU begins to expand rapidly that the child quickly leaves the chimp behind. By the age of 28 months, Seth was saying things like:

Let’s see how you pour it, Dad.
I want to put away that. Put it in Daddy’s waste basket.
You gonna put it away? This one goes that side.
Want you ta hit it, Dad.
Where’s that one? Where’s the other one?
I’m gonna throw that on Daddy’s head.
What’s that? (it’s a ruler) I could bat with that.

The way that MLU grows is also important. Nim’s apparent MLU grew to a certain extent, but this was due to a change in the kind of vocabulary he was acquiring (whole words or chunks of words), and not due to an increase in productive morphology. Seth’s MLU first starts
Figure 1. Seth's MLU in words and morphemes
its expansion with morphological bits like the plural -s, -'s, -ing, -y, etc. His MLU expands by chunks breaking apart and the separate parts being recombined into new units. We can see when these morphological bits become productive in Seth's utterances on the basis of their distribution. For example, initially -ing might appear only on one or two words, at which time it seems more like part of a chunk, or a memorized formula. Later, when it is analyzed as a separate element it begins to show up on any verb to which an adult might attach it. Two curves are shown in the graph in Figure 1. One shows MLU based on words, and the other MLU based on morphemes. The overlap in the graph indicates the period during which morphemes begin to be analyzed separately from words. With Nim on the other hand, there is no evidence that he ever learned any of the morphology of ASL (see Chapter II). Thus the period during which the speech of Seth and the signing of Nim are structurally similar is a brief one.

Implications

Terrace and his team found consistent word order patterns in Nim's signing, but on the basis of a discourse analysis argued that the behavior was not founded in knowledge of linguistic rules, that is, it required no linguistic explanation, because Nim was not creating sentences by applying grammatical rules to a lexicon of words. Instead, at best, he had learned to associate certain signs with their contexts, and to use them later in the appropriate context. The discourse analysis of Seth's speech during the period in which his MLU was close to Nim's indicated that he is not so different from Nim in terms of
adjacency, imitation, and spontaneity, and we have just seen that their utterances are also similar in terms of structure (or lack thereof).

Does Seth’s and/or Nim’s behavior at the stage when they are similar require a linguistic explanation? Considering the similarities between them, there are four logical possibilities of explaining both behaviors:

```
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We may abandon the final possibility at the outset. Not even the most avid ape-lover would argue that the chimp’s behavior is linguistic while the child’s is not. Let us look then at the first possibility, that Seth’s behavior is linguistic and Nim’s is not. This position would be advocated by Terrace, and probably most parents. As we saw, one argument for this possibility is that although the behavior may look similar, discourse analysis indicates that Seth and Nim are different. We have just seen however, that this is not necessarily so.

Another argument is the traditional argument that we all know the child’s behavior (barring disaster) eventually blooms into adult language. But is it legitimate to base an argument about the status of a particular stage on knowledge about what it will become? It may be legitimate in some cases, but it may also cause us to miss important
facts. For example, two plants may look very similar at a particular stage of their development. Because we know that one will eventually become a sequoia, and the other will remain a weed should we disregard this similarity? If we do, we run the risk of overlooking an important stage in the life of plants: the seedling stage.

The second possibility is that both Seth and Nim’s behavior is linguistic. This is what the Gardners and other signing ape researchers would argue, allowing the ape to hitch a ride, as it were, on the child’s performance. The Gardners have argued that similar performances should be interpreted alike:

Any theoretical criteria that can be applied to the early utterances of children can also be applied to the early utterances of chimpanzees. If children can be said to have acquired language on the basis of their performance, then chimpanzees can be said to have acquired language to the extent that their performance matches that of children. (Gardner & Gardner, 1975, p. 244)

I fully agree that the similar performance of chimp and child in the MLU 1-2 stage requires a common explanation, which means we cannot ignore the remaining logical possibility, and the choice which most likely represents the actual situation: neither Seth nor Nim’s behavior at this stage can be considered "linguistic". This idea might distress ape language researcher and parent alike, but it is not really so far-fetched, as we will see in Chapter V.
Summary

In this chapter we reviewed the signing project of Terrace et al. with the chimpanzee Nim, and compared his "language acquisition" with that of a child, Seth, during the period when their respective MLU's were in the same range. We found that actually, the outputs of Seth and Nim were similar both in terms of form and discourse strategies.

Terrace et al. agreed that the structure of Nim's utterances resembled that of children's utterances superficially, but that an examination of his discourse style compared to that of children suggested that the underlying basis for his behavior was not founded in application of grammatical rules to words, but in imitation of his trainers. What was argued here is that 1) Seth imitates quite often as well, so in that respect Nim is not so different from this child; and 2) Even when neither Seth nor Nim is imitating, neither seems to use syntactic rules in their creation of utterances, but both seem to be merely combining units.

It was suggested that perhaps the best explanation for these similarities is that neither Seth's nor Nim's productions are linguistic (in the sense that they are grammatically governed). In Chapter V these results will be discussed further and a model of language acquisition will be presented which can account for both the brief period in the development of the speech of the child and the signing of the ape when there seems to be no obvious difference in their output, and for the fact that after that this period similarities between them begin to
disappear rapidly as the child goes on to acquire his language while the chimp remains in the non-linguistic stage.
Chapter III Notes

1. Here, again, as we saw in Chapter II and as we shall see in Chapter IV, researchers in ALR have a misguided concept of "syntax", consistently and incorrectly equating it with "word order". Even if Nim did display consistency in the order of his signing, this alone would not demonstrate an ability to structure utterances grammatically.

2. Project members have included: Eileen Cain, Ermile Hargrove, Esmé Hoban, Barbara Voigt, and the project director, Dr. Ann Peters.

3. The father eventually moved away and was no longer available for consultation, but he reviewed all of the tapes discussed here.

4. Nim’s MLU was calculated by averaging the number of signs per utterance. This is comparable to counting words (as opposed to morphemes) for spoken language. The MLU’s reported here for Seth are also for words.

5. There were also several other types of utterance which were not included in the present analysis. Because the plans of the Seth Project call for a much finer coding system than that used on the Nim Project, every sound that Seth made (including those not made by his mouth) is included in the original transcript. This includes laughing, crying, mumbling, babbling, talking to himself, responses to requests that he say something, etc. These utterances have been excluded here analysis.

6. Sanders examined a set of 15 video tapes, spanning from 26 to 45 months. Only nine of these (spanning the same period) are discussed in the Terrace et al. reports.
7. This was true for every tape except tape number 21. This tape began with an additional adult present, which changed the discourse considerably (Daddy spoke to the other adult rather than to Seth). For this tape, data selection began from the time that the additional adult left the room.

8. It should be noted that terminology here differs slightly from the Nim Project. The terms used here are: Adjacent/Initiated, Imitated, Expanded, and Spontaneous; the respective terms used in the Nim reports were: Adjacent/Nonadjacent (also called spontaneous), Imitated, Expanded, and Novel.

9. "Prior context" was defined in the Seth Project as within five utterances of the model (Bloom et al., 1974). In the Nim Project the distance was one step back ("step" defined as 5 seconds from the beginning of the last sign in Nim's utterance) except in the case of spontaneous utterances, which were classified as such using a back-three (three steps) context.

10. All relative frequencies (percentages) in Table 5 were calculated relative to Seth's speech: the figures for Seth tell us how often when he began an utterance it was an interruption of Daddy's ongoing speech; the figures for Daddy tell us how often when Seth was speaking Daddy interrupted him; and the "Both" figures reflect how often when Seth began to speak Daddy began to speak at the same time.

11. Since the only data presented for Nim are classified as interruptions, it is impossible to know how often the teacher and Nim began signing at the same time.
12. Ochs (1979) notes how often one is not aware of this phenomenon until the transcript is carefully examined. Even the format of the transcript itself can bring this out or obscure it.
CHAPTER IV
MARINE MAMMALS

Apes are not the only subjects of animal language research. Recently, marine mammals (in the form of dolphins and sea lions) have joined the ranks of captive pupils in these experiments. This chapter will review this area of ALR and describe how a group of researchers using the least language-like model have made the strongest claims of linguistic achievements by their subjects.

What makes an animal a good candidate for language studies? In the case of apes, they were considered the logical choice for ALR because of their evolutionary relatedness to our own species:

[The phylogenetic closeness of man and ape] is in fact, very real, not (sic) matter how much some would hope to ignore it. The existence of common neurological substrates of language can, at the present time, best be determined by behavioral studies, not anatomical ones. (Savage-Rumabugh & Rumbaugh, 1982, p. 572)

It was thought that any language capability they might demonstrate could then be attributed to common evolutionary development. If an ape could be shown to be capable of learning language, the continuity/discontinuity controversy would be resolved. On the other hand, if dolphins could demonstrate similar linguistic achievements,

[this] would shift the emphasis of explanation for language skill from that of homologous processes of ape and man to common selective processes in the form of convergent cognitive
adaptations to ecological, biological, or social demands.
(Herman, 1980, p. 411)

There are two basic reasons why cetaceans have been chosen as participants in ALR. The first reason is based on the common belief that the roots of language are to be found in less sophisticated communication systems, as discussed in Chapter I. It has been assumed that the best candidate for language training would therefore be the animal most likely to desire to communicate with humans. This would be a highly social animal which associates closely with members of its own species, and is thus likely to have a more developed communication system of its own. Cetaceans fit the bill, and one species in particular has been singled out on the basis of its ecological distribution. Unlike many of the pelagic species of cetaceans, the bottlenosed dolphin (*Tursiops truncatus*) inhabits coastal areas, and readily adapts to captivity.

The second requirement of the ALR candidate is a large brain. Even Aristotle recognized the connection between brain size and intelligence, but it was Harry Jerison's "encephalization quotient" (EQ) which was hypothesized to capture the important relationship between brain size and cognitive capacity (Jerison, 1973, 1977). One way to compare species is to examine the ratio of (log) brain weight to (log) body weight, but the EQ theory goes beyond the brain to body weight ratio to consider function. The EQ refers to the amount of brain tissue in excess of that required by a creature to support the biological systems of the body. Thus an EQ of one is hypothesized to mean that the
organism has just enough brains for its job. An EQ of greater than one is hypothesized to reflect extra brain power to apply to problems beyond the everyday struggle for survival. EQ has been considered a direct measure of information processing capacity, or biological intelligence. According to Jerison, highly encephalized species (aside from man) include elephants, dolphins, monkeys, and great apes. Eisenberg (1981), using an adaptation of Jerison's EQ formula, calculated EQ's for selected species. Using such a measure, humans are said to be endowed with a brain seven times the size of that which they need to function, dolphins are next with an EQ of up to 4.9, and chimps have an EQ of about 2.3. Thus, in this requirement as well, dolphins pass the candidacy test.

Early Work with Dolphins

Work with dolphins began as early as the sixties, with Lilly's (1961, 1962, 1967) attempts to teach dolphins to speak English, and included a "home environment" approach in which Margaret Howe, Lilly's assistant, lived with Peter dolphin for almost three months (described in Lilly, 1967). Alas, Peter never learned English, and, as evidenced in his ardent sexual approaches, communication was limited to body language. Feeling that dolphins were too intelligent and sensitive to be maintained as captive subjects in human experiments, in the late sixties Lilly decided to abandon his efforts until such time as dolphins could be willing participants in communication studies, free to come and go as they pleased. Today he has reconsidered this abrupt decision, and can be found in northern California working on training dolphins to
mimic human vocal language—the approach is the same, but progress in electronic technology has introduced the voice digitizer into his system. Results of this resumed effort have yet to be published in scientific journals.

Lilly's work inspired Dwight W. Batteau, a mechanical engineer who worked with the Navy, which undertook a language project of its own in 1966, a description of which appears in Wood (1973). Batteau, working in Hawaii, used a comprehension approach, rather than focusing on production, as had most ape language researchers (and Lilly as well). In his study, two dolphins, Maui and Puka, were taught to respond to whistle sounds (produced by transforming the sound of the human voice with an electronic device) which were projected underwater through speakers. Each sound (i.e. human word transformed by the "man-to-porpoise translator") represented a command to carry out an action ("slap flukes", "jump"), or an action to an object ("hit ball with flipper", "swim through hoop"). The dolphins were called to their training station by individual "name" sounds, followed by a "ready" signal, a command, and an "execute" signal. Correct responses were rewarded with a "correct" sound, and the feeding of a fish. In addition, the dolphins were taught a signal which called for them to approach the work station, and a different signal which told them to move outward, away from the station. Finally, with two-way communication (and the use of a "porpoise-to-man translator") in mind, Batteau taught the dolphins to mimic the sounds given them as commands.
At this stage of the project, commands required a response to a particular object. One sound triggered an entire sequence of taking an action to an object, so for example, the sound "bip" meant "hit ball with flipper", and the only object present was a single ball. Wood (1973) reports that probe trials in which such a command was given with a different object present (perhaps with the intention of eventually having separate sounds for the action "hit with flipper" and the object "ball") resulted in confusion, but his description of the dolphins’ behavior would seem to indicate generalization of the specific command to a new situation. For example, when told to hit the ball with the flipper, with only a hoop present, the dolphin either hit the hoop with its flipper or swam through the hoop (the activity normally associated with that object).\(^2\) No data are provided on the frequency of these responses (in the Wood, 1973 report), but it is difficult to imagine what the "correct" response would be in this situation, and the dolphins’ behavior seems to cover both logical possibilities.

The project progressed to the point where one dolphin, Maui, had a repertoire of 15 responses, which included swimming to the left or right on command, or swimming away (each time further away than the location at the time the command was given). At this point, Batteau died in an apparent drowning accident, and the project was terminated. Evidently, at the time the Navy saw no reason to continue what it considered an unpromising avenue of research. Wood summarizes the conclusions:

In the end, the real significance of this project lay not in what the animals learned to do, but in their apparent failure
to understand that the game involved more than a trained response to an arbitrary command. (Wood, 1973, p. 112)

(One wonders what Wood would have the dolphins understand after such limited training.)

In sum, Batteau's work was, if anything, damaging to the idea that porpoises have the degree of linguistic comprehension and flexibility that proponents of their intelligence and language capability believed they had. (p. 113)

Others were more hopeful. Across the island from the Kaneohe, Hawaii Navy research facility was a psychology professor at the University of Hawaii who hoped to go much further than had Batteau. The dolphin, Puka, the same animal used in the Batteau experiment, was transferred to the facility of Dr. Louis Herman where she continued to be a subject of psychological experimentation, while her tankmate, Kea, began a new language training program. Herman followed the comprehension approach, but he added a new dimension, hoping to overcome limitations of the previous work. Batteau's approach was seriously limited by the fact that one sound triggered an entire response chain. Since the actions and objects did not each have unique sounds associated with them, it was impossible to maximize efficiency by recombinining elements. There was no provision for replacement of items in action or object slots, so it was impossible, for instance, to replace the word "ball" with "hoop" in the command for "hit the ball". Instead, an entirely new command would have to be generated, thus failing to capture an essential feature of human language-- "reconstitution", or
"recombinability" (Bronowski & Bellugi, 1970). Herman hoped to overcome this limitation by providing separate sounds for actions and objects for the dolphin Kea. The work proceeded rapidly and with great success (as described in Herman, 1980), but just as the earlier work with Puka and Maui had been terminated by the death of Batteau, strangely, this project too was doomed to end in misfortune. Puka and Kea were stolen and released in the ocean, presumed dead by their former owner.

The Herman Dolphin Project

Herman's dolphin language project resumed in June, 1978 when two new pupils, the dolphins Phoenix and Akeakamai arrived at the laboratory. The project has now entered its eighth year, and though it is "younger" than any of the ape projects, the results have been heralded by Herman and his colleagues (Herman, 1986; Herman & Forestell, 1985; Herman et al., 1984) as representing the most successful effort thus far in the field of animal language research, in terms of both the experiments themselves (design and execution), and the achievements of the dolphins.

In the resumed language research, the language format was the same as the previous project, i.e. objects and actions were assigned unique signals which could then be combined and recombined to generate a limited but complete set of all permissible commands. In addition to object and action symbols, each dolphin was taught one pair of opposing directional symbols. It is important to note that these were relative, as opposed to absolute directional symbols. Thus, one dolphin could be commanded to act on an object to her left, but only if there were a
corresponding paired object on her right. The other dolphin could respond correctly to commands to act on a lower (sunken in the tank by a lead weight) object, but only when it was opposed to an identical upper object. Generally, when these directional symbols were used, two to three objects were paired. Thus, there might be pairs of right and left balls, frisbees and hoops, for example. These object, action, and directional symbols could then be strung together to form commands of two basic types. One type of command required the dolphin to go to a single object and perform an action on it; the other required the dolphin to perform an action relating two objects. Thus, two simple rules were used by the researchers to generate all of the commands to the dolphins:

1. rule 1: (direction) object action1
2. rule 2: (direction) object1 action2 (direction) object2

where action2 was either of two signals, which instructed the dolphin to bring object1 to object2, or put object1 in object2. Reversing the positions of the objects in the command reversed the meaning.

As in the Batteau project, each command was preceded by a "name" sound which called a particular dolphin to station, followed by a "ready" signal which called the dolphin to attention. (Herman did away with the "execute" sound which Batteau had used to tell the dolphin that the command had terminated and it was time to respond.) Correct execution of the command was rewarded by a "correct" signal, followed by the "name" sound, and the dispensing of a fish accompanied by verbal
praise and plenty of petting and patting. Incorrect responses resulted in the dolphin being called back to station and receiving no fish. The dolphins were trained on a regular schedule, consisting of one to two-hour sessions twice daily, five days a week. This was their primary source of interaction with the humans at the laboratory, and their sole source of sustenance. It was during these two training sessions that the dolphins received their daily ration of fish as rewards for correct responses, and when the fish ran out, the session was terminated.

Training consisted of pairing the signals with the objects, actions, or directions and practicing with a subset of the set of permissible combinations of signals. The remaining combinations were set aside for later transfer tests.

Based on the dolphins' above chance performance on these transfer tests, Herman et al. (1984) make a set of rather extraordinary claims, considering the impoverished language model and the minimal evidence (in the form of behavioral responses to simple action commands) to support the hypothesis that the dolphins had a tacit knowledge of a grammar and could comprehend sentences. In the next section we will examine these claims and the evidence provided to support them, and argue that many of the explanations by Herman et al. of the dolphins' behavior are not in conformity with Occam's razor.

We will begin with what the dolphins have demonstrated.

Sensitivity to Order

The first problem with this claim, as expressed in Herman et al. (1984), is that they equate word order with syntax: "Sensitivity to
word order (syntactic understanding)" is the heading of one section of their paper (p. 195). As has been emphasized frequently in this work, word order and syntax are not the same. Aside from this misconception, the authors provide a wealth of evidence that the dolphins indeed learned to pay attention to the order of the signals in the commands they received:

a) For those commands involving two objects, the dolphins were able to respond correctly when the objects were reversed, that is, responding appropriately to the difference between a command glossed as "take the ball to the basket" and one meaning "take the basket to the ball". The authors note that when the dolphins did err on this type of command, reversal errors (taking object2 to object1) were extremely rare.

b) For Akeakamai, there was one type of combination in which the direction symbol fell between two object symbols:

\[
\text{object2 direction object1 action2}
\]

These were commands of the type: BALL RIGHT HOOP FETCH, meaning "take the hoop on your right (as opposed to the one on your left) to the ball." The dolphin, Akeakamai, was able to choose the correct objects on a fairly consistent basis. In all combinations involving directional symbols, it was the case that the correct behavior was a linear response, that is, swimming off in the direction of the object whose symbol followed the directional symbol, but Herman et al. chose this type of combination as a demonstration of syntactic skills because they suppose that the dolphin might easily have erred by swimming in the direction of the preceding object since it is so close by in the
command sequence. They report that this rarely occurred. As mentioned above, generally, if one object was paired, then so were one or two more, yielding a set of two or three pairs for the dolphin to choose among. What Herman et al. do not tell the reader is how often it was the case that commands like BALL RIGHT HOOP FETCH were given when the ball was not paired. In this case, the dolphin could correctly choose the right hoop on a pragmatic basis alone (since no right ball is available). Given the facts that 1) directional symbols were only used when objects were paired; 2) symbol order called for a linear response to directional symbols, and this was true whether the command required one object to be acted upon or two; 3) there is no breakdown of data that would enable one to determine how often the correct solution could be arrived at on a pragmatic basis, it is difficult to see what caused the authors to jump to the following conclusion:

In summary, the understanding of the function of object names as direct or indirect objects, and of how modifiers may be attached to object names, further illustrate the considerable sensitivity of the dolphins to syntactic structure. (p. 199)

c) Another piece of evidence which Herman et al. list under this subheading of syntactic structure, is the fact that Akeakamai was able to respond to the action IN (to place one object in or on another object) correctly the first time it was used as part of a combination, BASKET HOOP IN ("put the hoop in the basket"). Certainly, this behavior demonstrated the dolphin's ability to generalize from the familiar FETCH combinations relating two objects to the new IN
combination (generalization will be discussed below), but it is not clear how this is "of ultimate importance for the demonstration of syntactic knowledge" (p. 199).

d) Another kind of evidence that the dolphins were sensitive to order was found in their responses to anomalous commands in a study by Herman, Wolz and Richards (1983). When the usual order for object-action commands was reversed, the dolphins did not simply ignore the anomaly and execute the command as if the order were normal. This constrasts sharply with evidence from a similar study with children by Hoban (1983), in which she found that children between the ages of two and six were able to respond to the essence of the command, paying attention to the semantic content rather than to word order, and suggests that for the dolphins order was of ultimate importance in their ability to respond to commands. When the order was disturbed, they were unable to use semantic information to carry out the command. Herman et al. attribute this difference to the fact that the dolphins were deliberately trained to pay attention to order, nonetheless, they would conclude that

the dolphins’ understanding of the function of word order, as revealed by their utilization of syntactic information, was well developed in comparison with that reported for very young children. (p. 198)

Thus, the dolphins were indeed sensitive to order. Exactly how sensitive they needed to be in order to respond to the commands in their respective artificial languages will be discussed later.
Ability to Generalize

The dolphins demonstrated two kinds of generalization. On the one hand, having been trained on a particular stock combination of signals, the dolphins were able to generalize responding to other instances of this combination, and even expansions of it. For instance, given the combination BALL OVER, ("jump over the ball") and taught the symbol for a new object or action, say HOOP, or TOSS, the dolphin could respond correctly to the first instance of the command HOOP OVER, or BALL TOSS, or HOOP TOSS. In addition, the dolphins responded correctly the first time they were given an expanded command of the type "take the ball to the right basket", or "take the right ball to the basket" after protracted training on commands of the types "jump over the right basket", and "t·ke the ball to the basket". Thus the artificial language was characterized, as are natural human languages, by openness and productivity in that new symbols could be added and immediately incorporated into the system. Note that this was a property of the language. The dolphins did not need to be aware of this property in order to respond correctly, yet Herman et al. attribute to them an understanding of "lexical novelty" and "structural novelty", and claim that "the type and levels of linguistic processing exhibited by the dolphins are substantial" (p. 192). Herman et al. compare the dolphin data with data from developmental psycholinguistic experiments, yet they fail to notice that no one has ever suggested that children are aware of the lexical and structural novelty in language when they learn new words and how to use them in new constructions.
The second kind of generalization which the dolphins demonstrated was the ability to generalize across a class of objects, thus responding to various exemplars of the object ball, for example. It is not clear why Herman et al. refer to this as "semantic processing", but at any rate, the ability to generalize is not strictly a linguistic capability. Although it may be necessary in language learning, it is hardly sufficient, and would seem to be a more general cognitive capability. A careful study of the parameters which form the basis of the dolphins' concepts would be of interest. Herman et al. provide no such data, so we have no way of knowing on what basis the dolphin determines "hoopness", for instance, or if it is able to categorize objects and actions into classes.

We turn now to those claims which are not supported by the data.

Tacit Knowledge of Linguistic Rules

Herman et al. (1984) claim that:

As in natural languages, tacit knowledge of the syntactic rules underlying the language was necessary for a correct interpretation of the function of lexical items in a sentence, and for an understanding of the unique semantic proposition being expressed. (p. 203)

According to Herman et al., the evidence for this claim can be found in Akeakamai's ability to respond to commands generated by the "inverse grammar" used to create commands given to her. Recall that commands involving two objects took the form of object2 object1 action2 for this dolphin. So BALL HOOP FETCH is glossed as "take the hoop to the
Further evidence is provided in the form of responses to "anomalous" commands in which the two object signals were then followed by an action of type one, such as TOSS or MOUTH, as opposed to type two (FETCH or IN). The dolphin’s response to these anomalous commands was to ignore the first object and perform the action on the second object in nine out of twelve of the cases reported in Herman et al. Rather than drawing immediate conclusions from these data, as did the authors, it is instructive to pause and consider what other behavior might be expected of the dolphin in such a case.

The command is of the type FRISBEE PIPE UNDER. Since the command is anomalous in that the two object symbols are not followed by the usual action symbols FETCH or IN, but instead by an action symbol which requires that an action be performed on only one object, there is no correct response. The dolphin responded by swimming under the pipe. This was applauded as an ability to "reinterpret the meaning of an early word in the sentence on the basis of a later occurring nonadjacent word" (emphasis in the original) (p. 188).8

But what if the dolphin had swum under both the frisbee and the pipe, or taken the pipe under the frisbee? Would not these have been more appropriate responses? Had the dolphin done so, in the former case, this would surely have been interpreted as knowledge of recursion in the grammar, as were similar responses by Phoenix (see below). In the case of Phoenix’s responses to such commands it is claimed that she was able to recognize an "unspoken" conjunction, interpreting the command as "swim under the frisbee and the pipe". If recursion is to be
incorporated into the language at a later date, would not this be the logical form for conjoined objects to take?

The problem with this sort of data is that it is simply inconclusive, and in fact tells us nothing about the dolphin’s "tacit knowledge of syntactic rules". What the authors are trying to get at is "dependency", but in this case, even if we could say that the interpretation of one word depends upon the interpretation of another, this would not demonstrate the kind of structural dependency that is characteristic of natural language.

In the case of the artificial languages used with the dolphins, there are never more object slots to be filled than agent (always the dolphin)\(^9\) and patient slots (for commands of type one), or at most agent, patient and goal slots filled by symbols for objects (in commands of type two). Although the dolphin must learn that the first object is always agent, (always itself), that the second object is always patient, and the third object is always goal (as mentioned above, the exact ordering differs for the two dolphins, but for each dolphin order is consistent), this has nothing to do with the sort of structural dependency we see in natural languages. In natural languages the subcategorization frame of the verb determines what theta-roles will be filled, and the argument hierarchy determines at what point in the tree each argument can be attached at deep-structure. Thus in the sentences below, the first object in the sentence can be agent, goal, or theme, where theta-role is attached to a structural position rather than position in the linear string:
1) John gave Bill a book.
   (AGENT) (GOAL) (THEME/PATIENT)

2) Bill underwent an operation.
   (GOAL) (THEME)

3) The ship sank.
   (THEME)

At surface-structure, provided that noun-phrases are properly governed and that other criteria are satisfied, still more radical differences in order are possible even where only a single sentence is involved. Thus "the postman" is the patient in all the cases below, even though it appears in different positions in the sentence:

4) The rabid dog bit the postman.

5) It was the postman that the rabid dog bit.

6) The postman was bitten by the rabid dog.

Movement is allowed because it is always possible to reconstruct "who did what to whom", yet there are specific conditions under which movement can take place, thus preventing ungrammatical sentences like 7) and 8) below:

7) *The postman the rabid dog bit.

8) *The rabid dog the postman bit.

both of which, it will be noted, are perfectly grammatical if the two objects appear in separate clauses as in 9) and 10):

9) The postman the rabid dog bit was given a rabies shot.

10) The rabid dog the postman bit became hysterical, never having been bitten by a human before.

Even examining the glosses of the commands given to the dolphins we can see how different natural language is from this artificial language.
In sentences like "take the ball to the basket", "the basket" is governed by the preposition "to", and it is this preposition which indicates what is the goal, not the position of the noun-phrase "the basket" in the sentence. In the system used with the dolphins, this most basic feature of structural dependency is lacking. Placing "basket" in the position formerly held by "ball" causes reversal of their semantic roles. This one to one correspondence between position in the string and function is consistent, a fact that limits the system so severely as to place it far beyond the realm of possible human languages. (See Bickerton, in press b for arguments against a similarly limited language proposed by Premack, 1985a.)

If the dolphins are tacitly aware of the grammar, then it should be possible to devise an experiment (depending only on comprehension) that could begin to test this. For example, Herman et al. posit a set of six rules for each dolphin’s grammar. Can the dolphins sort commands on the basis of their grammatical form? One way that linguists are able to determine just such a "tacit knowledge" in human speakers of a natural language is by asking them to sort sentences such as:

a. Jump over the ball.
b. Swim under the basket.
c. Go through the hoop.
d. Jump over the red ball.
e. Swim under the brown basket
f. Go through the white hoop.
g. Put the ball in the basket.
h. Take the hoop to the person.
i. Take the red ball to the basket.
j. Put the red ball in the basket.
k. Put the ball in the brown basket.
l. Take the ball to the brown basket.
m. Put the red ball in the brown basket.
n. Take the red ball to the brown basket.

Given these 14 imperative sentences, any speaker of English should be able to sort them into groups and subgroups as follows: (s)he will be able to tell you that a,b,c are instances of the same sentence form, and that d,e,f, although forming a second subgroup, go together with a,b,c to form one set of simple sentences containing only one object. Continuing, (s)he will recognize that g,h are of a new subtype, as are i,j,k,l and m,n, and that together g-n comprise a second set of simple sentences relating two objects. All of the above sentences are of the same type as the commands given the dolphins. For humans above a certain age these commands can be viewed either metalinguistically as linguistic objects, as in the above sorting task, or directly as instructions to carry out the command.

If the dolphins are really "tacitly aware of the grammar", then why not train them to sort sentences? Using the matching-to-sample paradigm, they would first have to be taught a command meaning "match." This should not be difficult, as they are able to perform match-to-sample tasks and have already been taught a command which initiates a similar task: the command MIMIC instructs the dolphins to mimic sounds
(some of which are actual command sounds) projected through the underwater speaker (Richards, Wolz & Herman, 1984). It might be difficult at first to keep the dolphin from leaving station to carry out a command as soon as it is given, but Batteau had the solution to this problem: an EXECUTE command. Until and unless this command is given, the dolphin goes nowhere. In the first stage of the command-matching training, the dolphin would be given a command, say BALL OVER, then the command MATCH (rather than EXECUTE) followed by two choices, one identical to the sample, both in form and content (BALL OVER), and the other maximally different, say FRISBEE FETCH BASKET.

Once the dolphin has learned to respond correctly in this paradigm, say, by pressing a paddle with its rostrum, something the dolphins have already been trained to do in another context (cf. Herman & Forestell, 1985), it could be taught to match BALL OVER with commands like HOOP UNDER, and finally, the test could be given in which the dolphin is asked to match commands across all six subtypes. The results might reveal how the dolphins classify commands. Would they sort them on a grammatical basis, or would they resort to differentiating between commands on the basis of number of symbols in the command, as did Herman et al. in their list of syntactic rules (Table 2, p. 147)?

Sorting sentences on the basis of syntactic form does not require an overt knowledge of grammar. Users of language are able to recognize commonalities of form without knowing the rules posited to generate those forms. If the dolphins have a tacit knowledge of the rules of the grammar, they should be able to do the same thing.
Recursion

Herman et al. point out that "recursive rules are a property of all natural languages and, in principle, allow for the generation of an infinite set of sentences" (p. 184). As evidence of the understanding of a recursive rule, they cite Phoenix’s responses to supposedly anomalous commands like FRISBEE FETCH THRU HOOP, to which she responded by taking the frisbee through the hoop. As additional evidence, they cite her ability to correctly respond to two commands, given one after the other. These were commands which required two actions to a given object, as in "mouth the hoop, touch the hoop."

While it is true that conjunction in natural language is one form of recursion, the dolphin hardly had to use any rule to perform the correct response. When I tell my dog "sit", then "down", does she use a recursive rule to know what to do?

In fact, this is a serious limitation of the grammar of the system used with the dolphins. There are no slots for recursive elements such as modifiers (although the slot for directional symbols might be expanded to be used this way) and embeddings, because the system does not have the hierarchical organization basic to any human language in which structures branch out, as if from a tree, allowing for the realization of recursion.

Linguistic Reference

Are the object names understood by the animal as symbolic representations of the objects? That is, do they really function as do words in natural languages? This has been an important issue in ALR.
In particular, Savage-Rumbaugh and her colleagues have focused on this issue (Savage-Rumbaugh, 1984; Savage-Rumbaugh et al., 1983; Savage-Rumbaugh, Romski, Sevcik & Pate, 1983) and come to the conclusion that, unlike in human children, reference does not simply appear in "language"-trained chimpanzees without giving them specialized training (Savage-Rumbaugh, Rumbaugh & Boyson, 1980; Savage-Rumbaugh, Rumbaugh, Smith & Lawson, 1980; Savage-Rumbaugh & Rumbaugh, 1978).

As evidence of linguistic reference in the dolphins, Herman et al. cite several sets of data. The first is the dolphins' ability to generalize across classes and context. As this was discussed above, it will not be elaborated further here except to say that generalization of a paired association is just that, generalization, which does not require the "name" to actually stand for the thing named.¹¹

The next set of evidence is from a "reporting" experiment with the dolphin. This will be discussed in detail below, but here it will suffice to say that the ability to refer to objects not present is one thing, while being told to take an action to an object which is not present, entailing searching for and not finding that object, is entirely different. While the former requires reference, the latter requires no more than the original paired association. The authors note that the dolphins seemed to develop specific search images for particular objects—"looking above the water surface to the high floating ball or the person at tankside", or "swimming near the bottom looking up for the flat floating frisbee or pipe" (p. 202). Is this evidence of linguistic reference? If the signals merely trigger a
response, then using a particular search pattern is part of the response.

It is not clear how one would establish the referential quality of the symbol (cf. Savage-Rumbaugh et al. 1980, 1983) within the comprehension mode. There are certainly some simple match-to-sample experiments using comprehension which might shed light on this. (See also the experiments suggested in Chapter VI). One possible experiment which comes to mind is:

a. show the dolphin a ball, then have the dolphin choose between another ball and a basket. (This is the basic match-to-sample paradigm, which would be used here to establish the "game" for the dolphin.)

b. show the ball, then have the animal choose between the symbol for ball and the symbol for basket.

We know that the animal can correctly choose the ball when given the ball sign, but does this work both ways? Are the signs and their referents interchangeable?

The final piece of evidence is the so-called "displacement" testing in which the dolphins were given commands with varying delays before the objects to be acted upon became available (after the delay all of the objects were thrown into the tank at once). It is important to stress once again (see Chapter I) the difference between withholding a response for a brief period, and the concept of displacement. A better test of displacement would be to provide the animal with information relevant to the tank world and see if the animal can respond appropriately. For example, if I am driving to work and hear on the radio that traffic is
heavy on the freeway, I can change plans accordingly. I have been made
aware of a state of affairs which I do not need to witness in order to
act upon. In the same way, it should be possible to tell the dolphins
where an object is, or merely that it is present (or absent) and see if
they can use this information to adapt their behavior. For example, let
us say that the dolphin puts the ball in the basket. Later, the ball is
surreptitiously removed from the basket and placed in another location,
say, in the far tank. The dolphin is then told that the ball is in the
far tank, and that it should get the ball and put it in the net. Does
the dolphin return to look for the ball in the basket where she left it
(as she would normally do), or is she able to use the information
provided by the trainer to alter her behavior?12 The problem is that
at present there is no provision in the Herman et al. system for telling
the dolphins anything other than what to do. It should be noted,
however, that Herman et al. do intend to remedy this situation shortly.
We await further results.
Summary and Implications

To recapitulate briefly, let us review what the dolphins' responses
have demonstrated, and what they have failed to demonstrate. The
dolphins have learned to pay attention to order, and the choice of order
is arbitrary. The form of the signal is also arbitrary in that one
dolphin was trained with gestures and the other with sounds. The
dolphins are able to generalize across objects in a class, from one
combination of elements to another, and across contexts. The dolphins
have not demonstrated that the symbols had a referential quality for
them. They have not demonstrated knowledge of structure and structural dependencies. They have not demonstrated knowledge of recursive rules.

The problem here is that all of the abilities which the dolphins have demonstrated are perhaps necessary for language learning, but certainly not sufficient. Those which they have not demonstrated are either lacking in the system (through no fault of the dolphins!), as in the case of structural dependency and recursion, or the proper experiments have yet to be conducted, as is the case for demonstration of referentiality. We can only say that the dolphins have learned to perform within the system in which they have been trained, but that the system itself has little to do with language. So it is not even the dolphins who have failed to demonstrate linguistic capacity, but the approach used to test them.

As discussed in Chapter I, the "list" approach to ALR is simply inappropriate and counter-productive. There are two serious problems with this approach. It is always possible to describe the properties of the system itself, but difficult to provide evidence that the animal comprehended these properties of the system. A similar difficulty can be seen in Stahlke's (Stahlke, Rumbaugh, Gill & Warner, 1979; Stahlke, 1980) analysis of the language of Lana chimpanzee (Rumbaugh, 1977b). It may be possible to neatly characterize the model, but since 1978 even the Rumbaugh's admit that Lana was not aware of the workings of the system, either tacitly or otherwise (Savage-Rumbaugh & Rumbaugh, 1980).

The second, more serious problem with listing "linguistic features" of systems used in ALR, was discussed in Chapter I. The listing
procedure is based on the incorrect assumption that it is possible to break language down into a set of independent features, each of which can be said to be manifested in particular types of performance. From here it is further assumed that if an animal can be trained to display the particular performance, it can then be argued that the animal "has" the linguistic feature in question. In this view, features can be built into the system one by one, and added to the repertoire of the animal until the animal can be said to "have language."

Language may not be a unitary phenomenon, indeed it was argued earlier that a modular approach is called for, but what seems to escape the researchers in ALR is that the modules are inextricably related—they must be integrated as a whole to make up language. Each module alone is no more a "part of language" than a single hydrogen molecule is part of water. Obviously it is a part, but unless it is combined with another molecule of hydrogen and one of oxygen it does not make up water. The relation of a single hydrogen molecule alone to water is as uninformative as the relation of the feature "openness" is to language. Perhaps this is better expressed by Lenneberg (1971), in discussing the "indivisibility of language knowledge" in the brain using the kaleidescope as a metaphor:

The patterns of a kaleidescope are infinitely variable; yet the range of this variability is delimited by the physical properties of the scope. No configuration can escape the limits. Thus, each pattern that is formed has its unique structural characteristics, yet every pattern is related to
every other. It is possible to take a single pattern and describe it, for instance, by describing the position and nature of each piece. However, the gestalt that is formed is not a simple aggregate of these descriptive items; removing certain pieces would change the structural characteristics of the gestalt in more complex ways than simple subtraction.

(p. 137)

There is a danger in misleading oneself too long, and the results can be seen in Herman and Forestell (1985). Having established (in their minds at least) that the dolphins comprehend language, Herman & Forestell go on to attribute to them the ability to report to us on their surroundings, based on the following experiments.

In one experiment, the dolphin was given the usual two-signal command in the form of object-action. As usual, the dolphin attempted to execute the specified action upon the specified object, but in this case the object might be absent. A typical response was to conduct a futile search and then return to station. The dolphin was then trained (in the second experiment) to return to station and press a paddle when it was unable to locate the object specified. At first, the dolphin transferred its behavioral response (the action of the command) to the new object (the paddle). Later, it was trained to change this response to a paddle press, as opposed to the original action specified.

In the final experiment, a new "interrogative" signal was introduced following the signal for the object (BALL ?), and the dolphin was required to press on one paddle if the object were present, and
another paddle if the object were absent. Based on the dolphin's performance in these experiments, Herman and Forestell conclude:

The present results evidenced the ability of a language-trained dolphin to report, through arbitrary responses, on the presence or absence of named objects not part of its natural world. This ability extended immediately to objects not used during initial training for explicit reporting, and was carried out effectively in response to both imperative and interrogative sentences. The reporting ability implied an understanding of symbolic references to objects given through the artificial languages used, including references to absent objects as well as to objects that were present. (p. 678-679)

The problem here, again, is one of interpretation. In the case of "language"-producing apes, our ability to assign meaning to their productions does not mean that they used the same processes to produce the words that we do to produce or comprehend them. We do not attribute language to the voice machine which reads out our purchases at the supermarket merely because they mean something to us. The difference between 1) unintentional communication (that of the dolphin above); 2) intentional communication without language (the crying child-- although crying can also be a simple response to a negative situation and therefore classified as unintentional); and 3) communication by language, must be recognized. Of course, one's attitude towards the importance of these issues depends on one's ultimate goals. Herman's research is funded in large part by the Office of Naval Research. The
dolphin may have learned a particular behavior, and we may be able to interpret this behavior as a "report", and there may be practical implications for these behaviors in naval reconnaissance activities, but one could hardly say the dolphin is reporting or deliberately communicating information to her trainers when she was merely responding to a cognitive map of her tank. One is reminded of the Lenneberg (1975) study in which high-school students were tutored in an artificial language system like that of Premack (1971a). Although the students could use the system to produce "grammatical sentences" within the system, they never realized the system was meant to be a language. They were able to generate proper constructions on the basis of non-linguistic rules which they deduced. 14

In considering the knowledge required of the dolphins in order to correctly respond to the commands within the artificial languages, a comparison with the Thompson and Church (1980) analysis of the language of Lana chimpanzee (Rumbaugh, 1977b) may be useful. The authors proposed that the chimp's behavior in producing "sentences" by pressing on the keys of a special "lexigram" keyboard could mainly be attributed to the two basic processes of paired associate learning and conditional discrimination learning (Thompson & Church, p. 313). They were able to simulate Lana's behavior using a computer model "in which the animal selects, depending on context, one of six stock sentences with fixed and variable elements" (p. 313).

Since the dolphins were not required to produce the commands, but only to respond to them, they had to learn even less. It is possible to
attribute their behavior to 1) paired associate learning; 2) learning a simple order rule for relating the two objects in those commands containing two object symbols. For one dolphin the first object in the command was the one taken to or put in the second object; for the other dolphin the opposite was the case. Like the strings which Lana produced, all commands given the dolphins were instances of the two stock commands: (direction) object action1 and object1 action2 object2, where each slot can be filled by various symbols of that category (see note 5). What is surprising is that Herman et al. are aware of the limited nature of the commands (as evidenced by their list of rules for generating them), yet they insist nonetheless that correct responding to them demonstrates linguistic competency. The Rumbaughs at least are aware of the limitations of the initial work with Lana:

It is true that Lana's initial sentences were the product of none other than operant conditioning. This was recognized and so reported (Rumbaugh & Savage-Rumbaugh, 1978; Savage-Rumbaugh, Rumbaugh & Boyson, 1978). For this reason they were called stock sentences. There was no claim, ever, that the "words" which comprised them initially had linguistic significance. (Savage-Rumbaugh & Rumbaugh, 1980, p. 317)

As noted in Chapter III, behaviors which are similar require at least an attempt at a common explanation. How then do we account for the results of work by Schusterman (Schusterman & Krieger, 1984)? Schusterman based his initial work with California sea lions (Zalophus californianus) on that of Herman and his colleagues. Although still in
the early stages (in the 1984 report Schusterman had yet to include commands relating two objects), the results are remarkably similar to those of the dolphins. The sea lions are capable of responding correctly to commands of the form (attribute N) object action (where N ranges from zero to two) at well above chance levels.

It is generally accepted that the small-brained sea lion is of limited intellectual capacity (unfortunately, no sea lion EQ is available for comparison here, but pinniped EQ values listed by Eisenberg (1981) average about 1.3—hardly impressive), yet it is able to comprehend commands in an artificial language specifically designed to test the large-brained dolphin. Of interest here is the fact that Schusterman feels no need for a linguistic explanation of his data, instead referring to the sea lions' behavior as "symbollic matching". Schusterman's work may be the first step in demonstrating the following two facts: 1) the sort of behavior exhibited by the dolphins does not require much in the way of brain power, and 2) if we want to test the ability of an animal brain such as that of the dolphin, the model we choose will have to be more complex than that chosen by Herman.

In sum, Herman and his colleagues (1984, 1985) have presented some data from psychological experiments with bottlenosed dolphins. There is nothing even remotely reminiscent of linguistic skills in the behavior of the dolphins. At the same time, this is not the end of the road, but only the beginning. While Herman's work has not demonstrated any linguistic capacity in the dolphin, his work does provide evidence of
untapped cognitive abilities, and indicates that the time has come to reconsider our approaches and reexamine our models.
Chapter IV Notes

1. Dolphins, with their slick, wet, sensitive skin, are naturally sensuous animals which are easily aroused. Peter's sexual approaches to Ms. Howe are hardly extraordinary behavior, and therefore not surprising in the least.

2. This is impressive, considering that the dolphins were not trained in the basics before attempting these transfer tests. Normally in this sort of training, where eventual generalization is to be required, it is necessary to introduce flexibility from the start. Thus one trains the animal to respond to a variety of exemplars of a given object in a variety of contexts. Only after this initial training period is the transfer test attempted with new objects and new situations.

3. Note that this refers to ordered combinations, rather than permutations, so that for instance two objects and two actions yield four object-action combinations, as opposed to eight permutations. As the reader will recall, a central criticism of the Gardners' work was that word order played no role for Washoe, and two-word combinations were accepted in either order, but recorded according to English word order conventions. Herman hoped to avoid this problem in his comprehension study by building order into the system from the start.

4. The dolphins were both trained and tested on directions as pairs, yet the authors claim the dolphins understood the notion "surface" "bottom" or "right" "left" as unique and meaningful words. An interesting test of this would be to test the dolphins' ability to carry out a command referencing say, the "bottom hoop" when a surface hoop is not present.
If she were unable to successfully execute such commands, this would indicate that "bottom" is not "bottom" to her. That is, it is not a unique and independent word, but is somehow dependent on the other half of the pair, "surface." Unpublished data indicate that this is the case, and that in fact these symbols do not act as modifiers of the object as the authors claim. Rather, they instruct the dolphin to move in a relative (as opposed to an absolute) direction. So where the authors claim that Phoenix understood "bottom" and "surface" as attributes of particular objects, more likely, she understood "lower" and "upper" as directions in which to swim. This interpretation makes more sense when one considers the natural environment of the dolphin, in which "upper" and "lower" could be meaningful concepts. Awareness of one's relative position in the ocean depths would make more sense than absolute position.

5. Rule 2 differed slightly for the second dolphin (Akeakamai):

\[
\text{rule 2: } (\text{direction}) \text{ object2 (direction) object1 action2}
\]

where only one direction could be filled at a time. The authors provide no explanation for the fact that Akeakamai was unable to respond correctly to five-symbol commands-- those in which both direction slots were filled, such as: RIGHT BALL LEFT HOOP FETCH ("take the hoop on your left to the ball on your right.")

6. In this chapter, all caps will be used for commands given the dolphins.

7. Throughout their report, Herman et al. use linguistic terminology with no concern for its appropriateness. As Premack so poetically
describes it, these terms "descend upon the article in a wild flurry, assailing nearly every page." (Premack, 1985b, p. 16).

8. The authors argue that since normally two object symbols occur only in commands with actions of type two (FETCH or IN) in this case, the fact that the two object symbols were then followed by the symbol for an action of type one required the dolphin to reinterpret the sequence—changing her interpretation of the function of FRISBEE from goal to "garbage" and discarding it. That is, they claim that processing of the third symbol in the string FRISBEE PIPE UNDER caused the first symbol to be ignored, thus their use of the word "nonadjacent".

9. It will be recalled that all commands to the dolphin are prefaced by a "name" assigned to that dolphin, but since commands only address the one being commanded, "agent" is redundant here anyway.

10. Actually, although the rules used can generate six subtypes of commands, only one dolphin was successful in executing commands of five symbols in length. (see note 5 above).

11. Although the dolphins have been trained using a variety of shapes and sizes of exemplars, the range of their generalization has never been tested.

12. As with any other suggested experiment described only superficially in the present work, the reader is warned not to assume the actual design could possibly be so simple. It is always necessary to implement the appropriate experimental controls to rule out the chance of the animal relying on something other than the input with which we are concerned in order to solve the problem. In this experiment, for
example, we would have to make sure that the dolphin could only know that the ball was no longer in the basket by comprehending the information provided by us in "telling it" that the ball was in a different location.

13. Stahlke's conclusions are so incredible that I simply must quote him here:

We have demonstrated that the linguistic competence of at least one ape language analog requires a set of formal devices including a CFPSG, [context free phrase structure grammar] a CSPSG, [context sensitive phrase structure grammar] and a TG [transformational grammar]. Precisely these formal devices are among the formal universals of natural language. While both natural and ape language may yet be shown to require some further formal devices, it is clear from those presented above that, formally, natural language and ape language are qualitatively very similar. (Stahlke et al., 1979, p. 104)

I wonder if Stahlke is the reason most animal language researchers shy away from asking linguists for help in their projects. His disastrous treatment of the Lana data serve to illustrate that one needs both a knowledge of linguistics and a background in psychology and animal behavior before taking on ALR.

14. No further details are available about this study which was presented at the Seventeenth International Symposium of Neuropsychology at Puyicard, France in June, 1972. A brief mention of the study can be found in the journal Neuropsychologia (see reference list).
15. At the time of this writing (February, 1986) Dr. Schusterman has just informed me that the artificial language he uses with the sea lions has been expanded to the point where it is possible to string together seven symbols as in SMALL BLACK BALL LARGE WHITE CUBE FETCH ("take the large white cube to the small black ball.") While he has not yet tested the sea lions on strings of this length, he has tested them on strings of five and six symbols in length, and he tells me that their accuracy levels are very high. It is interesting to note that the symbols for size and color (LARGE/SMALL, BLACK/WHITE) can occur in either order, so the animals respond correctly whether told to use the "small black ball" or the "black small ball."

16. There is an important difference between this and the artificial language used with the dolphins, in that allowing for multiple modifiers opens up the possibility of recursion in the grammar, but, as is the case with the dolphins, these symbols are only used when the objects are paired, so it is difficult to know how the animal interprets them, and it is not at all clear that they are in fact attributes which modify the objects.
CHAPTER V
DISCUSSION

In Chapters II, III, and IV data were presented on chimps learning a sign "language", dolphins learning an artificial "language" and a human child learning English. In this chapter an attempt will be made to account for the fact that neither chimps nor dolphins learned even the basics of language, yet, at the same time, for a short period of about four months, a child and a chimpanzee exhibited similar behavior in their production of utterances. The chapter is organized as follows: In the first section, a hypothesis will be proposed to account for these facts; the second section goes beyond the hypothesis to relate it to current models of child language acquisition; in the third section, relevant evidence is presented, including a discussion of what additional evidence is needed to further support the hypothesis. The final section discusses specific predictions of the hypothesis.

How are we to explain what animals can and cannot learn about language, and what children can and do learn? Beginning with what animals have demonstrated, as we concluded in Chapter IV, chimps and dolphins have been tested in some basic psychological paradigms (learning sets, paired associate learning, match-to-sample) and have shown that they are capable of solving problems of varying degrees of difficulty, but they have not demonstrated that they use language or knowledge of linguistic rules to solve these problems-- nor do the problems given require use of language to solve them. The input to the animals is part of the problem. Given what we have seen thus far, one
might say that animals have not learned human language because they have not been taught human language. But this is a little bit like saying that we can not know what happens to a person who is run over by a steam roller until we test it. We cannot say for certain that learning language is beyond the capability of animals until we actually test it by teaching an appropriate language to a large enough sample of members of various species, using several methods of teaching. But before opening a chain of animal language schools across the country, we can make a set of recommendations and predictions. Why it is that in nearly three decades of ALR the language used as a target has never been an acceptable human language (to linguists) (except of course in the case of the home-raised chimps exposed to English by their surrogate families) is a question in and of itself. More importantly, what needs to be explained is why, despite this oversight, a chimpanzee exposed to such a training program (using Pidgin Signed English as the target) produced utterances which are remarkably similar to those of a child acquiring English, both in terms of structure and content. Given the present state of knowledge in ALR and child language research, it is possible to posit an empirical hypothesis which can be tested in further studies of animals and children and comparisons of the two.

The Two-Stage Hypothesis

As discussed in Chapter I, two characteristics which set language apart from non-linguistic communication are reference, and the displacement which it allows, and syntax, and the structural complexity which it allows. Recapitulating what we know from earlier chapters:
chimps tutored in ASL signs with English word order did not learn syntax, and there is no evidence that they learned reference either. The signing projects discussed in Chapter II concentrated exclusively on production of utterances, attributing full referential meaning to what the apes signed. Since the signs have referential meaning for the human teacher, it is hard not to interpret them according to one’s own use. This problem of overinterpretation is a serious one for any ALR project, but particularly in these signing projects where there were no tests of comprehension, and certainly no tests for reference. Although displacement is evidence for reference, it is still not clear how one would go about testing for reference (but see Chapter VI for some suggestions), but in the absence of any attempt at such tests, we can only say that the referential quality of signs was not demonstrated in the signing ape projects.

In Chapter IV we discussed the recent work of Herman and his colleagues with a pair of bottlenosed dolphins tutored in an artificial system. It was concluded that the dolphins learned to pay attention to order but did not demonstrate knowledge of reference or syntax. In Chapter III the structure of utterances produced by Seth from age 19–23 months and those of Nim was found to be similar (both lacked syntax), as were discourse strategies employed by both. Although reference was not specifically compared in Chapter III, Nim is considered not to have developed an understanding of the referential quality of signs he used (Petitto, 1985; Sanders, 1980). Reports on the Nim Project argue that Nim learned to use signs only as a means of achieving a desired end,
such as obtaining an object which was withheld from him, but that he never learned to use signs as symbols. According to Petitto (1985), Nim did not use signs to describe the world. For him signs were not "words"; they did not have meaning separate from their referents, but were understood solely in the context of the objects or situations to which they referred. The special relationship which humans develop between symbols and their referents-- that the symbol continues to have meaning even when the referent is removed-- seems to have been lost on Nim.

Whether or not Seth used words referentially during the period in question is difficult to determine from the transcripts alone, but it is my subjective opinion, having listened to numerous audio tapes of Seth interacting with his father, that words did not begin to take on a truly referential quality until after the period analyzed in the present study. Although Seth does speak about objects and events removed from the present (as when he answers "honk" to Daddy's question "what did the geese say?"), which would seem to indicate displacement, these utterances are often routinized responses, or words connected by association. By triggering one word in such an "association bundle", several others may be released. An example of true referential use of words in displacement can be seen much later in Seth's acquisition of English. For example, at 32 months Seth and Daddy discuss a visit to the zoo. Seth's description is multidimensional, covering which animals they met, where and what they fed them, what colors they saw, what sounds they heard at the zoo, and so on. Seth's use of language at 32
months enables the listener to experience the zoo visit as well. Notice that this is not merely due to the fact that Seth now speaks in multi-word utterances. The same effect could have been achieved at the one-word stage if Seth had had referential use of words.

We may agree then, that the thorny question of reference is still up in the air. It is not clear whether or not apes (or dolphins) can learn to use symbols referentially; and, perhaps a more serious lacuna for students of child language, we have little knowledge of when children learn reference (but see works by Bates and her colleagues which address this issue: Bates, 1976; Bates, Benigni, Bretherton, Camaioni & Volterra, 1979; Bates, Bretherton, Shore, & McNew, 1983). On the other hand, we may agree that the facts on syntax are more clear (even if the reasons behind them are less-so), allowing us to posit the following hypothesis:

**HYPOTHESIS:** First language acquisition involves at least two stages; the first stage is a non-linguistic stage which may be shared by other species. The second stage is a linguistic stage which is limited to human children.

An explanation of the choice of the term "non-linguistic" might be helpful here. I chose "non" as opposed to "pre" because it is not at all clear that this and the stage that follows it are two halves of a continuous developmental sequence. Although certainly this stage precedes the next, it may only be in this sense that it is "pre"-linguistic. The term "linguistic" is used here to refer to the stage
when both reference/displacement and syntax/complex structure are part of the system. Thus "non-linguistic" is meant to convey the fact that the system at this stage lacks the two characteristics, reference/displacement and syntax/complex structure, which were proposed in Chapter I as hallmarks of human language.

Models of Child Language Acquisition

In the sections which follow, we will discuss support for this hypothesis, both from the data and analyses presented in earlier chapters, and from other areas of child language research. In addition, further possible sources of support will be discussed, and possible evidence against the proposed hypothesis will be considered. In this section we turn to models of child language acquisition which have been proposed in the literature. While most were never meant to be applied to ALR, it is possible to consider how well they fit the current data.

Abrahamsen, Cavallo and McLuer’s Three-Stage Model

The first two models to be discussed break language acquisition down into three rather than two stages. Abrahamsen, Cavallo and McLuer (1985), in their discussion of the "sign advantage phenomenon" (it has been claimed that there is an advantage in acquisition of signs based on evidence that manual signs are acquired before spoken words) speak about the following three stages of language acquisition: 1) gestural 2) symbolic 3) linguistic. Briefly, during the gestural stage (10-15 months) the gestures that children make are directly related to the present context, as opposed to being related to other gestures. In the symbolic stage (16-19 months) gestures take on a representative function
and can be considered as symbols for objects and events in the child's world. Finally, in the linguistic stage, (20-26 months) gestures (vocal or manual) attain the status of words as lexical and syntactic connections develop between them. According to this view then, there is an intermediate stage, in which gestures function as symbols but are not considered linguistic symbols because of the lack of syntax.

Berman's Three-Stage Model

Berman (1985), in her paper on the place of grammar in language acquisition, also speaks about three stages. The first stage (pre-grammatical) is when children are able to recognize the sound-meaning relationships necessary to learn words, and can even string them together into linear strings. This stage is pre-grammatical in that it forms the basis for all subsequent learning of semantic and syntactic structures, yet, according to Berman, until approximately two years of age these utterances are non-grammatical in the sense that combinations of units are not structurally dependent or hierarchically related.

Only some time after the age of two do children move into the "grammaticization" stage in which they "acquire the context-free, structure-bound set of rules which constitute the grammar of their language" (p. 5). In the third stage of language acquisition, the grammatical knowledge which is learned in stage two is applied in a variety of discourse situations. This is where children learn to "use" their grammar, and when the grammar becomes sensitive to the context in which it is used. For our purposes, in comparisons of child language acquisition with that of apes and other animals, only the first two
stages discussed by Berman are relevant, the "pregrammatical" and the "grammaticization" phases. According to Berman's description then, Seth would have been in the pregrammatical stage throughout the period studied here (19-22 months) and so would have Nim.

Petitto's Two-Stage Model

Laura Petitto was one of Nim's trainers and has an extensive background in ALR. Her arguments (1985) for a two-way split between "pre-linguistic communication" and "linguistic expression" take this knowledge into account. Petitto argues that not only are these two very different stages of development, but further, that they are discontinuous. Her argument is based mainly on her studies of the acquisition of ASL by deaf children, and the discontinuity evidenced in those studies as the child passed from the pre-linguistic to the linguistic stage of acquisition of signs. This discontinuity would explain why, according to Petitto (and consistent with the present findings), children and chimps appear similar for a time (until 14-18 months according to Petitto), but that between 18 and 22 months (somewhat earlier than Seth) there is a fundamental reorganization in the child's knowledge which signals the cut off point between non-linguistic and linguistic systems.

Bickerton's Two-Stage Model

Bickerton (in press a) holds a similar discontinuity view in his two-stage model of language acquisition and evolution. According to Bickerton, there is a "protolanguage" stage which precedes acquisition of "genuine language" in the second stage. Bickerton also takes data
from ALR into account. Because Bickerton devoted an entire paper to the
presentation of his two-stage model, there is more of it available for
consideration here. As Bickerton's model seems most complete and
consistent with my data, this model will be considered here in somewhat
more depth.

It is only within the last decade and a half that pidgins and
creoles have become serious objects of linguistic inquiry, with
researchers focusing on the processes of pidginization and creolization
more than on the product of these processes (Bickerton, 1975a; DeCamp &
Hancock, 1974; Hymes, 1971). Even more recently, pidgins and creoles
have begun to play a role in the study of how languages are acquired,
with authors considering both as special cases of language learning
(Anderson, 1980). From there the discussion has turned to one of pidgin
as a special case of second language learning (Anderson, 1981;
Bickerton, 1981b; Gilbert, 1981; Schumann, 1981), leading finally to a
debate as to whether or not creole should be considered a phenomenon of
second language learning as well (Bickerton, 1980, 1983a; Sankoff, 1983;
held all along that creole is an interesting phenomenon of first (as
opposed to second) language acquisition, but he has gone further than
this observation to propose that creolization is the phenomenon of
language acquisition, and the best place to look for universals of human
language.

Bickerton arrived at this position after research on pidgins and
creoles revealed that they are not mere mirror images of the same
process (the prevailing view to emerge from the conference which produced Hymes, 1971), where pidginization is a simplification process, and creolization is seen as the corresponding complexification. While it is true that pidginization in all cases involves simplification, it is important to note that the result is not a unitary phenomenon, but rather "numerous idiosyncratic varieties of a reduced nonnative version of the target language" (Anderson, 1983, p. 210). Creolization, on the other hand is a unitary phenomenon according to Bickerton. Bickerton (1975b, 1977, 1981a, 1982, 1983b) provides evidence that all creoles exhibit the same steps in the process of complexification. This requires the child to follow a course which is not mapped out by the input he receives from adults around him (who are assumed to be speaking pidgin), and therefore must be mapped out by a "template" in the brain. Bickerton (1984a) has described this template as a biological program ("bioprogram") for language which guides the child's acquisition of language. Recently, (Bickerton, 1984b) he has suggested that the study of creoles may lead to a better understanding of the nature of parameters in universal grammar; and a comparison of pidgin-like phenomena versus "genuine" forms of human language has lead him to posit a two-stage model of the evolution of the human language faculty (Bickerton, in press, a).

Bickerton (in press a,b) juxtaposed several phenomena of language: the speech of early-stage pidgin speakers; the speech of children under two; the output of linguistically deprived children (which includes both "wild" children like "Genie" (Curtiss, 1977), and deaf children whose
parents are not signers of ASL, and who are not exposed to a language model in the early stages of their acquisition (Feldman, Goldin-Meadow & Gleitman, 1978; Goldin-Meadow, 1979; Goldin-Meadow & Feldman, 1977); the speech of adult early-stage second-language learners (Schuman, 1978); the output of "language"-trained apes. Bickerton proposed that in fact these are not phenomena of language at all, but warrant examination under a separate heading. Bickerton termed these phenomena "protolanguage". We may add to this list of protolanguage phenomena the following three members: baby talk (that sometimes used by adults when addressing babies), foreigner talk (that sometimes used by adults addressing non-native speakers of their language— which at its most extreme form may actually become a pidgin) (cf. Ferguson, 1971 for a discussion of baby talk and foreigner talk), and now animal talk (which is what psychologists produce when they try to teach animals language). In a second group which Bickerton terms phenomena of "genuine" language, we may include creole languages; the speech of children between two and four (although, as we saw in Chapter III, perhaps an MLU of above two is a better gauge than simply saying age two); the speech of second-generation deaf children, whose parents' speech was classified as a member of group I (these children manage to learn ASL although their only input is the pidgin-like sign language of their parents (Newport, 1982) (see Figure 2 below). All of the forms of protolanguage in group I share commonalities of form, beginning with the fact that utterances either have no structure, or at best consist of a linear stringing
together of words, as opposed to the hierarchical branching structure of natural language.

<table>
<thead>
<tr>
<th>Protolanguage</th>
<th>Language</th>
</tr>
</thead>
<tbody>
<tr>
<td>Early-Stage pidgin languages</td>
<td>Creole languages</td>
</tr>
<tr>
<td>Child speech below MLU 2</td>
<td>Child speech above MLU 2</td>
</tr>
<tr>
<td>Speech of linguistically-deprived children</td>
<td>Output of children of</td>
</tr>
<tr>
<td></td>
<td>linguistically-deprived</td>
</tr>
<tr>
<td></td>
<td>children</td>
</tr>
<tr>
<td>Speech of early-stage adult</td>
<td>Speech of successful</td>
</tr>
<tr>
<td>second language learners</td>
<td>second language learners</td>
</tr>
<tr>
<td>Baby, Foreigner &amp; Animal talk</td>
<td>Adult speech</td>
</tr>
<tr>
<td>Output of &quot;language&quot;-trained apes</td>
<td></td>
</tr>
</tbody>
</table>

Figure 2. Phenomena of "protolanguage" and "genuine" language

As illustrations of group I phenomena, reproduced below are samples of Seth’s and Nim’s utterances from Chapter III, and samples from Bickerton (in press a) of the speech of Genie and that of sea captains speaking a pidgin.

N: BERRY
T: DICK EAT--

N: DICK *EAT.
T: ME--
T: *EAT.
T: NIM--

N: BERRY NIM--
T: EAT.

N: EAT.
T: NIM EAT YOGURT, *YOGURT--

N: *EAT
T: YOGURT, *YOGURT--

N: *YOGURT NIM.

(Sanders, 1980, p. 47)
N: AFRAID.  T: SEE RAIN OUTSIDE?
N: HUG.  T: YOU AFRAID NOISE?
N: MARY, AFRAID.  T: WHAT YOU THINK ABOUT NOW?
N: HUG.  T: WHAT PLAY?
N: PLAY.  T: (later) YOU TIRED NOW?
N: PULL, JUMP.  N: TIRED.
N: SLEEP, BRUSH-TEETH.  N: SLEEP, BRUSH-TEETH.
N: HUG.

(Terrace, 1979, p. 186)

S: Tanya
S: baby, uh Seth.
S: tape
S: *Seth
S: me-ee!
S: ha' baby.

(D: Baby Seth?)
(D: Who is Seth?)
(D: *Who is Seth W.?)
(D: Awright! I thought so. (5 sec.)
(D: Tonya has babies.)

(Seth, 19.2 months old)

S: brush teeth.
S: brush teeth?
S: wake up?
S: no.
S: please. (means yes)
S: jump?

(D: Is that your brush?)
(D: Uhuh.)
(D: Did you wake up this morning?)
(D: (later) You want any more oatmeal?)
(D: Okay, you want down?)
(D: You want more milk?)

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S: milk? milk?
(Daddy, 21.2 months old)

D: You want jump down?

S: Oatmeal.
S: Oatmeal.

D: Daddy's gonna go fix your breakfast.

S: Hungry?
D: Yeah. Yeah. Let's go do that.

S: Please. (means "yes")

(Daddy, 22 months old)

S: Ya hungry?

R: Nothing. Four half.

D: Ya hungry?

Want Milk.
Mike paint
Big elephant, long trunk.
Applesauce buy store.

(Genie's utterances, from Curtiss, 1977; taken from appendix of Bickerton, in press a)

G: Genie have yellow material at school.

M: What are you using it for?


(Conversation with Genie, from Curtiss, 1977; taken from appendix of Bickerton, in press a)

R: What say? Me no understand.

N: Expensive, Russian -- goodbye.

R: Nothing. Four half.

N: Give four, nothing good.

R: No brother. How me sell cheap?
Big expensive flour on Russia this year.

(Conversation between Russian & Norwegian sea captains, from appendix of Bickerton in press a)

Bickerton describes four ways in which it can be demonstrated that all these samples, which may superficially resemble speech produced on
occasion by adults, or other producers of group II language phenomena, are in fact the product of a different source altogether. Bickerton’s arguments can be briefly summarized as follows:

1. breakdowns in ordering-- although the utterances of Genie, for example, tend to have consistent word order, there are utterances which demonstrate a mismatch between thematic role and structural position, so that Genie may produce "applesauce buy store", whereas members in group II would be prevented from doing so by the fact that for them there must be a consistent match between thematic role and structural position in the sentence.

2. omission of constituents-- Bickerton points out that speakers whose utterances are members of group I may omit consituents which are not necessary for semantic interpretation. Thus Nim and Seth produce utterances like Seth’s "ha’ baby", and Nim’s "afraid" or "pull, jump", which have no grammatical subjects. As Bickerton notes, in "genuine" language it is impossible to simply leave out constituents at random, or on purely semantic grounds. Instead, in "genuine" language gaps are constrained as to where they may occur, and further, by the fact that they must be systematically related to some constituent which appears in the sentence.

3. Lack of expansion devices-- modification of simple constituents is limited to the simplest kind in protolanguages. Genie may say "blue paint", and the Russian may say "big expensive flour", but like Seth’s frequent use of "chocolate milk", or "Pink Eddie" (not shown above), these are more likely formulaic in nature rather than demonstrations of
constituent modification, which allows for further expansions of the form: "the large, black, floating bottle." In addition, there is no productive use of recursive embedding, which produces sentences like "this is the cat that ate the rat that killed the mouse that lived in the house that Jack built". Adults speaking to children, foreigners, and animals may also avoid these kinds of constructions. Both of these devices for grammatical expansion are built into natural language systems by virtue of the tree structure which organizes such structures hierarchically, and which is characteristic of all members of group II. 4. lack of grammatical morphemes--all of the members of group I lack the grammatical bits that are necessary in the syntax of a language--those words or markers which "glue language together", such as determiners, auxiliary verbs, and inflections. A foreigner learning English may produce "I think he often" instead of "I think of him often", leaving out the preposition, and failing to use the accusative form "him". Adults speaking to babies often produce utterances like "want bottle?", "go potty", "time go bye-bye" (but of course they do not use only "baby talk", they go back and forth between normal speech and this sort of "condensed" speech), and systems used in ALR categorically delete all but major constituent categories.

What characterizes the members of group I is that the protolanguage utterances seem to be generated by a semantically, rather than a syntactically driven device. As long as the semantic slots which are necessary for interpretation are filled, and it is possible to make sense of the utterance, it is an acceptable utterance. The utterances
in group II, on the other hand, are characterized by a hierarchical organization in which constituents are structurally dependent upon one another. Aside from the apes, all of the producers of group I output have counterparts who produce group II output. Children under approximately two and a half grow into children over two and a half, and eventually reach adulthood and use adult language. Second language learners may leave the early stages and often go on to learn the target language. The children of deaf children who have no linguistic model and produce only a pidgin sign language go on to have children who creolize this pidgin input and end up with ASL. Baby talk, foreigner talk, and animal talk are all temporarily used by adult speakers who can use normal speech if they choose to. Pidgin languages may be replaced by creoles as a generation of children is born for whom the pidgin input is inadequate to satisfy their language needs.

All of these facts suggest three things:

1) language acquisition takes place in at least two stages
2) the first stage is not "linguistic" development at all, rather it is a general cognitive development stage, shared to some degree by animals.
3) the second stage involves an ability to perform linguistic analysis, which would seem to be unique to humans.

Chomsky (1980) has described the human language capacity in terms of two components which are useful here because they correspond to these hypothetical stages. Chomsky spoke of a conceptual component and a computational component. The first is a general cognitive capacity which is not specific to language, but does interact with the language
faculty in language acquisition. The computational component proposed by Chomsky is conceived as unique to language, and is assumed to "provide the rich expressive power of human language" (p. 6) by determining the rules of structural dependency in the language.

Bickerton (1984a) hypothesized the existence of a language bioprogram which unfolds over time in the language acquisition of the child. Where Chomsky speaks of a language organ (which implies that such an organ may be located somewhere in the human brain), Bickerton's hypothesis suggests organic growth: as the brain follows its natural development (and more and more neural connections grow in), the genetically endowed language bioprogram is activated. Bickerton is very specific about the character of the bioprogram. Based on extensive comparative research of pidgin and creole languages, Bickerton proposes that creole languages "derive from a single grammar with a restricted list of categories and operations". This grammar is that of the bioprogram, which "provides a skeletal model of languages which the child can then readily convert into the target language" (p. 173).

Borer (1983) suggests along these lines that there is only one human syntax (according to Bickerton, 1984a, this would be that which is specified by the bioprogram) which children apply to their language acquisition, converting it into the target language as they learn the particular idiosyncratic morphological system of their native language.

Given our present data, we see that it provides a time frame in which to place the two components proposed by Chomsky, and it allows us
to pinpoint the activation of Bickerton's hypothesized language bioprogram to a specific period in the child's development. Rather than simply speaking of two components in the brain of the human language user, in the present model it is hypothesized that these components are not both present at birth. They grow in in two distinct stages, with the conceptual component feeding into the computational component as its main input. Further, while the conceptual component may begin to be active soon after birth, as the child begins to explore the world with his senses, the computational component only grows in sufficiently for its presence to be evident around the age of two and a half.6

In the case of Seth, activation of the language bioprogram can be further pinpointed to the period between 24 and 28 months. What the data on Seth and Nim together tell us is that animals may be able to achieve the first stage, in which the conceptual component is used in communication, and it would seem that every child passes through this "pidgin" stage on the way to language acquisition. It is not however, until the computational component grows in and the language bioprogram is activated that, as in the case of the move from pidgin to creole languages, the child is able to use the pidgin input as fodder for the creolization mechanism, whereby he begins to produce the utterances of "genuine" language. In the absence of additional input, the child will create a creole, but, as Bickerton points out, as long as the child has additional linguistic input (from those around him speaking the genuine language target), the skeletal model of the bioprogram will be converted into the local target language.
Newport also proposes that child language acquisition can be represented with a two stage model, as can be seen in the following passage from Newport (1982):

while the early stage of acquisition involves organizing the lexicon in terms of individual lexical items, each related to its meaning, the subsequent stage requires that the child perform a distributional analysis across (some portion of) the lexicon, deriving morphological components by analyzing form-meaning relations shared by numerous lexical items across the system; morphological analysis cannot be performed on individual form-meaning pairs, but only across a set of such pairs. Moreover, it requires building paradigms into which the individual items fit. (p. 482)

The difference here is that the two stages have been related to Chomsky's conceptual and computational components, Bickerton's bioprogram, and finally, to ALR. Here, the view of the bioprogram is that it is a language-specific *portion* of the computational component, or a program for the application of the computational component to first language acquisition. Newport (1982) argues that the paradigmatic organization that occurs is not limited to the domain of language:

In short, my claim (see also Karmiloff-Smith, 1979b,c) is that young learners tend to move from organizing units independently of one another to organizing them in paradigmatic systems; that they do this outside language as well as within language; and
that they do this even when the data they are organizing in fact do not quite fit into neat paradigms. (p. 483)

So according to Newport, if there is a computational component which is used for paradigmatic organization, its use is not limited to the domain of language. I would tend to agree with her to a point. I am arguing that the original use of the computational component, as hypothesized in Chapter I, was specifically for organizing language, but as is often the case in biological preadaptation, its use is no longer limited to language. What is being argued here is that while the computational component as a whole may not be specific to language, one portion (Bickerton's bioprogram for language acquisition) is language specific. Thus while the entire computational component may not be dedicated to processing of language, it is necessary for language, and it does seem to be unique to humans. To clarify this point further, it is necessary to distinguish between the position held here and that held by Newport, as opposed to Chomsky's. While I am arguing for a one-to-one correspondence between Chomsky's conceptual component and what I consider the cognitive component, I do not see the same one-to-one relationship between Chomsky's computational component and a linguistic component. In this respect I agree with Newport that some functions of the computational component are not specific to language, but at the same time others (for instance formal system building) are language specific.

What is indicated by the ALR studies thus far is that 1) animals may, through years of tutoring, develop a use of some organizational
aspects of the conceptual component, and they may learn to use their conceptual component to communicate with us, 2) but they do not ever reach stage II where the computational component has grown in, and they do not have access to the language bioprogram, because it is evolutionarily beyond their development. By studying their ability to learn a hypothetical first human language (based on what we now know about "radical" creoles; see Chapter VI), we may gain insight as to what sorts of developments took place in the first stages of the creation of the computational component— that is, what parts were immediately necessary for language, and what could have been handled by a conceptual component alone.

The main difference in the models of child language acquisition discussed above are in their treatment of symbolic use of words—treating reference/displacement as a separate stage (Abrahamsen et al. (1985) and Berman (1985) do; Petitto and Bickerton do not), and in their view of movement between the stages as continuous or discontinuous (Abrahamsen et al. and Berman see it as continuous, Petitto and Bickerton as discontinuous). It is interesting to note that the models which take account of ALR do not separate the semantic (reference) from the syntactic properties of language, and therefore see a discontinuous progression from communication systems lacking these properties to bona fide language. Bickerton’s model did not take reference into account, and was based solely on innovation of structure. At any rate, it is not clear if positing a separate stage in which there is reference but no syntax would undermine an argument for discontinuity. The best way to
choose between these different models is to find out whether there is a separate stage during which children have referential use of words, but do not combine them syntactically. That is, we need to look for evidence for three, as opposed to two stages, as follows:

Stage I: -reference, -syntax (this would include ALR results thus far, Berman's first stage, Abrahamsen et al.'s first stage, and the prelinguistic stage proposed by Petitto).

Stage II: +reference, -syntax (this would include all of the members in Bickerton's protolanguage list except animals, Newport's first stage, and Abrahamsen et al.'s symbolic stage).

Stage III: +reference, +syntax (this would include Berman's grammaticization phase, Bickerton's stage II, Newport's second stage, Petitto's linguistic stage, and the third stage proposed by Abrahamsen et al.)

This is exactly the sort of model proposed by Abrahamsen et al. (1985), but it is not clear how referential use of words was established, or if it was merely the subjective opinion of teachers based on observations of the context in which children used words and signs. At any rate, it is probably not possible to establish reference solely on the basis of passive data collection (see discussion of Seth data below), rather it requires a multifaceted approach, including psycholinguistic experimentation (see Chapter VI).

If, in addition to demonstrating a middle stage for children, it could be shown that chimps can also achieve such a stage in which they have reference but no syntax, then this would mean that animals can be
brought to this intermediate stage through education. The question then remains whether the same is true of syntax, but as we shall see in the following sections, according to the present hypothesis, this should not be possible— that is, the present hypothesis predicts no middle stage.

The Evidence

In this section, we will consider the data which support a hypothesis that on the way to language acquisition children pass through a non-linguistic stage, followed by a linguistic stage, while chimps (and perhaps dolphins and sea lions) may achieve a similar non-linguistic stage, but do not progress beyond it. In addition, we will discuss the kind of data which might further support this hypothesis, and what sort of evidence might force a re-thinking of it.

In order to find support for our two-stage hypothesis, it must be possible to provide evidence of a non-linguistic stage, as we have in Chapter III, but in the case of the child, it is also important to describe what happens next. We know that children eventually learn to use the language of the environment in which they grow up, so we must ask, when do they leave the non-linguistic stage and how, and what follows it? As we shall see in the discussion which follows, there are several independent sources of evidence which could indicate a move from non-linguistic to linguistic processing of language input.

One indication that the child is leaving the non-linguistic stage is that she begins morphological analysis of utterances. In the case of Seth, at about 23 months of age, he begins breaking up the chunks of speech he has been using to communicate with his father.
During the next four months, he will systematically separate these chunks, recognizing that phrases like "talk phone", "take a bath", "geese say", "brush a teeth", and "chocolate milk" are composed of more than one element, and learning to recombine these newly separated elements in productive ways. Further, he will begin to show awareness of the morphological structure of words, learning to use the plural 's' productively, as well as verbal endings like '-ing', '-ed', '-en' and adjectival affixes like 'y'.

As children reach this linguistic stage, it seems as if a structure-, or "order-from-chaos"-imposing device overrides analyses that would be indicated by a conceptually driven device. It seems as if children’s analyses of language are now motivated by a strong desire to impose order and regularity on the linguistic input to which they are exposed. Evidence for this trend can be found in the kinds of errors children make in their production of utterances. According to the present hypothesis, the child is engaged in paradigm building, and this is the basis for errors of overgeneralization.

This seems to be an across-the-board phenomenon in child language acquisition, that somewhere between the age of two and three (subject, no doubt to wide variation; see note 6) the child does an "about-face", for example regularizing forms to get words like "foots", and "breaked" (see Bowerman, 1982, for a discussion of the phenomenon of reorganization of the lexicon and syntax). The traditional argument is that the child is using analogy to overgeneralize in the above examples, but it is important to see that it is a purely linguistic analogy based
but it is important to see that it is a purely linguistic analogy based on paradigmatic levelling, and (in the case of Seth at least—correlational data on other children would be helpful here) it appears just as the child moves to an MLU of two and beyond, at around two years of age, the point at which it is hypothesized here that the child begins to process all language input in terms of a linguistic analysis—the point when the child moves from the non-linguistic to the linguistic stage of language acquisition.

There is an abundance of other kinds of evidence as well for the existence of this second "linguistic" stage in language development. For instance, many children go through a period during which it is apparent that they are not yet clear as to how pronouns work in their language. It has been hypothesized that children make first and second person pronoun reversal errors because they fail to grasp that pronouns have changing referents, depending on who is speaking. Instead, these children evidently consider pronouns to have consistent referents, as do names (Chiat, 1981, 1982; Clark, 1978). That is, it is hypothesized that the child includes pronouns in her distributional analysis of nouns. Such a hypothesis logically follows from the fact that prior to using pronouns the child uses full proper nouns to refer to himself and others (although there are "pronominal children"—cf. Katherine Nelson’s work—who use both pronouns and proper nouns during the same period).

The case of children acquiring ASL is of particular interest because in ASL pronouns are indicated by pointing at the referent (see Chapter II). Pointing at oneself to indicate "me", and at another to
expected to greatly facilitate acquisition of the morphemes. Yet Petitto (1983, 1985) reports that some deaf children make the same kinds of pronoun reversal errors as do some hearing children, most often misusing the pronoun "you" to refer to themselves. Why should this be so? This phenomenon is easily explained by our hypothesis as follows:

As the child moves into the linguistic stage she imposes a linguistic analysis on the use of pronouns, in this case, based on her knowledge of reference. Petitto (1985) proposes that on the one hand, these pronoun reversal errors occur at a time when the child has begun to understand that signs are referential -- she recognizes the relationship between the sign and its referent. But notice that when adults use "you" in speaking to the child it always refers to the child. Based on her understanding of reference, and by analogy with other nouns, the child assumes that "you", like other nouns she knows for which the form–meaning relationship is arbitrary, has only one referent: herself. Thus, a purely linguistic analysis could lead the child to assume that the symbol–referent relationship is constant here (for pronouns) as it is for other nouns, and in overgeneralizing the application of this linguistic analysis, the child ignores the information provided by the fact that one actually points at the referent in using the sign.

It might be argued that this phenomenon could also be explained by a "formulaic hypothesis" whereby pronouns are first learned as parts of phrases encountered in directives to the child and picked up unanalyzed and hence unreversed. But Petitto argues that these errors cannot be
attributed to imitation of adult utterances. Petitto reports that in the case of one child, even repeated modelling and explicit correction failed to affect the child's behavior. If she were merely imitating, her signing behavior would be expected to change when the model did.

A final example comes, again, from the field of ASL acquisition research. As discussed in Chapter II, many ASL signs are iconic in that the mapping of symbol to referent is more direct than in other signs, so the sign for "tree" is said to look like a tree. Again, this direct relationship between sign and referent might be expected to facilitate learning, yet, as mentioned in Chapter II, there is no evidence that children exploit the iconicity factor in their acquisition of ASL signs. In order to understand why this might be so, we must consider what enables one to recognize iconicity. In order to recognize iconicity, one must concentrate on the individual item, rather that seeing it as part of a system. Before Stage II, the child may not be capable of performing the complex mapping between form and referent necessary to recognize that they are similar (as suggested by Newport and Meier, 1985), after all, cognitive development is also continuing along with linguistic development. By the time the child is capable of such a complex mapping, which goes beyond understanding of reference, I am hypothesizing that the second stage has begun, and the child analyzes forms as part of a system, rather than concentrating on the individual form-referent relationships. Thus, although the child may be capable of recognizing iconicity, studies have shown that it does not play a significant role in language acquisition, because, according to the
significant role in language acquisition, because, according to the present hypothesis, the child is driven by a mechanism that organizes units into paradigmatic systems.  

What further evidence might help support our two-stage hypothesis? Returning to the comparison of Seth and Nim, it presently seems evident that the performance is similar, but is it similarly motivated? Peters (in press) suggests that a useful approach to the development of early syntax in children is to concentrate on processes of development, as opposed to static stages. Identifying the stages through which a child passes in the acquisition of language has been useful, and now there is a need to examine the processes by which the child moves from stage to stage. The same principle may be applied here. Peters was referring to diachronic processes, or how the child moves from one stage to the next. Here we need to look at synchronic processes, or processes occurring within one stage. In Chapter III we looked at the structural similarities between the output of Seth and Nim, and the discourse strategies employed by both. What is called for in future research is an examination of the processes by which the subjects (child and animal) learn language and use it. Having identified a stage which seems to be common to chimp and child alike, we need to see if the same processes are also evident, in order to show that the underlying competence is the same. We need to study how they perceive language input. If we find that the processes by which Seth and Nim produce and comprehend language are similar, then this would be further support for the idea that they do indeed share Stage I.
How does one go about studying processes? Psycholinguistic studies of both chimps and children would be most helpful in this regard-- that is, we need to engage in "comparative psycholinguistics". This would include studies of the development of reference, tests of generalization of vocabulary items, and examinations of formulaic speech. The same kinds of tests used in ALR can be used with the child. Traditionally, psycholinguistics has been divided into two broad areas: developmental, and experimental, there is a need for more developmental-experimental work. The existing data can give us ideas about what to look for, and may help formulate questions, but not every question can currently be answered, as we have seen, and new kinds of data need to be gathered.

If we find the same synchronic processes occurring, then we can say that apes truly share this non-linguistic stage. By studying how children and chimps differ, when they separate and how and why (that is, by refocusing on the diachronic processes), we can learn more about how animals learn the systems we teach them, and how children acquire language. We need to find out if vocabulary learning occurs the same way in child and chimp. For instance, Peters (in press) notes that in children the move from one to two word utterances occurs at the point when the child has approximately 50 words in her vocabulary. Is the same sort of "critical mass" phenomenon present in apes?

Predictions Which Follow From The Hypothesis

It was hypothesized above that chimps share with children a non-linguistic stage, which for children precedes a linguistic stage of language acquisition, defined by reference/displacement and
syntax/hierarchical structure. Since this hypothesis has two parts, namely that 1) a non-linguistic stage is shared by chimp and child alike, and that 2) only children then move on to the linguistic stage, there are in principle two ways of falsifying it. On the one hand, if it could be shown that the similarities between child and chimp in the hypothesized first stage are in fact so superficial as to be trivial, this would seriously weaken the hypothesis. On the other hand, if it could be shown that animals can and do achieve the second stage, or that the second stage must itself be divided into two stages (as in the three-stage model described earlier), this would also threaten the validity of the hypothesis. We begin with a discussion of possible evidence against the first part of the hypothesis.

Often, attempts to refute a hypothesis produce more answers than attempts to support it, and there is a need to consider what might constitute negative evidence. Negative evidence can take either of two forms: 1) when predictions of the hypothesis are not borne out in subsequent studies, or 2) when subsequent studies turn up findings which the hypothesis predicts should not occur. It is to be expected that any experiment we might undertake to find evidence which could further support our hypothesis might just as well reveal negative evidence. For example, what would happen if an infant chimp were brought up alongside of a human deaf infant? It is surprising that this has never been attempted, but instead hearing teachers were assigned to care for chimps in a home-like environment in the Washoe and Nim projects. If, as it has been argued in ALR, all that a chimp needs in order to learn language is
a good environment, a proper language model (and modality), and some extra help in the form of tutoring, then raising a chimp in an ASL-signing family environment should do the trick. This should be an ideal situation in which to test the ability of a chimp to learn language. The present hypothesis predicts that the chimp will share only the non-linguistic stage with the child. If such a project were undertaken, we might find instead that chimp and child share Stage I to an insignificant degree, but this would be important information as well.

One way to show that even though the output of Seth and Nim was similar, their underlying competencies differed, would be to show that Seth already had indications of referentiality during the period when MLU was less than two. As mentioned above, it is not possible to show this given the present data, but this would be an important task of any future comparison of chimp and child, and would surely require principled tests (as mentioned above) rather than merely collecting "naturalistic" data.

It was shown in Chapter III that neither Seth nor Nim showed evidence of syntax in their utterances, and it is predicted that this observation would apply to comparisons of other chimps and children as well. There is always the chance with an N of one that that subject is extraordinary in some way. So, just as we need to look for the beginnings of referentiality in the early speech of children as discussed above, continued study of the acquisition of syntax should provide further support for the hypothesis by showing that indeed, syntactic processing of language does not occur until the child has
entered the hypothesized Stage II, sometime after an MLU of two. Any evidence to the contrary would constitute negative evidence for the hypothesis.

We turn now to the kind of evidence which could weaken the second part of the hypothesis, which states that only human children move on to the linguistic stage of language acquisition. If we go beyond the mere statement of the hypothesis, we see that it makes some very specific predictions. The hypothesis states that the linguistic stage includes acquisition of both reference and syntax. The two are considered inseparable for a reason. Both are based on an underlying cognitive prerequisite for language, which has been referred to throughout this dissertation as the "element/system" relationship. Displacement is evidence for reference, and hierarchical structure is evidence for syntax; both reference and syntax deal with the symbols of language and how they fit into a system. Reference is the system by which symbols are related to the real world. Syntax is the system by which symbols are combined in linguistic expression.

It should be pointed out that these are two very different kinds of system. Whereas reference deals with the arbitrary mapping of symbol to referent, syntax deals with the building of a formal system. Both are fundamental to language, but it is not clear exactly how they are related, or how a relationship might be revealed in further research. Thus far, we have only been able to point out a correlational relationship-- that they occur alongside each other in language acquisition and use. One way in which syntax and reference might be
related is in the information that each supplies to the other system. If the reference system is composed of words and how they are related to the real world, then certain kinds of syntactic information would be useful in organizing this kind of knowledge about words. The fact that a word is a noun or a verb (distributional information) is obviously relevant for referential use of that word. By the same token, some of the information which makes up part of the network of reference of a word may be useful for syntactic classification of the word. Again, it should be stressed that it is not yet clear how reference and syntax are related, only that they are integral elements that are fundamental to a language capability.

The hypothesis presented here predicts that animals can neither learn to use symbols referentially in displacement nor syntactically in hierarchical structures. Thus far, there is no evidence to the contrary. As researchers, when we look for and do not find "X" (in this case, "reference/displacement" and "syntax/complex structure") two possible reasons which immediately come to mind are: 1) it really is not there; 2) we just failed to detect its presence (also known as "type II error"). Failing to detect the presence of X may mean that we simply did not look hard enough. In either case, there is always the possibility that X could have been there, that is, its absence does not indicate the impossibility of its being present. This possibility is particularly relevant here because it is exactly what Savage-Rumbaugh claims the work with the chimps Sherman and Austin shows. According to Savage-Rumbaugh and her colleagues, chimps are
capable of learning to use symbols referentially, if only they are
tutored in the right ways, and tested in the proper paradigms:

In our laboratory, we believe that we have provided
considerable evidence to show that chimpanzees can learn to use
symbols in a representational manner, but that they do not
necessarily do so spontaneously once they have learned to
produce a symbol (Savage-Rumbaugh, Rumbaugh, Smith & Lawson,
1980; Savage-Rumbaugh, 1981; Savage-Rumbaugh, Pate, Lawson,
Smith & Rosenbaum, in press). We believe that representational
symbol usage has come about through the continual devising of
training tasks in which multiple alternatives exist within a
given context and thus the context alone cannot specify more
than a general message without the help of a symbol. (Savage-
Rumbaugh & Sevcik, 1984, p. 209-210)

Savage-Rumbaugh and Rumbaugh (1978) recommend against beginning
language training by working on object names and attributes, because,
according to them, this sort of training leads to just the kind of paired
associate learning which is counter productive in a language project.

In 1978 Savage-Rumbaugh, Rumbaugh, Smith and Lawson claimed to
have discovered "a paradigm which makes possible, for the first time, an
unequivocal determination of the presence or absence of representational
symbolic function." In the Savage-Rumbaugh and Sevcik (1984) report the
authors are no less convinced that this pair of chimps has demonstrated
referential use of symbols. If it could be shown that the chimp can
achieve a stage at which he uses symbols referentially, as Savage-
Rumbaugh claims, then this would instead constitute negative evidence for the two-stage hypothesis. This would be evidence for an intermediate stage, a stage in which the chimp has referential use of words, but does not have the ability to combine them syntactically. The two-stage hypothesis predicts that an intermediate stage can not exist. Savage-Rumbaugh and Sevcik (1984) argue for just such an intermediate stage (although they do not address the question of syntax):

Such symbol usage represents an advance from the prelinguistic level in which a symbol is only a part of the context, to the linguistic level, wherein a symbol adds representational information which goes beyond the information already available in that context. (Savage-Rumbaugh & Sevcik, 1984, p. 210)

I am concerned that the authors’ new-found interest in comprehension (as opposed to production) may prevent them from recognizing that the same paired associate learning which enables animals to perform in the production mode may also occur in comprehension. For example, Savage-Rumbaugh and Sevcik speak about a situation in which the chimp is requested to retrieve objects which are not immediately present. This is identical to the dolphin work discussed in Chapter IV. The fact that an animal’s behavior is consistent in either production, comprehension, or even both, still does not establish the referential use of symbols.

The contribution of the Savage-Rumbaugh team (and it is by no means a small contribution) is that they point out the importance of reference and displacement, and do not dismiss them quickly. It is difficult to contrive experimental situations which might demonstrate the elusive
quality of reference. What the Savage-Rumbaugh team correctly emphasizes is that no one experiment will be sufficient.

What I am proposing here is that animals are prevented from learning these properties of language (reference and syntax) not merely by the fact that they lack some sort of language faculty, or computational component, but by the very character of their conceptual component, or cognitive endowment. The position argued here is that the cognitive component feeds into the computational component in the following way. Part of the human conceptual component includes the capacity to move from element to system and back to element. This cognitive capability feeds into the computational component as a necessary prerequisite for the linguistic capacity to link elements together into the complex formal systems necessary for language.

One area of language in which this capacity to move from element to system and back is clearly exemplified is in human use of the phonology of natural languages. The present hypothesis predicts that no animal could learn a phonemic system, not because animals lack the ability to build formal systems but more importantly, because they lack the cognitive ability to move from element to system.

Thus far, no animal has learned a phonemic system. In some cases, this is due to the choice of "language". The system used with the dolphins is logographic in that each symbol represents a whole word. The same is true in Premack's work with token symbols. The language designed by Von Glaserfeld (a linguist) for Rumbaugh's work is "phonemic" (in the sense that each lexigram is made up of a combination
of lines, shapes and colors) yet that aspect of the system was never relevant for Lana or any of the other animals using the lexigrams-- the chimps do not need to make phonemic distinctions in order to interpret the lexigrams. It is important to stress that as yet no animal at the Rubaugh laboratory could demonstrate the capability to use a phonemic language because the animals were not given the opportunity to manipulate the symbols at that level. The chimps could only use existing "words" by pressing keys on which the symbol appeared, but they have not been able to manipulate the components to create new "words", although the component system was available to experimenters when they created new words. Chimps trained in sign, like Lana, never had to be aware of the features or "cheremes" of ASL signs in order to use them, and there is no evidence that they were aware of the component make-up of signs. Three kinds of evidence that we look for in humans to show phonemic processing are 1) errors, for instance, "slips of the tongue" (or hand), 2) recognition of phonotactic patterns (for instance, the ability to recognize "blik" as a possible word in English, and "bnik" as impossible in English although it might be possible in Arabic), 3) productive use of phonological features, for example in word-play, or in making up words. These are the kinds of evidence that need to be looked for as evidence that an animal has learned a phonemic system.

The best way to test this prediction of the hypothesis, that an animal could not learn a phonemic system, is to attempt to teach an animal to use a phonemic system. If it could learn to sequentially segment streams of sound (or gestures) into phonemes, and manipulate
them productively, this hypothesis would be proven incorrect. If an animal could do this sort of element/system recognition, it does not necessarily mean that that animal could learn language, but if it could not learn a phonemic system, there is no way that it could learn language.

The point is that this kind of cognitive capacity needed to move between element and system is a prerequisite to the learning of the purely formal systems necessary for learning language. There is no reason to expect an animal to be able to learn an abstract formal system if it cannot even learn a concrete element/system relationship. In addition, it is important to note that a basic premise of the hypothesis is that syntax and reference are acquired together (probably each system feeding into the other system), and not in separate stages.

A final important source of evidence which should not be overlooked is further comparative studies involving other species. If it can be shown that species other than the non-human primates are capable of acquiring referential or syntactic use of symbols, this would obviously constitute negative evidence, and indicate that perhaps what we are dealing with is a general mammalian capacity that is not related to higher intelligence or language learning at all. The current hypothesis predicts that only humans should progress beyond the first stage. Only extension of ALR will answer this question.
Chapter V Notes

1. This phenomenon of "association bundles" (a term coined by Bob Wilson) is one place that we may look for the roots of reference, using displacement as evidence. To the extent that all children have such bundles (we have no evidence on this yet) perhaps it is by expansion of these initial "bundles", by adding more terms and forging new connections between terms-- building up networks-- that words begin to take on the quality of reference so that they eventually become symbols which represent referents.

2. This just serves to further demonstrate the important connection between memory capacity and displacement discussed in Chapter I. The expansion of "association bundles" mentioned in Note 1 above cannot take place unless memory capacity is sufficient to allow such expansion.

3. In Bickerton (in press a) this term refers not to a particular reconstructed ancestral language, as in "a" protolanguage, or "the" protolanguage of, for example Indo-European, rather, it refers to a possible proto-human language.

4. This is not to imply that second language learning is considered here to be identical to first language acquisition-- quite the contrary, in my opinion, those who do go on to learn the target language do so only through conscientious study, just as they would approach the study of history or geography of a foreign country. (It should be noted that the issue of just what processes occur in second language learning is a controversial one, and this is just one possible view).

5. Based on the data from Genie and the deaf children in group I, it
seems this ability must be utilized in childhood for first language acquisition.

6. Obviously, this is only an approximate age, based on what was evident in the Seth data. The actual age may vary greatly from child to child. For example, in the case of "Eve" (studied by Brown, 1973) it began to operate a year earlier. As Bowerman (1982) has noted, determining the extent of individual differences is an important area of future research. We need to "identify the domains of language structure they affect, and to discover what factors facilitate or impede the child's search for linguistic regularities" (p. 345).

7. Notice that I use the word "may"—there is a need to perform experimental studies of recognition of iconicity in children, something along the lines of tests used by Klima and his colleagues with adults. It should be possible to demonstrate iconicity by illustration with a few signs, and then to test children's ability to recognize further examples of it by showing both hearing and deaf children signs and asking them to guess at their meanings.

8. If apes can really be shown to demonstrate the capability to recognize the iconicity of ASL signs, as would seem to be the case based on Patterson's claims for Koko discussed in Chapter II, this would lend further support to the two-stage model. It would demonstrate a clear cut-off point, where the ape's cognitive capabilities continue to develop, enabling it to do the kind of mapping required to recognize iconicity, while the child enters Stage II (which the ape will never achieve) and is unable to use iconicity in his/her acquisition.
CHAPTER VI
FUTURE DIRECTIONS

The preceding chapters have been critical of some of the recent work by psychologists attempting to teach language to animals. Signing apes have learned to sign in a pidgin-sign at best, dolphins have learned a system which bears no resemblance to natural language, and Nim Chimpsky learned to imitate. At the same time, we have been able to learn more about our own language as a result of the effort of considering these contributions to animal language research. What have we learned, and where does the future of ALR lead? What is the promise of animal language research?

ALR has stimulated linguistic research in many ways. In attempting to understand what Washoe was doing in signing ASL signs, researchers were driven to find out more about how humans use sign language. By isolating and describing those features of ASL that apes do not acquire, even after years of instruction, we learn more about the domain of the human language faculty.

Comparative research sharpens our research tools. The process of borrowing a method from human research, applying it to the rigors of ALR experimentation, and then reapplying the revised method to humans yields better data. We are only beginning to appreciate the importance of experimental design and control for non-linguistic factors in child language acquisition research, and this is due in part to the work going on in ALR. If chimps and dolphins undergo double-blind testing, then so should children.
Input from the intersection of these fields (ALR, child language acquisition) and others (particularly, neurolinguistics and second language learning) can only serve to further our knowledge by revealing new connections. We may not have all the answers as to Nim's acquisition of pidgin-sign, or Seth's acquisition of English, but a comparison of the two support a hypothesis of language acquisition that can now be tested in further studies. There is a need to forge ahead with this research, but not before pausing to take stock, and reconsidering our approach. This chapter will propose a three-pronged approach to the future of animal language research.

The first branch of the approach calls for a retreat in the face of language. The first generation of ALR researchers hoped to break a language barrier and "talk to the animals". Many have already begun to retreat from this position, but it has not always been in the right direction. Researchers working with signing apes, overwhelmed by the spectre of that unattainable hallmark of human language, syntax, have retreated into themselves, becoming defensive of their charges, and more concerned with communicating with their apes than with their scientific peers. They no longer seek to achieve language, but this is only because they no longer accept the differences between language and communication as being essential ones. These researchers consider the artificial language projects to be improperly designed because they do not allow communication to take place in a natural setting—Premack's chimps need their tokens, and the Rumbaugh's chimps need to have access to their computer keyboard. At the same time, the latter approaches in
which artificial languages are used in a laboratory environment allow more careful observation and better data collection and experimental control.

It is researchers in the latter group who have begun to retreat in the right direction, by focusing on limited aspects of language which may be cognitive prerequisites to language learning. These researchers have realized that there is no point training an ape to "ape" language—to perform language-like behaviors when the result is only superficial performance with no underlying language competence. Why bother to teach an animal to say "ball" when we do not know whether it understands that the symbol represents the referent? The Rumbaugh's believe it is possible to instill the concept of reference, given the right approach (Savage-Rumbaugh et al., 1980).

Herman et al. (1984) have noted an important difference between their project and ape projects, relevant to the question of reference. Because of the environment in which it is necessary to house dolphins—namely, in a water medium—there is no one-to-one correspondence between object and location. It is possible, of course, to anchor objects in place in the water, and this procedure may even be used for some training, (and some of the objects in the dolphins' vocabulary are permanent tank fixtures that never move) but generally objects can be left to drift about, bobbing on the waves caused by the dolphins' movements. Herman et al. liken this to a parallel situation for a chimp in which objects would float about in the air. It is true that chimps (and other animals) tend to encode objects spatially, and this may not
encourage them to recognize the referential quality of symbols because they tend to associate the symbol with the location of the object, as opposed to its attributes and functions, but merely having objects move around in visible space is not enough. A better way to truly test whether a symbol actually represents the object for the animal is by removing the presence of the object. There is simply no way to get at reference without displacement.

This comparison between the training environment of the dolphin versus the ape leads to another important question: how do these animals in ALR projects perceive their world? In the case of the chimpanzee, it is reasonable to assume that at least some of their perceptions will correspond to our own, and thus through instruction they will be able to arrive at similar concepts, but the dolphins pose a whole different challenge. Life in the water is very different from on land. In Chapter IV we saw how our intended meaning of "surface" and "bottom" may be interpreted by the dolphins as "upper" and "lower". Why should they be able to see the world our way? I am the first to admit that I cannot perceive the world through a dolphin's eyes. Therefore, it is important to conduct research which will tell us something about the way in which each animal perceives its world. Even a limited artificial language like those currently employed can answer many of these questions. By posing problems for chimps to solve, as have Premack and the Rumbaughs, or requiring that the animals cooperate to achieve a common goal, we may begin to tap their cognitive capacities, and at the same time we may discover approaches
that can be useful in applications to mentally handicapped humans. Both Premack's (Davis & Gardner, 1976; Gardner et al., 1976; Hughes, 1974; Velletri-Glass et al., 1973) and the Rumbaugh's (Parkel & Smith, 1979; Parkel, White & Warner, 1977; Romski, White, Mellen, & Rumbaugh) approaches have successfully been applied to such individuals. (cf. also Schiefelbusch & Hollis, 1979).

Today we have the unique opportunity to observe the development of two infant apes in the laboratory "language" environment. At the Fouts' laboratory in Washington, Washoe has adopted a baby chimp "Loulis" who is learning signs from Washoe and her companions, and is not being trained in signs by the humans at the lab. Across the country at the Yerkes Primate Center, the team of Sue Savage-Rumbaugh is currently working with a pygmy chimpanzee who apparently learned how to use the lexigram keyboard by observing his mother. These two animals may eventually achieve a greater communicative ability using their acquired systems than any other ape studied thus far. We may gain important insights by studying the changes the system undergoes in being passed from generation to generation of apes, just as we have with humans.

We must not overlook the natural communication of these animals. The second branch of the three-pronged approach calls for renewed interest in animal communication in a natural environment in the light of ALR. The recent work of Cheney and Seyfarth (Cheney, 1984; Cheney et al., 1982; Seyfarth, 1984; Seyfarth et al., 1980a; Seyfarth et al., 1980b) has demonstrated our underestimation of the communicative ability of free-ranging monkeys and perhaps the overestimation of laboratory
chimps as well. It is necessary that we know more about these animals’ natural communication in order to properly interpret their behavior in the language training situation. If we know to what extent they use communication naturally, and how complex that system is, it will help us to recognize the difference between their natural communication and communication-via-language in the laboratory. Otherwise, we may assume in a particular situation that the explanation for behavior is comprehension of language, when really the animal’s behavior has nothing to do with language.

Using human language as a measuring stick, communicative systems of animals may not measure up, but I predict that future research will reveal even more complex communication than we have seen. Humans have language to aid in their communication; if we did not have language, no doubt our communication system would have become more and more complex depending on how much and what sort of information we needed to communicate. Perhaps we can speak about a "communication quotient" (like Jerison's "encephalization quotient" discussed in Chapter IV) as a measure of the amount of communication an organism uses in excess of the amount required for mere existence, (although it is not at all clear to me how we might go about measuring this "base amount"). Language puts the communication quotient of humans in an entirely different league from that of say, the vervet monkeys mentioned in Chapter I and above.

The study of natural communication may be limited by restrictions on our ability to recognize forms of communication that differ from our own:
The same structures that make it possible to learn a human language make it impossible for us to learn a language that violates the principles of universal grammar... we're not designed to learn perfectly usable languages that violate universal grammar. These languages would simply not be within our range of abilities. (Gliedman, 1983 interview with Chomsky, p. 118)

Dolphins lead very social lives, dependent upon each other for their livelihood. They may not have anything like a human language capability, but they definitely communicate. The evidence for this is mainly anecdotal, and needs to be studied and documented carefully. Lilly (1967) reported a case in which one of his dolphins had been wounded in such a way that prevented it from surfacing properly to breathe through its blowhole. When this dolphin was released among its companions, they crowded around as if to help. When a first attempt was not successful, Lilly reports that they retreated as if to confer on a better strategy, returned, and together lifted the wounded dolphin by his flippers in rhythm with his breathing.

In my own experience with dolphins, I have seen this "conference" phenomenon many times. Once during a training session an "object monitor" inadvertently dropped his boat hook (a long pipe with a hook on the end used for retrieving the objects floating in the tank) into the water. The dolphins are worked with separately during training sessions, with one receiving commands via the control tower, while the other is played with "locally" so she will not interfere with the
training. The dolphin who was being trained at that moment was instructed to fetch the "pipe" (there is a symbol for pipe in their vocabulary, and they are quick to generalize their responses to new exemplars). She immediately swam to the boat hook, probably in part due to the fact that it was a foreign object in the tank, which had intruded rather suddenly into her world. She was unable to lift it alone, as positioning her rostrum under one end of the 12 foot pole caused it to slide off, and she was unable to balance it from the middle. Meanwhile, the other dolphin continued with "local" training. The second dolphin's trainer was instructed to release the dolphin and allow her to attempt to fetch the boat hook too. The dolphins approached the boat hook together, backed off for a "conference", and proceeded to lift the pole, by placing their rostrums on either end, and swimming with it to the hands of the waiting trainers.

On many occasions swimming with the dolphins, my partner, Nancy Chin, and I experienced this sort of behavior, which we termed "plotting and scheming" as the dolphins always seemed to come up with "sneaky tricks" to play on us after one of these "conferences", but often they tried to communicate with us, to teach us new games. Once the dolphin Phoenix taught me a chase game by demonstrating on my body what I later gathered she wanted me to do to her. She came over and poked my flippers (on my feet) quite deliberately with her rostrum. I glanced down to see if there were something wrong with them, as the dolphins were very sensitive to changes in the environment, especially on our persons, and closely examined us every time we entered the tank, often
rejecting portions of our attire (they never liked weight-belts for example). She continued to poke me and then flee with a wild look in her eye, which was the established way of communicating "chase me", but when I just chased her as usual she got angry and turned, driving me back to the corner each time and poking my flipper. I tried taking them off, but that was no use. She kept putting me in the corner and trying to tell me something. Finally, I got mad and poked her tail. She was delighted and took off across the tank with me in hot pursuit, trying to poke her tail while she flicked it just out of reach most of the time, but occasionally allowed me to poke it. Undoubtedly, our interpretation of their behavior is thoroughly pervaded by our own conceptions as humans, and may be far from the truth, which is why such playful interactions should become the subject of serious study.

Research carried out by the navy in this country has suggested that dolphins can interpret echolocation signals broadcast by us. An envelope of sound, comprised of a computer-generated simulation of the return echo of an echolocation signal, broadcast to the dolphin can be interpreted as well as their own echo. Do they send such envelopes of sound to each other, and might the whistles which often accompany these echolocation clicks be akin to our facial expressions and intonation? The theory of Universal Grammar has made it clear that some sort of biological faculty restricts the form of grammar in human language to just one of the many possible forms that a grammar of a language could conceivably take. Along with wondering who else out there can "do" our kind of language, we should be looking for manifestations of the other
myriad possibilities. As Chomsky has warned us, it will not be easy. We may already be looking at it without recognizing it.

Turning to the third branch for future research in animal language, finally, after studying natural communication in the wild, and tutored communication in the laboratory, the knowledge we gain may provide a basis for turning again to look at a human language model. It would seem that the important role that creole languages play as a focal point for research on the human language faculty has been clearly established by Bickerton and others' work (mentioned in the previous chapter). Just because of its special status as a key to understanding human language, creole can play an important role in ALR as well. We turn now to a description of a specific approach to the future of ALR (as opposed to the general approach outlined thus far), an approach which uses current knowledge of creole languages to help us describe the basic cognitive prerequisites for language, in order that we might begin to look for them in animals.

In Chapter II we saw that the Gardners originally chose ASL as the language to teach their chimp because it was gestural, human, and at one time considered less structurally complex than spoken languages. As we have seen, the chimps did not acquire ASL, partly because ASL was not, in fact, modelled to them. Since they were not exposed to true ASL grammar, it is impossible to determine whether or not they could have learned to use the complexities of the grammar. We know that they do not acquire language without being provided with a language model, but that is something that we already knew before the Gardners and other
researchers spent a lot of time and money on their projects. What is
interesting is that what the Gardners (and others) actually modelled was
a sort of pidgin language.

Pidgins are languages which arise in situations of language contact
where, for one reason or another, bilingualism does not occur as the
solution to the language-communication problem. Instead, both parties
meet mid-way in arriving at an impoverished system of communication
(pidgins are often called "marginal" languages) limited in both
structure and vocabulary, which serves their purpose of basic
communication in their limited contact with each other, mainly for trade
or labor situations. As discussed earlier, although several different
pidgins have arisen in various sociopolitical and historical contexts,
all share the same kind of simplification process, which yields a system
of communication with limited grammatical complexity: little or no
inflectional or derivational morphology, a minimum of lexical
redundancy, and in which grammatical relations depend on word order.

In fact, the same simplification of grammar that occurs in
pidginization seems to occur whenever psychologists attempt to teach
animals language-- the input to the animals looks more like a pidgin
than any other form of human language. In the marine mammal studies
discussed in Chapter IV, the input to the animal was even more
impoverished, in that it was limited to two types of commands. So far
then, the input to the animals has been too limited in all cases
(including the artificial language projects of the Premacks and the
Rumbaughs) to determine whether the output (either the productions of
apes or the comprehension responses of the marine mammals) was limited by the input or by the animal processing it. This may be illustrated as in Figure 3 below:

```
<table>
<thead>
<tr>
<th>INPUT</th>
<th>PROCESSOR</th>
<th>OUTPUT</th>
</tr>
</thead>
<tbody>
<tr>
<td>pidgin</td>
<td>Ape</td>
<td>pidgin</td>
</tr>
<tr>
<td>Dolphin</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
```

Figure 3. The animal as processor of pidgin input

Looking at Figure 3, one is reminded of the computer user's expression "garbage in, garbage out." If these pidgin-type languages are not the appropriate input, then what should be taught to the animals? Should researchers be striving (as are those currently involved in signing-ape projects) to achieve the original goal of only using true ASL, as signed by native signers, in their communication with apes? Is ASL really the most appropriate model? In fact, ASL is no more appropriate than any other language, spoken or signed. ASL is not a good target because its grammar is too complex (see Chapter II). In this respect, ASL is just as bad a model as say, English, or Russian, or any other bona-fide human language that has been around long enough to develop the grammatical complexity necessary to handle the rhetorical expressiveness that humans require as language users (Slobin, 1977).

What then might be an appropriate language model for ALR? The Gardners were correct in their realization that the language must have an appropriate form, that is, one that the species being taught is physically capable of receiving and transmitting. The Gardners were
also aware of the difficulties of creating an artificial language that would both comprise some sort of "minimal human language," and would not be dismissed immediately by linguists as not being a possible human language. Thus far, all of the artificial languages have failed in this respect, from the Herman et al. two-command system, to Premack's set of unrelated sentence types, to the Rumbaugh's lexigram system. Artificial languages have fallen short of the goal of modelling a minimal human language, but there is one possibility which has never been considered. There are in existence human languages which meet the requirement that they be neither too complex (as are ASL and English) nor too simple (as are pidgins and the artificial languages that have been taught). These are creole languages, in particular, "radical" creoles like Saramaccan (Bickerton, 1984a).

If pidgins can be said to be the result of second language learning under adverse conditions, then creoles are the result of first language learning under the adverse condition of having pidgin as the input to the system (Bickerton, 1977). Where pidgins share the common characteristic that they simplify the language to a point where it can no longer be considered a "genuine" language, creoles exhibit the structural complexity and grammatical mechanisms which render them comparable to other standard languages, yet a creole in the first stages of development may exemplify a minimally complex human language. Bickerton has shown that all creoles exhibit similar syntactic and semantic patterns (1977, 1980) and proposes that these could only be the result of processes common to all children, since they are the ones who
create creoles (1981a, 1982, 1983a). This led Bickerton to hypothesize the existence of a language bioprogram which children use to acquire language. In particular, Bickerton has suggested that it is this bioprogram which enables children to fill in the gaps that result from having impoverished language input.

Where a pidgin may lack the following features of natural language: sentential complements, relative clauses, movement rules, or a system of tense, aspect and modality (Bickerton 1982, p. 18), the creolization process replaces these features in the language of the children creating it. Thus, when we replace the processor in Figure 3 with the human child, the output also changes, as in Figure 4 below.

Where researchers in ALR attempted to design systems embodying central features of human languages and failed, creole languages provide a ready-made form to use as a target language.

Creole plays a central role in Bickerton’s model of evolution of the language faculty in that it is by studying creoles and the process of creolization that we may discover what is minimally required in a human language. If the creolization process is the child’s way of creating a language that can meet the minimal requirements of human linguistic expression, then looking at the first stages of
grammaticization of a creole should give us an idea of what a minimally human language looks like, and suggest what human language looked like when it was first grammaticized. By examining what a creole language must contain (as evidenced by what all creoles do contain) we can get an idea of what the "missing link" between no language and language as we now know it might have looked like.6

The closer we get to knowing just what is in the "must have" list, the less complex that first manifestation of language looks. For example, it seems creoles can manage (for a while at least) without non-independently referential expressions such as relative pronouns and reciprocal anaphora, yet pronouns are always part of the system, even in pidgins (Bickerton, personal communication). Further study of creoles may allow us to pare the minimal requirements down to a "bare-bones" sort of language. From the evidence so far, this would be a language without anything but nouns and verbs (including adjectival predicates), a language without non-independently referential expressions (and therefore anaphora), bound morphology, or movement of constituents.

This hypothetical language could still be a possible form that human language might have had at one time, since it was arrived at by dropping all those features without which some languages manage: creoles have no bound morphology; Saramaccan has no NP-movement (Byrne, 1985); Chinese (Huang, 1982,) and Tok Pisin (Woolford, 1978) have no WH-movement; early Haitian (Carden & Stuart, 1985) had only pronouns (no anaphors or reciprocals). As we can see, these are all
features that can be dispensed with by natural languages-- of course, probably no extant natural language actually lacks all of them.

Although we may pare the syntax down to the above elements, the following three characteristics are probably found in all natural languages and would seem to be essential properties of human language: 1) recursion, 2) correlation of thematic roles (i.e. semantic roles such as patient and agent) and hierarchically structured positions, 3) a developed means of grammaticizing the tense/aspect system necessary for language (cf. Chomsky's (1981) "INFL." ) (We may come up with more essentials as we learn more about the human language faculty.)

Recursion is necessary in order to allow for utterances which are long enough to be rhetorically expressive, yet at the same time clear, processible, and unambiguous (cf. Slobin's (1977) description of "the four basic groundrules to which a communication system must adhere if it is to function as a full-fledged human language (p. 186).") Thematic roles must be correlated with hierarchically structured positions if utterances of any length are to be automatically parsable (cf. Slobin's (1977) rule 2) "be humanly processible in ongoing time.") A human language requires a developed tense/aspect system in order to enable the speaker to refer to objects and events that are not in the here and now (displacement).

Could a chimp learn such a minimal human language? Since by "minimal" we mean just that language specified by the bioprogram in its application of the computational component, which we are assuming that chimps do not have, the answer is expected to be: No. This does not
mean that we will not make important discoveries by observing the chimp's learning process using such a model. As long as our goal is not to achieve language in a non-human species, but instead to find out if there are any aspects of language that can be learned by that species with what it has (its own particular conceptual component) plus our tutelage, we will not be disappointed in our endeavor. I am not suggesting that all those currently involved in ALR drop everything and turn to creole as the ultimate answer to their language model requirements. There is no point attempting to achieve language-like performance in yet another language system without first establishing the underpinnings. Having used what we know about creole to arrive at the "bare-bones" language described above, we may now approach this latter issue: the underpinnings.

First we must consider the cognitive prerequisites of the "must-have" list above. We can separate the three requirements into elements and the systems they are part of as follows:

<table>
<thead>
<tr>
<th>Element</th>
<th>System</th>
</tr>
</thead>
<tbody>
<tr>
<td>Theta roles</td>
<td>Theta roles correlated with structural positions</td>
</tr>
<tr>
<td>Conjunction and Modification</td>
<td>Recursion by embedding, subordination</td>
</tr>
<tr>
<td>Tense/Aspect</td>
<td>INFL</td>
</tr>
</tbody>
</table>

In order to assign theta roles, one must first recognize who did what to whom. It is possible within the current animal language projects to establish this ability. We can draw here on experimental methods used in developmental psycholinguistics. If one wants to
determine a child’s level of linguistic development in one language domain or another, there are basically two ways to do so: through questions and commands. For example, in order to determine how Hebrew-speaking children understand relative clauses, Hoban (1978) had the children act out sentences like "the girl who is wearing a blue dress is reading a book." A second group of children listened while each sentence was read to them and were then asked a question which required that they repeat all or part of the sentence, the correct answer being the relative clause. In a comparison of the two test types (acting-out game versus questions) Hoban found that there was a statistically significant difference in the results of each in that the children performed better when asked to play with dolls than to answer questions.

In applying this method to chimps for example, we would first have to find out if they could be taught to act out simple sentences with toys. This would be an excellent test of reference-- if the chimps could translate from symbols in a sentence to their referents in the real world (the toys) then this would demonstrate the referential quality of the symbols. In order to act out a sentence like "The monkey is eating monkey-chow," the animal must be able to understand that the symbols in the sentence refer to actions and objects which may take place in the real world, (even though it can not perceive them taking place here and now) and further, it must be able to move from this abstraction to actually demonstrating what the referents are by manipulating the toys according to the meaning of the symbols in the sentence.
This method, along with enabling one to examine the extent of the chimp’s ability to recognize who did what to whom, that is, to understand assignment of theta-roles, is a way of testing comprehension of a variety of syntactic forms and could open up a whole new range of possibilities, including tests of comprehension of recursive elements.

Perhaps an even better way to examine the ability to assign theta-roles would be to show the animal a variety of scenes illustrating such simple sentences as "John hit Bill," "John gave Bill ice cream," "the ice cream melted," and have the animal indicate for example who the patient, or agent was in each case. The point is that first of all we have to establish what roles are even important for each species. For example, when we see a scenario of "John hit Bill" we can identify what was done, who did it, who it was done to, with what, and so on, because each one of these roles is important to us at one time or another. Perhaps some animals only pay attention to the action. So in a scene where John eats a banana, the animal might be aware of the fact that the action was eating, or even that what got eaten was the banana (and you might be able to train it to indicate these things to you), but what we need to find out is if it can consistently identify the who's the what's and the whom's in the real world before we try to find out if this ability transfers to the comprehension of abstract forms like sentences.

Alternatively, we could show the animal scenes like those described above, and then ask it to act them out with toys. Would the animal replicate the scene exactly, filling in all the theta-roles that we recognize, or might it omit those which it is unable to assign, and
could it be taught to include all those that we humans use? And what about children? At what age are they able to perform such tasks?

The same experiment could be extended to examine displacement and reference, as in the first experiment suggested. If the chimp could go further and represent what it had seen with symbols, that is, describe a previous event (such as the scenarios described above) using symbols then it would have demonstrated reference and displacement as well.

Turning to recursion, in order to understand the concept of modification, the animal must to be able to move between the whole and features of a whole, as discussed in Chapter I. For example, the animal must be able to recognize that an apple is red, round, has a stem, etc. Premack's feature analysis study discussed in Chapter I is one way to approach this question, but, as mentioned in Chapter I, we must be able to rule out alternative explanations before we can say that Sarah was performing a feature analysis. The ability to perceive context as an attribute of an object is also important, so for example, Sarah must know that sitting on the table can be a relevant attribute of a particular apple. This is how we get relative clauses in language: "the apple which is sitting on the table," where "sitting on the table" modifies "apple." An additional precursor to recursion is some sort of ability to count, or at least recognize numerosity. For example, one must be able to perceive continuous action as separate activities in order to conjoin verbs: "he chased, caught, and ate the rabbit", or nouns as in "he ate the rabbit, the mouse and the squirrel."
A counting ability is also relevant for tense and aspect. Bickerton (1975b) has proposed several basic cognitive prerequisites for operating the creole tense/aspect system:

- a speaker needs to be able a) to know the order in which past events occurred, b) to distinguish between sensory input and the product of his imagination, c) to tell whether something happened once only, or was either repeated or protracted in some way, d) to distinguish states from actions. (p. 12-13)

The ability to count has been the focus of several recent studies in animal behavior, particularly studies of rats in the laboratory. One such study by Church and Meck (1984) demonstrated that "the number of successive events (stimuli or responses) can serve as an effective discriminative cue for behavior" (p. 445). These authors use such evidence to support the claim that rats can count, and further, that "the same internal mechanism is used for duration, number, and rate discrimination" (p. 445). Church and Meck note that the ability to count has functional significance in a rat's life, and that there are indications that this ability evolved as a part of reproduction and fitness: the optimal foraging theory says that animals tend to prey where prey density is highest (p. 462). So, although the ability to count may be necessary for language, there is no reason to assume that it is not relevant for functioning in general.

If rats can be said to "count" (and it is debatable whether, for example the ability to respond with a behavior consistently after three successive stimuli is really the same as counting-- what it demonstrates
is that they can distinguish a crowded gestalt from a relatively uncrowded one), then there is reason to believe that large-brained animals like dolphins and chimps should be able to as well. It is interesting that Church and Meck speak about duration and rate along with counting in the rat, because these are just what one must be able to distinguish in order to operate the tense/aspect system described above. Instead of trying to teach animals ASL, researchers should be looking at what their colleagues have learned by studying rats. The next step is to replicate those studies in dolphins and chimps, and make sure they actually address the ability to count.

In a recent study with dolphins (Mitchell, Yao, Sherman & O'Regan, 1985) it was found that a dolphin could learn to discriminate between objects on the basis of the amount of food reward associated with each object. Of course, being able to distinguish more food from less food is not the same as being able to count, but it is possible to attempt to train the animal to associate number names with amounts. For example, in the same way that we train BALL and later BLACK BALL by pairing the symbols with the object, we can try to train ONE BALL versus TWO BALL, versus THREE BALL, etc., with one, two and three balls respectively. This is possible within the present system used by Herman, and requires no additional training other than that just mentioned. Using the command format currently employed with the dolphins, we would then command them to perform actions to different numbers of objects, telling them, for example, THREE BALL OVER ("jump over three balls"), or TWO BALL IN BASKET ("put two balls in the basket"). If they could
perform correctly within this paradigm then they could be said to have the ability to count, at least to the extent that is necessary for language (even if they do not do it the same way that humans do).

Really, what the sets of cognitive prerequisites discussed above show is how far the conceptual component can take an organism on the way to language before the computational component is necessary to go any further. According to the model proposed here, in each case, to get from the list on the left to the one on the right, the computational component takes the element as input and fits it into a system. If an animal can be shown to have the above cognitive prerequisites, then we can say that that animal is at the point where it is conjectured that humans developed the computational component. Returning to the discussion in Chapter I, it is by bringing an animal as close to the brink as possible that we can find out how humans may have bridged the gap between not having language and that first minimal human language. Using a creole-modelled language may help us do just that.

Future work in ALR may tell us more about the human "language faculty," or "computational component"-- in particular, whether or not it developed in stages (and if so, then what they were) and whether all of the computational component is species-specific, or only part of it. The results of attempting to teach animals a creole-based language may cause us to redefine our concept of language and build new models, just as comparing Seth and Nim allowed us to view Seth's output prior to two and a half in a new perspective, comparing Washoe to children learning ASL shed new light on the workings of ASL, and just as comparing the
sea lions to the dolphins caused me to reevaluate the dolphins' achievements. In Chapter I Chomsky was cited as having said that looking at animals in order to find out about human language was like looking at human jumping in order to find out about how birds fly. We have compared the broad jump of man to the flight of birds, and found that the bird too hops before it takes off in flight.

The only way to progress toward the goal of understanding human language is to proceed at once on several planes. Experimentation and data collection provide important input, but only if we take the time to process that input and use it in constructing models and testing hypotheses. We need to start by testing different kinds of subjects in the same language model. This will allow us to compare animals with each other, as with the sea lions and dolphins, and it will allow more reasonable comparisons with humans. If we find places where the ape differs from the dolphin, yet is similar to the human in his learning of a creole-based language, these places are where we should then look for homologous developments-- where we can look for the roots of human language.

The promise of ALR lies not in the hope of talking to the animals by teaching them language and breaking a "language barrier" between man and beast; rather, ALR promises to help us learn about how language first developed in our species, and perhaps redevelops every time a child passes from the non-linguistic to the linguistic stage of language acquisition. On the animal side, the controversy associated with ALR has led to more clever experiments and tighter controls. Through continued
projects like those of Herman, Premack and the Rumbaughs, we may learn how memory and thinking work without language-- ALR opens great vistas in the infant area of animal cognition. Returning to humans, the results of ALR have already had interesting applications in aphasia research and work with the mentally retarded. ALR may provide the means for better integration of animals into the work force as well. Already monkeys are used as helpers for the physically handicapped; dogs are used for guarding, search and rescue, and as seeing eye companions to the blind; dolphins and sea lions are used for various marine activities. Their usefulness could be vastly improved by ALR training.

In Chapter I some basic issues were raised, and three questions fundamental to ALR were asked. We return now to these questions. Beginning with the question "what is the difference between communication and language?", we can now say that the difference is that any creature who can achieve Stage I of the hypothesis proposed in Chapter V (and perhaps even some that do not) can communicate, but only those that achieve Stage II are capable of language. There is no animal communication that we know of that requires a stronger device than that depicted in Stage I.

Where did language come from, and what will ALR tell us about language origins and language development? As we have seen, ALR tells us that the first stage of language development is not linguistic at all, and is shared by some animals. If it is true that the chimp reaches Stage I and stops, while the child goes on to Stage II, since the chimp is evolutionarily related to the human, this suggests
evolutionary implications of the two-stage hypothesis. The implication would be that the computational component, and the language bioprogram within it are relatively recent developments in the evolution of the human brain. The computational component would have developed to handle organizing linguistic processing of the great quantities of information gathered and generated by the conceptual component, and the bioprogram would have developed specifically for language acquisition.

Bickerton (in press a) provides a compelling argument for a model in which language evolved in two stages, first, as a protolanguage, and second, with the use of the computational component, into genuine language. Bickerton describes three possible evolutionary stages in the development of our species:

a) the conceptual component
b) the conceptual component and the use of it in communication
c) the conceptual component plus the computational component, plus the use of the conjunction of both in communication.

One can imagine two possible mutations which would have caused our ancestors to move in the first stage from a) to b) by a mutation which would have brought about volitional control of utterances. This would have been followed by a protracted period of gradual adjustment of the vocal tract (Lieberman, 1984). The second stage, the move from b) to c), would involve a much more significant mutation:

a massive increment in neural processing, capable of taking the raw material of protolanguage and imposing on it a systematic structure which enables complex utterances to be produced and
parsed automatically, leaving the mind free to process the content of messages and virtually ignore their form.

(Bickerton, in press a, p. 27)

Arguing that, in this case, it is not unreasonable to suppose that ontogeny copies phylogeny, Bickerton proposes that we can model the relationship of an ancestral child to an ancestral adult after the relationship of a modern child to a modern adult pidgin speaker:

It is proposed that the linguistic development of the human infant roughly parallels, if it does not exactly replicate, the linguistic development of our own species and its immediate ancestors, and that it does this for the very natural reason that postnatal stages of brain development involve principally those portions of the brain that were added latest and that are directly concerned with general information processing and specific language skills. Specifically, the speech of children under two is related to the asyntactic protospeech of early sapiens varieties and the speech of children over two is related to the onset of fully syntactic language in the earliest members of h. s. sapiens. (p. 31)

Whether we choose to accept Bickerton’s admitted speculation as a description of language evolution (there is presently no way of testing it) or not is not important. It certainly is a plausible account, and as such lends further support to a two-stage model of language acquisition.
Thus, a model consistent with the facts deduced from a comparison between chimp and child can also provide a plausible (if speculative) account of language evolution. In addition, this research helps us clarify the differences between communication and language. ALR has much to offer to those who want to know the "what does it do and how does it work" of language.

The present work has drawn on the fields of linguistics and psychology in discussing ALR, and, it is hoped, has demonstrated the necessity of a cooperative effort in future animal language research. I have proposed that the child passes through two stages in her acquisition of language, a non-linguistic and a linguistic stage. Further, I have claimed that there is evidence that chimps and perhaps other animals can also achieve the first stage, but that they do not go on to the second stage. It is up to future research to refute this hypothesis by showing that either animals do not in fact share stage one, or that they may also go on to achieve stage two, if only provided with the proper education.

The first generation of researchers in ALR has laid the groundwork. Now there is a second generation of students of ALR who are more critical, and have broader backgrounds because of the work of their predecessors. It is up to this second generation to follow a three-pronged approach like that outlined above in order to ensure the future of animal language research. It does indeed hold great promise.
Chapter VI Notes

1. Ronald Schusterman (personal communication) has just concluded an interesting experiment with his sea lions which would seem to indicate that the same sort of absolute/relative discrepancy is true for them as for the dolphins. In his experiment, the animal was first trained to carry out commands like "jump over the small/large ball" with a particular ball exemplifying the "small ball", and a larger ball as the standard "large ball". Once the animal was responding consistently to these commands, Schusterman inserted the test trial. On this trial, the animal was told to perform an action to the large ball, except the only balls available were the standard "large ball" and a new "giant ball" (with which the animal had no previous experience). The animal's response was to choose the larger of the two balls, i.e. the "giant ball". A subsequent test showed that his sea lions seem to be able to go back and forth between the absolute and relative values of size. When the animal was told to go to the "small ball" (with only the standard "large ball" and the new "giant ball" present), it hesistated and seemed to be searching for the standard "small ball", but then it chose the smaller of the two present (i.e., the standard "large ball"). The difference in the animal's rapidity in responding may be due to the fact that in one case the object with which the absolute value was associated was present (first case described) and in the second case that object (here the "small ball") was not present, making the comparison and final decision more difficult.
2. An additional reason for ASL being a poor choice for the experimental teaching of language to animals is its ephemeral character. Like the spoken word, ASL signs appear only briefly (Hockett’s feature of "rapid fading") making processing more difficult than if the signs were to remain in place, like the written word. Processing of complex information is considerably facilitated by presenting it in a more permanent format, thus allowing repeated passes through the material without having to rehearse the original input constantly. So a sentence like "The boy whose sister we met yesterday will tell you tomorrow behind the schoolhouse what time the film their father made will be showing next week and where" is much easier to comprehend when it is written than when it is spoken. For this reason, language formats like that of Premack, in which plastic chips are stuck to a board, or that of the Rumbaughss, in which computer keys light up when pressed, are to be preferred, because they make it easier for the animal. To briefly summarize the criteria for an appropriate language: it must be two-way, easily processable, in a proper modality for the animal, and have a grammar that is just complex enough to allow for linguistic expression.

3. Premack taught a set of unrelated sentences like:

- Mary give Sarah apple
- Clay on plate
- Blue color of grape
- Apple is red same red color of apple

Premack is the first to admit the inadequacy of this language: "It is essential to see that these rules are unrelated. That is, they are not framed in terms of common categories and indeed have virtually nothing in common beyond the mechanics of using word order as a mapping device."
This melange of independent rules could have been avoided, however."
(Premack, 1985a, p. 286)

4. Like Premack, the Rumbaugh's no longer consider their lexigram training to be language training. Their focus has shifted (appropriately) from the days of trying to teach Lana language to working on prerequisites to language such as symbolic representation, referential quality of symbols, and the ability for cooperative use of the symbols with the chimps Sherman and Austin, and more recently, with the pygmy chimp, Kanzi.

5. The term "minimally complex" here refers to a language which contains just what is specified by the bioprogram. If the complexity of a language is in the interaction of its morphology with the universal properties specified by universal grammar (Borer, 1983), we can use creole as a model of a language which has human language syntax with a minimum of idiosyncratic lexical properties, and thus is less complex than a language with a more fully developed lexicon and morphological system.

6. It must be clarified here, as it was argued in Chapter I, that language did not develop by complexification of the simple call system, so this missing link is not meant to be a connecting factor between call systems and human language. Rather, it is the hypothetical first form that human language could have taken when the computational component developed and began organization of input from the conceptual component, before years of linguistic evolution resulted in the thousands of varied forms of human languages we have today.
APPENDIX A
TRANSCRIPTION CONVENTIONS

(These conventions are adapted from the general conventions established by Dr. Ann Peters to be used in transcribing tapes for the Seth project)

Each transcript begins with the following information:
1) Tape number and letter (for each week of recording, tapes recorded that week were labelled with letters: "Tape 16a" for the first half hour tape recorded that week)
2) Seth’s age and date recording took place.
3) Names of participants.
4) Location of recording.
5) Transcriber(s).

The transcripts contain five different entry lines, as follows:
1) Adult speech (generally Daddy). These lines begin with "D:", and may include comments enclosed in parentheses.
2) Code for the adult speech (see below). The lines begin with "d:".
3) Seth’s speech plus comments in parentheses following Seth’s speech. These lines begin with "S:"
4) Gloss of Seth’s speech. These lines begin with "g:"
5) Interaction code of Seth’s speech (see below). These lines begin with "i:"

Intonation:
Every speech-line is terminated with an intonation marker as follows:
1) "." for falling intonation
2) "?" for rising intonation
3) "-,-" for level intonation
4) "!" for "exclaim" intonation
Note: these are not punctuation markers as in normal English orthography, so for example, rhetorical questions often have no rising intonation, as in "What are you doing."

Stress:
Seth’s speech (and occasionally Daddy’s speech) are marked for stress as follows:
1) "#" indicates primary stress on a word (or syllable).
2) "-" indicates secondary stress.
3) Emphasis for D is marked by "#", but if his speech is particularly strong, it is marked by using all capital letters.

Pauses in speech are indicated as follows:
1) "--" indicates a break in rhythm
2) ".." indicates a pause of one to two seconds
3) "..." indicates a pause of two to three seconds
4) longer pauses are timed on a stopwatch and indicated in parentheses.
Simultaneous Speech:
When more than one person speaks at a time, this is indicated by marking the beginning of the overlap with "*" on the speech line of both speakers, e.g.:

D: Let's *go get the paper.
S: *no-no?
(Note that in this case S interrupted D, since D was already talking when S began; D continues talking despite the interruption).

When one speaker cuts off the other speaker this is indicated by use of "--*", e.g.
S: no-n--*
D: *Don't say "no", we have to go.

Untranscribable Speech:
When it is impossible to hear speech well enough to transcribe it accurately, the presence of these utterances is marked by "xx" for each syllable that is uttered.

Adult Speech
Adult speech is transcribed orthographically, but includes information about contractions, hesitations, etc, and about stress and pronunciation where deemed important.

Coding of Adult Speech
Some of Daddy's utterances were flagged by following them with special coding lines (d:). There are three types of d:-lines:

1) say-- this code is used when D tries to elicit an imitation from S by requesting that S say something, as in "Say X".
2) dot-- this code is used when D tries to get S to "fill in the blank", as in "and their names were Flopsy, Mopsy, and...". Even when S does not fill in the blank these are still coded as "dot".
3) say dot-- this code is for when D actually tells S "say" before the "three dot" intonation, as in
D: Say...
d: say dot
S: thank you Daddy

Seth's Speech
Seth's speech is transcribed in broad phonetic notation following these conventions:

Phonetic conventions for S:-lines:
Vowels: iy uw "u" as in "put"
i r u "O" as in "saw"
ey o "@" as in "hat"
e + O "a" as in "hot"
@ a "4" as in "gut"
Glides: y w

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Nasalized vowels are indicated by a following "N": aN, +N. Lengthened vowels are indicated by doubling: aa, ++, iiy.

Special consonant conventions:

- j -- as in "jump"
- ng -- as in "hang"
- ngg -- as in "bring-geese"
- ngk -- as in "bring-cup"
- ch -- as in "chair"
- ' -- glottal stop
- x -- velar fricative
- sh -- as in "shoe"
- zh -- as in "closure"
- dh -- as in "those"
- th -- as in "think"
- D -- alveolar flap

NOTE: a few symbols have multiple meanings, depending on whether they occur on speech lines or gloss lines:

<table>
<thead>
<tr>
<th>symbol</th>
<th>speech lines</th>
<th>gloss line</th>
</tr>
</thead>
<tbody>
<tr>
<td>+</td>
<td>shwa sound</td>
<td>1) gloss for &quot;+&quot; when shwa isn’t clearly glossable</td>
</tr>
<tr>
<td></td>
<td></td>
<td>2) to link morphemes</td>
</tr>
<tr>
<td>*</td>
<td>overlapping speech</td>
<td>past tense</td>
</tr>
<tr>
<td>=</td>
<td>secondary stress</td>
<td>formulaic speech</td>
</tr>
<tr>
<td>?</td>
<td>rising intonation</td>
<td>questionable gloss</td>
</tr>
</tbody>
</table>

Gloss of Seth’s Speech

Each of Seth’s speech-lines is followed by a gloss line which includes a gloss for every "word" Seth utters, including untranscribable and unglossable words, followed by an intonation code for falling (F) rising (R) or level (L) intonation; in general, speech will be normalized on the gloss-line according to American English spelling. The following is an example of a speech-line followed by its corresponding gloss-line:

S: xx waN go @Ngoruw.
g: ? want go ? F

Sometimes Seth restarts his utterance mid-way, or repeats a word or words in the middle of a sentence. These phenomena are indicated as follows:

1) Restarts-- are marked with " / ", e.g.
   S: ay wan-- ay wan wØDr.
g: I want / I want water F

2) Disfluencies-- are marked with "/ ", e.g.
   S: ay wan s+m-- s+m mOr wØDr.
g: I want some/ more water F

Some S:-lines are not glossable into English lexical items. These include fussing, crying, babbling, imitating animal noises. These are indicated with a standard set of all-capital words as follows:
At the earliest ages Seth had a set of idiosyncratic lexical items. They have been assigned English glosses reflecting their meanings:

S: ntuu; babaa; chih
ihiy; t+k+t+k+... nuuw
S: ihiy; t+k+t+k+... nuuw

He also learned or invented several phrases which may vary somewhat in phonological form, but which are recognizable as instances of the following standarized targets:

diddlow; floppy; huu; Lordy-lordy; no-no-no; noninoni

An early verbal routine that he learned includes the phrase "I love you", which is pronounced "mushily" at first, e.g. /no uwN/ or /n+ nuw/. This is coded as the formulaic form "love-you" until the parts are clearly separated and analyzed as separate words.

Morpheme Analysis of Seth's Utterances

When words in Seth's speech contain more than one morpheme a morpheme analysis is indicated as follows:

1) Plural: regular noun-plurals are coded with a "+$", if the noun should be pluralized and is not, this is indicated by using "$", e.g.

S: thriy dOg.
S: thriy dOg.

Irregular plurals are coded in their plural form followed by "+$", e.g. "feet+$"; "geese+$", but note that incorrect irregular plurals are coded with "$+$" as well, e.g. "foot+$".

2) Possessive: possessive forms of nouns are coded +/ "s", e.g.

S: d@diyz buk.
S: d@diyz buk.

g: Daddy+s book

3) Diminutives: when transparently derived from common nouns are coded as "+y", e.g.

S: dOgiy.
S: dOgiy.

g: dog+Y

4) Derived Adjectives: are coded with "Y", e.g.

S: skr@chiy.
S: skr@chiy.

g: scratch+Y

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5) Comparative and Superlative: comparative and superlative forms of adjectives are coded with +/-"er" and +/-"est", e.g. "happy+er"; "happy+est"; "worst+est".

6) Third Person Singular: these forms of present tense verbs are coded with +/- "3", with the infinitive form used as the base form, e.g.
   
   S: givz; h@z; d+z;
   g: give+3; have+3; do+3;

7) Progressive: these forms are coded with +/- "ing", using the infinitive form as the base form, e.g.
   
   S: hiyring; r+ning; giving;
   g: hear+ing; run+ing; give+ing;

8) Past Tense: past tense forms of regular verbs is coded with +/- "ed", and in addition, each past tense form is flagged with an asterix (*), e.g "*kick+ed". If the particular verb is an irregular strong verb, the asterix will be used to indicate past tense, but the verb will be glossed with its normal spelling, e.g.
   
   S: brok
   g: *broke

   Thus if errors in past tense formation are made, they are marked as well, e.g.
   
   S: breykd; brokd;
   g: *break+ed *broke+ed

9) Contractions: contracted forms are coded so as to reveal their non-contracted components by using an apostrophe to separate the contracted parts, e.g.
   
   S: dont; wont; aym; its;
   g: do'not; will'not; I'am; it'is;

   The apostrophe is also used for non-conventionalized contractions, e.g.
   
   S: didj-; donchuw; w+ts@t; gOnn+;
   g: *did'you; do'not'you; what'is'that; go+ing'to

10) Formulas: a number of expressions are indicated as being formulaic in nature in the morphological coding. These are expressions that, while morphologically complex for the adult, seem to constitute only a single lexical item for Seth. The pieces of such chunks are linked with "=" , e.g.
   
   chocolate= milk; thank= you; some= more; nother= one;

11) Prefixes: when Seth was in the 17-30 month age range he often preceded words with one of a set of stereotypical phonological "prefixes". These prefixes have standard glosses as follows:
   
   nasal set: N; NG; NGG; NGL;
   non-nasals: + D+ DL

   Prefixes are sometimes combined in a single utterance. When a vowel (usually "+") follows another prefix, this is coded as a
separate prefix, whereas vowels which precede a prefix are included as part of that prefix, e.g. "+N", but "N +"; "+NG", but "NG +".

Interaction Codes
The following interaction codes were used (as described in the text) to code Seth's speech in a way that would render a comparison with Nim possible:

1) Initiated Utterances: these are utterances which spontaneously initiate a conversation. They are neither immediately preceded by an adult, nor are they repetitions of the previous child utterance. These non-adjacent utterances are coded "INIT" on the i-line.

All non-initiated, or adjacent utterances, were broken down into three types as follows:

2) Imitation: those utterances which were comprised solely of words which appeared in the model. If these utterances differed at all from the model, it was by reduction, and never addition (see "Expansion"). These are coded as "IM" on the i-line.

3) Expansion: when material was added in imitation of the model, this was considered an expansion. Expansions are coded as "IM+" on the i-line.

4) Spontaneous: spontaneous utterances are those which are originated by the child immediately following an adult utterance. They contain no imitation of the adult model. These utterances are coded as "SPONT" on the i-line.

Many of Seth's utterances did not fall into any of the above categories, and were not relevant for the analysis for comparison with Nim. These included times when Seth was mumbling to himself; babbling; non-verbal sounds; unglossable utterances; requested imitations (by Daddy). For completeness, these utterances remain in the transcripts (in Appendix B) but are coded as "DISCARD" on the i-line, and were not included in the analyses discussed in Chapter III.
APPENDIX B

SETH TRANSCRIPTS

Tape 16a, Seth is 19.1, 5/28/82. Participants are Seth, Daddy. Location: Spencer St. Transcriber: Ermile

D: We're gonna hang your swing up. We're gonna get your swing.
S: d@li.
g: Daddy F
i: SPONT
S: 'i shin-- #haay. (i seems barred; 10 sec)
g: + swing high F
i: INIT
S: 'n-- shing-- #haay? (high pitch)
g: N swing high R
i: SPONT
S: 'n shing?
g: N swing R
i: SPONT
S: 'n #shing'shing'shing?
g: N swing-swing-swing R
i: SPONT
S: 'n #chiN'chiN'chiN'chiN?
g: N swing-swing-swing-swing R
i: SPONT
D: Ya wanta swing high?
S: chep.
g: tape F
i: SPONT
D: There's Daddy's "chip".
S: 'n shiNh-- #hay?
g: N swing high R
i: IM
S: 'n *#shiNh-
g: N swing L
i: IM
D: *Okay. I'm ready. Let's go. Let's go in the swing. How 'bout a kiss first. Mmuh. (kisses S: part of the swing ritual)
Okay. In ya go. (puts S in swing) You look so tan? Didjer Mommy take you to the beach?
S: 'n shiN-- #haay? (rustles)
g: N swing high R
i: SPONT
S: 'n shing-- #haay?
g: N swing high R
i: SPONT
D: Let's get up here. Up way... up here. Now we go. Ready? (part of the swing ritual)
S: reyDi?
D: Voooom! (Daddy lets him fly for the first swing of the day. The swing goes high, to the tops of the door frames; squeaky swing noises) Look at us swing, how high we go! (S reaches up and touches rope.)

S: wmmmmmm.

g: mmm F
i: DISCARD
S: wmmm?

g: mmm R
i: DISCARD
S: wop?

g: rope R
i: INIT
D: There's the rope. (S touches rope on other side)
S: hrop.

g: rope F
i: SPONT
D: There's the #other rope.
S: hrop.

g: rope F
i: SPONT
S: 'nnn -- shwing-- #haay. (10 sec)

g: N + swing high F
i: SPONT
S: OOnnu Onnu Onnu. (slow, rhythmic mumble, in rhythm with swing)

g: MUMBLE
i: DISCARD
S: ON ylOylO' +p- (slow, rhythmic)

g: MUMBLE
i: DISCARD
S: shing-- #hay. (clack)

g: swing high F
i: INIT
S: shIN #hay.

g: swing high F
i: SPONT
S: +n+n+nnn. (slow rhythmic)

g: MUMBLE
i: DISCARD
S: Oyl'yloylo-- (in monotone)

g: MUMBLE
i: DISCARD
S: g+ngng! (draws breath to apply to word)

g: bump F
i: INIT
D: You wanna bump? (g+ngng was a request for D to bump the swing. D does so) Okay. (clatter, 3 times)
S: h+n d++. (monotone, swing still squeaking)
D: You gonna bump your foot?
S: uh?
D: You gonna bump your foot?
S: ngngh? (to self)
D: You gonna bump your foot?
S: +nn n+m #dah'. (low voice)
D: You gonna bump your foot?
S: 'nn #dah'?
D: You gonna bump your foot?
S: Diy chif. (touching teeth, I think; stress all first syll; 6 sec)
D: T eef. (imitating S)
S: '+n chif.
D: You gonna bump your foot?
S: nai#d+, n'n'n', nai #d+. (chanting in rhythm)
D: You gonna bump your foot?
S: N shing-- #haay. (one contour)
D: You gonna bump your foot?
S: shing-- #hay. (15 sec; OFF/ON)
D: You gonna bump your foot?
S: n+n #d+n- (low rhythmic murmur; OFF/ON)
D: You gonna bump your foot?
S: shing-- #hay.
D: You gonna bump your foot?
S: l@l@l@yt.
g: light F  
i: INIT  
S: #loe=luw. (retroflexed e; 10 sec; after pause, D decides to swing him again)  
g: love=you F  
i: INIT  
D: Y’ready ta swing?  
S: n tih? (having Teddy in his lap is part of the swing ritual, too)  
g: N Teddy R  
i: SPONT  
S: n #tedl?  
g: N Teddy R  
i: SPONT  
D: Here’s your Teddl. ’ll put it right in yer lap, ’n’I’ll pull you up, an’ I’ll swing you #high. (S likes to swing with his teddy; S mischievously throws teddy on the floor) Throw that DAMN teddy on the floor, an’ I have ta go back ’n get it? (S is chanting to the rhythm of the swing again)  
S: shing-- #hay. (20 sec)  
g: swing high F  
i: IM  
S: #hay.  
g: high F  
i: IM  
D: That’s right.  
S: tedl.  
g: Teddy F  
i: IM  
S: tedl.  
g: Teddy F  
i: IM  
D: There’s your teddy, on the floor.  
S: ’m fur!  
g: N floor F  
i: IM  
D: Didju say floor? (new word)  
S: fuur.  
g: floor F  
i: IM  
D: Good fer you. You’re so smart!  
S: fuu+.  
g: floor F
Here's your teddy. (D retrieves Teddy for S) Here it is in yer lap. (S swings back, D grabs Teddy from him) I took him away from ya! You want 'im back? (S whimpers) Say "please."

D: 'Kay. Here ya go. (car noises; S throws the Teddy on the floor again. The swing's path is in such a narrow space that it's hard to get by him to pick up the Teddy) THROW it on the floor back there, Daddy can't go back and get 'im. (car passes outside)

D: *There went the car.
S: kaay+*! (6 sec)

D: Give me five! (D grabs S's hand) Hold out your hand and give me (grabs it again) five!
S: faay!

D: Floor.
S: n #tedl
D: N Teddy F
I: SPONT
S: i tedl.
G: + Teddy F
I: SPONT
S: 'ng k+ming.
G: N come+ing F
I: INIT
S: k+ming.
G: come+ing F
I: SPONT

D: Here's your teddy. (D retrieves Teddy for S) Here it is in yer lap. (S swings back, D grabs Teddy from him) I took him away from ya! You want 'im back? (S whimpers) Say "please."

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S: kaay+*! (6 sec)

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D: Floor.
S: n #tedl
D: N Teddy F
I: SPONT
S: i tedl.
G: + Teddy F
I: SPONT
S: 'ng k+ming.
G: N come+ing F
I: INIT
S: k+ming.
G: come+ing F
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D: *There went the car.
S: kaay+*! (6 sec)

D: Give me five! (D grabs S's hand) Hold out your hand and give me (grabs it again) five!
S: faay!

D: Floor.
S: n #tedl
D: N Teddy F
I: SPONT
S: i tedl.
G: + Teddy F
I: SPONT
S: 'ng k+ming.
G: N come+ing F
I: INIT
S: k+ming.
G: come+ing F
I: SPONT

D: Here's your teddy. (D retrieves Teddy for S) Here it is in yer lap. (S swings back, D grabs Teddy from him) I took him away from ya! You want 'im back? (S whimpers) Say "please."

D: 'Kay. Here ya go. (car noises; S throws the Teddy on the floor again. The swing's path is in such a narrow space that it's hard to get by him to pick up the Teddy) THROW it on the floor back there, Daddy can't go back and get 'im. (car passes outside)

D: *There went the car.
S: kaay+*! (6 sec)

D: Give me five! (D grabs S's hand) Hold out your hand and give me (grabs it again) five!
S: faay!
D: Five! (grabbing S's hand each time the swing comes back; 8 sec)
S: 'm #babay.
g: byebye F
i: SPONT
S: #dak-trrr.
g: doctor F
i: INIT
D: BYE-bye. Doctor. What else can you say. (3 sec) Say "breakfast".
d: say
S: bwekt+s? (can't hear S clearly because of swing noise)
g: breakfast R
i: DISCARD
D: Say it again, "breakfast"?
d: say
S: 'ruh-
g: ? L
i: DISCARD
D: Okay, (17 sec)
S: 'a'a 'o'o 'o'o 'a' a* (a song he learned at school)
g: SINGING
i: DISCARD
D: *a'a 'a ho'i hoo'oo (15 sec; traffic noise as well as swing noise)
S: 'a i 'i 'i'i'i'i'i'i'i'i'i'i'i'i'i'i'i'.
g: SINGING
i: DISCARD
S: 'a'o'i xxxxx 'a'a'.
g: SINGING
i: DISCARD
D: 'o'o 'a'a 'o'o 'a'a 'o'o 'a'a 'ooo 'o'a' a-
S: 'a'a' 'o'o **a'a-
g: SINGING
i: DISCARD
D: 'o'o 'a'a 'o na 'ali'i... (D and S sing 'o' o 'a'a) Let's turn this chip recorder (tape off/on-- swing still going when it comes on again)
S: shiing.. #hay.
g: swing high F
i: SPONT
S: mm (12 sec)
g: MUMBLE
i: DISCARD
D: Where's the Teddy.
S: tedl!
g: Teddy F
i: IM
Tape 17a, Seth is 19.2, 6/4-5/82. Participants are Seth and Daddy.
Location: Tyler, TX, Ben and Julie's house in the country. Transcriber: Eileen

S: n #chriy-
g: N tree L
i: SPONT
S: n #chik-
g: N tree
i: SPONT
D: It's June the 4th? an' we're-- we're at Ben an' Julie's house, an' we're gonna walk down-- to the barn and see the geese. (7 sec)
S: ng #giys?
g: N geese+$ R
i: IM
D: Whatta the geese say. Whatta they say. Honk, honk. (4 sec)
S: ng #giys?
g: N geese+$ R
i: IM
S: ng *#giys-
g: N geese+$ L
i: IM
D: *Oh, what is this hangin' down from the tree.
S: no? (knows D is going to make him touch something unfamiliar)
g: no R
i: SPONT
D: What is it.
S: no: #no?
g: no-no-no R
i: SPONT
D: Tell me what it is.
S: no no no no no? (whiney)
g: no-no-no-no-no no R
i: SPONT
D: Le's walk on. (6 sec; walking sounds)
S: chip. (low)
g: tape F
i: SPONT
S: chip.
g: tape F
i: SPONT
D: Mhm? Didju see a rope hangin'down from the tree?
S: no?
g: no R
i: SPONT
S: luwp?
g: rope R
i: IM
S: n #chik- (low)
D: Yeah.
S: ng #giys? (high, urgent)
g: Ng geese+$ R
i: SPONT
S: ng #giys?
g: n geese+$
i: SPONT
D: Here we go. We're comin' to the barn.
S: ng giys- (high)
g: Ng geese+$ L
i: SPONT
S: ng #giys?
g: N geese+$ R
i: SPONT
S: ng #giys? (4 sec)
g: N geese+$ R
i: SPONT
D: We're comin' ta the barn. Gonna open the barn door-- (sound of door opening; lots of honking; 14 sec) We're inside the barn.
Listen to the geese. (15 sec)
S: #'a #'a #’o #’o #: #’o. (Leon and Malia's song about the 'o'o'a'a bird)
g: SINGING
i: DISCARD
D: Listen to the geese. Listen to the geese. (loud honking; 10 sec) Oh, look at the old gander. He's got his head down. Now
that we're leavin' he's gonna act mad.
S: (whines) (8 sec) (doesn't want to leave)
g: WHINES
i: DISCARD
D: Let's go back outside ..an' talk. Ok? Le's lock the barn door, so the coyotes don't get the geese.
S: ng #giys- (high)
g: N geese+$ L
i: IM
D: They have coyotes down in the  bottom, an' they come get the chickens an' geese. (BW: bottom = thickly wooded creek bed)
S: +w #giys? (high)
g: + geese+$ R
i: IM
S: ng #giys-
g: N geese+$ L
i: IM
D: Didja see the geese- What did the geese say. What did they say. Tell me. Honk-honk. Honk-honk.
S: +w #giys-
g: + geese+$ L
i: IM
S: ng #giys? (high, rapid, urgent)
g: N geese+$ R
i: IM
S: ng #giys?
g: N geese+$ R
i: IM
S: ng #giys?
g: N geese+$ R
i: IM
S: ng #giys?
g: N geese+$ R
i: IM
S: ng #giys?
g: N geese+$ R
i: IM
S: ng g- (chokes)
g: N geese L
i: IM
S: ng #giys-
g: N geese+$ L
i: IM
S: ng #giy?
g: N geese+$ R
i: IM
S: ng #giy?
g: N geese+$ R
i: IM
S: ng #giys?
g: N geese+$ R
i: IM
S: nn + giy. (low)
g: N + geese+$ F
i: IM
S: ng giys. (low & soft)
g: N geese+$ F
i: IM
S: ng #giys- (high, rapid & urgent again)
g: N geese+$ L
i: IM
S: ng #giys-
g: N geese+$ L
i: IM
S: ng #giys-
g: N geese+$ L
i: IM
S: xx xx xx. (low)
g: ? ? ? F
i: DISCARD
S: muw-
g: ? L
i: DISCARD
S: *no? (to D)
g: no R
i: INIT
D: *Look at all these #big, pretty trees. They’re so pretty-
   (footsteps)
S: m #bak?
g: N bark R
i: SPONT
S: m #bak-
g: N bark L
i: SPONT
D: I think this is a live oak tree.
S: no?
g: no R
i: SPONT
S: oh-
g: no L
i: SPONT
S: no nnn- (protesting: knows he’s going to have to touch
    something)
g: no-no L
i: SPONT
Tape 18a, Seth is 19.3, 6/13/82. Participants are Seth and Daddy. Location: Tyler, Texas. Transcriber: Ann

S: +h
g: uh
i: DISCARD
D: (whisper) OK. It's July 13th in the morning an' we're gonna--
tape. We jus' got up.
S: =+ng + #teyp?
g: NG + tape R
i: SPONT
D: Mm?
S: =+ng #teyp?
g: NG tape R
i: SPONT
S: +ng #teyp? (urgently)
g: NG tape R
i: SPONT
S: +ng +n #teyp?
g: NG + tape R
i: SPONT
S: nging#gng lang+#lang + #teyp?
g: NG ? + tape R
i: SPONT
S: +ng #teyp? (urgently)
g: NG tape R
i: SPONT
S: xxx?
g: ? R
i: DISCARD
S: ng #da?
g: NG down? R
i: INIT
S: =nglelon #daN?
g: NGL down? R
i: SPONT
D: Yeah. I'm gonna take you downstairs, then you can get down.
Kay? Come on.
S: 'ngk sh+sh.
g: NGG (down)stair+$ F
i: IM
S: ngg #+ =s+s?
g: NGG + (down)stair+$ R
i: IM
S: ngg #+ =sis?
g: NGG + (down)stair+$ R
i: IM
D: Downstairs. Let's go. (carries S down; footsteps; several
moments pass before S speaks up)
S: #b+h =b+NP.
g: bump bump F
i: INIT
D: Oh let's go put a diaper. Let's get a fresh diaper.
S: #k+i+.  
g: ? F  
i: DISCARD  
S: xxx?  
g: ? R  
i: DISCARD  
S: +N+N+N #ted1.  
g: N Teddy F  
i: INIT  
D: Here we are. (diaper changing place - utility area between kitchen and den)  
S: babaa. (typing in background)  
g: eat-meal F  
i: SPONT  
S: ngg #layts.  
g: NGG light/write F  
i: SPONT  
S: layt.  
g: light/write F  
i: SPONT  
S: +ye'ye'ye'xxx > squeal  
g: SQUEAL  
i: DISCARD  
D: Ooh your diaper is WET. (clunks. finishes changing S)  
S: (several squeals; long pause)  
g: SQUEAL  
i: DISCARD  
S: aph?  
g: up R  
i: INIT  
S: gingg #aph?  
g: NGG up R  
i: SPONT  
S: sol #aph?  
g: ? up R  
i: SPONT  
S: chiy?  
g: pick-me-up R  
i: INIT  
S: chiy  
g: pick-me-up R  
i: SPONT  
S: chih? (tape stops and starts)  
g: pick-me-up R  
i: SPONT  
S: 'nngg+i +n #cheyp.  
g: NGL + tape F  
i: INIT  
S: cheyp.  
g: tape F

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D: Mhm. (10 sec.)
S: 'ng #laysh.
g: NG light F
i: INIT
S: +ngg #layts.
g: NGG light F
i: SPONT
S: nggl #sh@h?
g: NGL (up)stair+$? R
i: INIT
S: +N?
g: uh R
i: DISCARD
D: No, we're not goin' upstairs. What do you wanna do today. You wanna go see Ben? An' Julie? An' David an' Tonya?
S: wa #dld+ #bibxx #bi=biy.
g: want DL baby baby F
i: SPONT
S: +N.
g: uh F
i: IV?
D: You wanna see E- Baby Erica? Wanna see the baby?
S: #+N=k++. 
g: Erica F
i: IM
S: #ha=kaa.
g: Erica F
i: IM
S: #ha=k+.
g: Erica F
i: IM
S: #ha=k+.
g: Erica F
i: IM
S: #ha=k+.
g: Erica F
i: IM
S: howh.
g: ? F
i: DISCARD
S: #ha=ka. (moaning sound)
g: Erica F
i: INIT
D: You wanna see Tonya? (moaning sound) Tha's the potty- c'you hear that potty? That OL' potty. D'you wanna go see the geese today?
S: =nnl =+gng #giys?
g: NL NGG geese+$ R
i: IM
S: *xxx
  g: ?
  i: DISCARD
D: *Whatta they say- Whatta they geese say.
S: **(laugh)
  g: LAUGH
  i: DISCARD
D: **(laugh) Is that what they say.
S: +Nh-
  g: uh L
  i: DISCARD
S: ng #waysh.
  g: NG light F
  i: INIT
S: waysh.
  g: light F
  i: SPONT
D: Right here. (holds S up to light switch) What's this? What is that? (typing noise)
S: 'ng =diy+#p+Np?
  g: NG ? R
  i: DISCARD
D: That's the switch. (click; rustling) C'n you turn the switch? We're moved now-- there. Now-- Push down. Push down.
S: cheyp.
  g: tape F
  i: SPONT
S: cheyp.
  g: tape F
  i: SPONT
D: Mhm.
S: ='+ng w+ #chip.
  g: NG + tape F
  i: SPONT
S: +Nh- (click; typing)
  g: uh L
  i: DISCARD
S: '*ngl @N.
  g: NGL on? F
  i: INIT
S: ty+N.
  g: turn F
  i: INIT
S: m+n #y@t?
  g: NG light R
  i: INIT
S: +ng #lat?
  g: NG light R
  i: SPONT
S: +nng + #layt?
D: Mm. Whaddaya think about some breakfast?
S: =m+#'+Nh?

D: You want oatmeal? Okay. *Daddy--
S: *xx

D: Let Daddy fix you some oatmeal. Here. (Let) me put you down. 'N I'll fix you some oatmeal.
S: piyz.

D: Okay. I will. I will do it. (rustles)
S: =go #di+. (S crawling)

D: Get it F

S: xrg+diN.

D: orange=juice F

S: uu#diN.

D: orange=juice F

D: SPONT
Tape 19A, Seth is 20.0, 6/20-21/82. Participants are Seth and Daddy. Location: Oklahoma City (D uses a very soft voice, sometimes even a whisper, throughout the tape) Transcriber: Ermile

D: Okay. It's June the 20th. (6 sec)
S: n#chunne.
g: June F
i: IM
D: Doodledoo. Listen. Didju toot in your pants?
S: no R
i: SPONT
D: I think you did.
S: jiit. (same intonation as 'did')
g: did F
i: IM
S: shiit.
g: did F
i: IM
S: #ng =kho.
g: N go F
i: IM
S: =ng =khoo.
g: N go F
i: IM
D: (mimicking) Ngo. Ngo. (whispered)
S: chiip.
g: tape F
i: SPONT
S: chiip.
g: tape F
i: SPONT
D: Yeah, that box the tape came in.
S: k++N.
g: *came'in F
i: IM
S: #ki’h+N.
g: *came’in? F
i: IM
S: ah—*
g: uh L
i: DISCARD
D: *I wantchu t' stay right here. I'll be right back. (9 sec)
S: aaN #ba@k. (high voice)
g: right back F
i: IM
S: aNh #b@k. (5 sec; banging)

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g: right back F
i: IM
S: m #bah? (high pitch)
g: N back R
i: IM
S: b+’ #ba’/. (water running; something wooden rubbing across ceramic tile; 14 sec)
g: + back F
i: IM
S: fw++. 
g: floor F
i: INIT
D: Hang on a minute. Let Daddy get the diaper. (away from mike)
S: ah+ #daypw+? (whispered; 10 sec)
g: ? diaper R
i: IM
D: Okay. Here we go. *Time for diaper.
S: *m #fwapiy?
g: N floppity R
i: SPONT
D: **Time for diaper.
S: **m #fwapiy?
g: N floppity R
i: SPONT
D: Okay.
S: +N #fwapiy?
g: N floppity R
i: SPONT
D: Okay. We'll do it. Get this diaper off.
S: + #awf. (This is a good sequence for "paying attention to the ends of utterances" - he answers the question on p. 1 but echoes the end of all the others.)
g: + off F
i: IM
D: Off.
S: +N #fa=piy? (urgently)
g: N floppity R
i: SPONT
D: Okay.
S: +N #fa=piy?
g: N floppity R
i: SPONT
S: +wN =fa#piy?
g: N floppity R
i: SPONT
S: +ng+N #fwapiy?
g: NG floppity R
i: SPONT
S: ng+N f--
g: NG floppity L
i: SPONT
S: goos.
g: gross F
i: INIT
S: *gos.
g: gross F
i: SPONT
D: *It's gross. It's gross. (5 sec)
S: chiip.
g: tape F
i: SPONT
S: chiyp.
g: tape F
i: SPONT
S: #awd+ #lifit. (softly)
g: ? floppity? F
i: DISCARD
S: n #kiyn. (softly)
g: N clean? F
i: DISCARD
S: n chish? (softly)
g: N tree+$? R
i: DISCARD
S: kan@p? (loudly; 6 sec)
g: come=up? R
i: INIT
D: What's that.
S: miymi.
g: mimi
i: SPONT
D: That's yer mimi. Okay. (softly)
S: m #fwapiy?
g: N floppity R
i: SPONT
D: Yeah. We're gettin' all so clean? An' then we'll floppity.
    We'll get all cleaned off 'n *then we'll floppity.
S: *m fwa-- ?
g: N floppity R
i: SPONT
S: m fwo?
g: N floppity? R
i: IM
D: Ready? Ready to floppity? Floppity, floppity, floppity,
    floppity, floppity, floppity, floppity, floppity, floppity. (shaking S
    back and forth)
S: mm-
g: MUMBLE L
i: DISCARD
S: m #fwappiy.
g: N floppity F

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D: Here comes your diaper.
S: m #fapiy.
g: N floppity F
i: IM
D: Want to floppity some more? Floppity, floppity, floppity, floppity, floppity. Okay.
S: m #fapiy.
g: N floppity F
i: IM
D: Here's your diaper.
S: M #fapiy.
g: N floppity F
i: IM
S: m #fapiy.
g: N floppity F
i: IM
S: m #fapiy.
g: N floppity F
i: IM
D: Mm hm?
S: m #fapiy.
g: N floppity F
i: IM
S: chip. (4 sec)
g: tape F
i: INIT
D: (softly) Putcher nice li'l pants back on. Your clothes.
S: kOs. (4 sec)
g: clothes F
i: IM
S: kOOS?
g: clothes R
i: IM
S: didldidl kOOS?
g: didldow clothes R
i: IM
D: Didju learn ta., didju learn ta stand up by yourself?
S: t+++.. (high squeal)
g: SQUEAL
i: DISCARD
Tape 21a, Seth is 20.2, 7/4/82. Participants are Seth, Daddy, and Uncle Bob. Location: Uncle Bob's house in Palestine, TX. Transcriber: Eileen

D: Here we are outside. Ya wan' see the bark? Wanna *see the trees--
S: *piys.
g: please F
i: SPONT
D: There. It's an *#oak tree.
S: -*#baak?
g: + bark R
i: IM
S: @dl + #baak?
g: DL + bark R
i: IM
S: dl + #baak? (high)
g: DL + bark R
i: IM
S: uz #shy? (high)
g: ? tree? R
i: DISCARD
S: #dl bak. (low and soft)
g: DL bark F
i: SPONT
S: chiy she. (low)
g: tree ? F
i: INIT
D: There's another tree. LOTS of oak trees. (4 sec)
S: n' *chiys?
g: N tree+$ R
i: IM
S: xxx xxx?
g: ? ? R
i: DISCARD
D: *We're at Uncle Bob 'n Aunt Marty's house in Palestine.
S: idl @t?
g: DL ? R
i: DISCARD
D: It's the 4th of July. Sunday mornin'.
S: paak.
g: bark F
i: SPONT
D: Bark.
S: paak.
g: bark F
i: SPONT
D: What it this.
S: liyz.
g: leaf+$ F
I: SPONT
S: ugulu*lu#si?
g: BABBLE tree
I: DISCARD
D: *Leaves.
S: + #chriy?
g: N tree R
I: SPONT
D: *Unhunh?
S: + n chriy?
g: N tree R
I: SPONT
S: n #shrey? (birds squawking)
g: N tree R
I: SPONT
S: n #shrey?
g: N tree R
I: SPONT
S: n #shrey?
g: N tree R
I: SPONT
D: I hear...
S: mb+l+ #kiyx?
g: NL geese+$ R
I: SPONT
S: +n#l+ =kiys.
g: NL geese+$ F
I: SPONT
D: No, there('re) no geese here. They don’t have geese. The geese are gone-gone. The geese are at Ben’s house.
S: *xx xx xx.
g: ? ? ? F
I: DISCARD
D: *Did we see geese--did we see geese at Ben’s house?
S: +w #hawngk.
g: + honk F
I: SPONT
D: Honk. The geese honk.
S: piys.
g: please F
I: SPONT
D: D’ you hear these birds? (4 secs) Blue jays. They’re blue jays.
S: w+dl #bak?
g: DL bark R
I: SPONT
S: dedl #bak?
g: DL bark R
I: SPONT
D: That’s such a #big tree down there!
S: baak.  (whispered)
g: bark F
i: SPONT
D: Such a big tree!
S: baak.  (6 sec)
g: bark F
i: SPONT
S: chiy.
g: tree F
i: IM
D: Great big *oak tree.
S: *n+ baak?
g: N? bark R
i: SPONT
S: dild+ baak?
g: DL bark R
i: SPONT
S: dild+ baak xx xx xx.
g: DL bark R F
i: SPONT
D: *What a nice yard.
S: *xx baak.
g: ? bark
i: SPONT
S: shuw.
g: tree F
i: INIT
D: There's a redbud tree.
S: + baak?
g: + bark R
i: SPONT
S: + chriy?
g: + tree R
i: IM
D: An' *a pecan tree?
S: *aa baak?
g: ? bark R
i: SPONT
S: didl chriy pak.  (quietly)
g: + DL tree bark F
i: IM+
S: baak-  (whispered)
g: bark F
i: IM
S: izl + baak?
g: ? bark R
i: IM+
S: xx baak xx xx kin chiy.  (whispered)
g: ? bark ? tree
i: DISCARD
S: xx paak xx xx xx xx xx xx pi.
g: ? bark ? ? ? F
i: DISCARD
D: Le's go in the house. I want Daddy ta put Daddy's shoe— I wan' put Daddy's shoes on. Daddy's #feet hurt. (17 sec; birds squawking)
S: mb@t.
g: bird F
i: SPONT
D: *Ooop! Ever' time I come through I let Willie in. (To Ub)
Ub: Oh that's fine. he stays in most o' the time...*
S: agaa *#sis?
g: ? shoe$ R
i: IM
D: I wanna get my #shoes on. Not used ta walkin' on-- not used ta walkin' arefooted.
S: +m #but.
g: N bird F
i: SPONT
S: b++t.
g: bird F
i: SPONT
D: Didja hear the birds out there?...
S: +n #kaak.
g: N ? F
g: DISCARD
D: He stops talkin' when th' machine goes off. (laughs)
S: +N #g+ =sa.
g: N go+3 off F
i: IM
Ub: I bet. Has he started doin' *that xx xx xx xx.
S: *+ba'.
g: I=bet F
i: IM
S: *ey.
g: tape F
i: SPONT
D: *This--
Ub: No, I mean, does he know when you're recording?
S: (fusses)
g: FUSS
i: DISCARD
D: I don't know. *I--
Ub: *xx xx xx xx xx
D: He doesn't **get self conscious--
Ub: **xx xx go against the grain?
D: No, no just ah-- (laugh) just seems ta-- seems not ta say the best stuff *when the thing's on.
Ub: *xx xx xx everyday-
S: m #buuk? ("let's book", i.e. "get out of here")

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D: (to Ub) Still talkin’ pretty good. (to S) Le’s go get Daddy’s shoes on. Get shoes on?
S: *+ =shssuw?
g: + shoe R
i: IM
Ub: *Think brought ’em back here.
S: e shu#sh+N?
g: + shoe+sh$'on
i: IM
D: Shoes on?
S: a #haat.
g: + hurt F
i: SPONT
D: Hurt feet. Hurt Daddy’s feet. Oh, here’s--
S: +N ge *#dawN.
g: N get’down F
i: SPONT
D: *Here’s the room where ya sleep. Here’s your-- here’s your bedroom.
S: be*druuw?
g: bedroom R
i: IM
Tape 22a, Seth is 20.3, 7/10/82. Participants are Seth, Daddy. Location: Tyler, Texas. Transcriber: Ann.

D: Good morning little bird.
S: #jushu? (soft)
g: ? R
i: DISCARD
S: 'm #f@n nan...
g: N fan on F
i: INIT
S: 'm #f@n an.
g: N fan on F
i: SPONT
D: Yeah I heard the fan come on. Look. See me. Come and see me.
S: #teddl.
g: Teddy F
i: SPONT
D: How's your Teddy doing? You throw everything under your bed. Let's get a fresh dry diaper. Oh? 'Scuse me. Did you toot?
S: nah?
g: no R
i: SPONT
D: No no. No no no. No no.
S: =fah#p+?
g: floppy R
i: SPONT
S: teyp-- teyp.
g: tape/ tape F
i: INIT
D: Yeah. Daddy's gonna do the tape this morning.
S: teyp.
g: tape F
i: SPONT
S: teyp.
g: tape F
i: IM
S: teyp.
g: tape F
i: IM
S: teyp. (5 secs)
g: tape F
i: IM
S: #fah=p+y?
g: floppy R
i: INIT
D: O.K. Here we go. Flop flop flop flop flop flop flop.
S: #fr+p/ =f+p/ #faap...#fa/ #fa #faap.
g: flop-flop-flop-flop-flop-flop-flop
i: IM
S: 'i #dedll...
Daddy F
i: INIT
S: 'id #deddl.
g: + Daddy F
i: SPONT
D: Yep.
S: 'id #deddl.
g: + Daddy F
i: SPONT
S: flaap.
g: flop F
i: INIT
S: teyp.. teyp.
g: tape tape F
i: INIT
S: n sez?
g: N stair+$$ R
i: INIT
S: sez.
g: stair+$$ F
i: SPONT
D: Let's go down the stairs.
S: d@wn-...
g: down L
i: IM
S: d@wnp.
g: down F
i: IM
S: #da=biy.
g: Dabee F
i: INIT
S: #da=biy.
g: Dabee F
i: SPONT
S: #da=biy.
g: Dabee F
i: SPONT
S: n #sh+. (Dabee does "get his sugar" by kissing his neck)
g: N sugar F
i: INIT
S: #da=biy. (5 secs)
g: Dabee F
i: INIT
S: layt....
g: light F
i: INIT
S: il #1ayt?
g: + light R
i: SPONT
D: Uh hum. Daddy turned on the light. (5 secs)
S: chiych.
g: switch F
i: SPONT
S: chiych.
g: switch F
i: SPONT
S: chiych.
g: switch F
i: SPONT
S: chiych.
g: switch F
i: SPONT
S: chiych.
g: switch F
i: SPONT
S: chiych.
g: high F
i: INIT
S: layt?...
g: light R
i: INIT
S: layt?
g: light R
i: SPONT
D: uh huh.
S: layt?....
g: light R
i: SPONT
S: layt...
g: light F
i: SPONT
S: ='ul #layt. (phone rings)
g: + light F
i: SPONT
D: Hang on a second. The phone's ringing. (In conversation with caller) Oh thank you. That's right. (10 secs)
S: chiych.
g: switch F
i: SPONT
S: chiych.
g: switch F
i: SPONT
S: chiych.
g: switch F
i: SPONT
S: chiych.
g: switch F
i: SPONT
S: (laughs)
g: LAUGH
D: (imitates Seth's laughter)
S: iy#hiy'.
g: ? F
i: DISCARD
S: trak...trak...
g: truck truck F
i: INIT
S: n #tr+k...
g: N truck F
i: SPONT
S: mp #tr+k. (10 secs)
g: N truck F
i: SPONT
S: layt.
g: light L
i: INIT
S: layt?
S: layt...
g: light R
i: SPONT
S: layt.
g: light F
i: SPONT
S: layt.
g: light F
i: SPONT
Tape 23a, Seth is 21.0, 7/19/82. Participants are Seth, Daddy. Location: Spencer St. apt. Transcriber: Esme

S: teip.
g: tape F
i: SPONT
D: This is July the 19th. What does Daddy have.
S: 'ig zi #teip?
g: NG + tape R
i: SPONT
D: uh huh?
S: #teip =piyz.
g: tape please F
i: SPONT
S: piyz.
g: please F
i: SPONT
S: piyz.
g: please F
i: SPONT
D: Mhm. There's Daddy's tape recorder.
S: h+ #chreiN.
g: + tray? F
i: SPONT
D: Train? Do you wanna e-eat? Do you want to eat that sandwich?
S: piyz.
g: please F
i: SPONT
D: There it is. Go for it. (6 sec) Let's do our routine. Do you wanta take bad medicine?
S: noh?
g: no R
i: SPONT
D: Do you wanta wash your hair?
S: noh?
g: no R
i: SPONT
D: Do you wanta go nigh-night?
S: noh?
g: no R
i: SPONT
D: Do you want me t' throw you on the floor? On your head?
S: noh?
g: no R
i: SPONT
D: Want me ta choke your neck?
S: piyz.
g: please F
i: SPONT
D: ('chokes' S's neck and growls playfully) I gonna choke it. I
gonna choke it. Choke choke choke.
S: n #teip?
g: N tape R
i: SPONT
S: n #teip?
g: N tape R
i: SPONT
S: piyz.
g: please F
i: INIT
D: There's the tape. I don't want you to getcher sticky hands on it, though. There's your juice, and your Cheerios and your sandwich.
S: #tabits.
g: rabbit+$ F
i: SPONT
S: N #chiyk+Nts.
g: N chicken+$ F
i: INIT
D: Rabbits and chickens?
S: (playing with sounds)
g: SQUEAL
i: DISCARD
D: What da the chickens say.
S: =gabo#gabo.
g: gobble-gobble F
i: SPONT
S: teip.
g: tape F
i: INIT
D: What does the turkey say.
S: #gabo#gabo.
g: gobble-gobble F
i: SPONT
D: What does a duck say.
S: (makes duck noises)
g: NOISES
i: DISCARD
D: What does the geese say. (sic)
S: haongk. (pause)
g: honk F
i: SPONT
D: Haonk. (5 sec)
S: #hiy+.
g: hair F
i: SPONT
S: #hiy+.
g: hair F
i: SPONT
S: #hi+. 
D: Where's... where's your.. nose. Started to reach for Daddy's nose and then reach for your nose. Where's Daddy's nose. Touchin' your nose. Where's Daddy's hair. (3 sec) Touch Daddy's hair. Where's Daddy's ear. (4 sec) Where's Daddy's ear. Reach Daddy's ear? (3 sec)

S: #iy+?

g: ear R
i: IM

D: Where's your face. (3 sec) Touch Daddy's face.

S: #he+

D: Where's your nose. Where's your nose. Touch Daddy's nose. Touch your OWN nose. (4 sec)

S: nuwz.

g: nose F
i: IM

S: nowz.

g: nose F
i: IM

S: nowz.

g: nose F
i: IM

S: =vava#vap.

g: vavava F
i: DISCARD

S: =vava#vai.

g: vavava F
i: DISCARD

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S: *v-
g: vavava L  
i: DISCARD 
D: *Vavavai.  
S: =vava#vai. 
g: vavava F  
i: DISCARD 
S: m+'m m+'m m+'m. 
g: BABBLE  
i: DISCARD 
S: m+'m m+'m. 
g: BABBLE  
i: DISCARD 
S: m+'m m+'m. 
g: BABBLE  
i: DISCARD 
D: Listen, tell me about your weekend with your Mommy. Did you see Kitty? (5 sec) 
S: #leid+l. 
g: Lady F  
i: SPONT 
D: Did you see Lady? 
S: ss@k.  
g: Zack F  
i: SPONT 
D: An' Zack? 
S: N kid+l.  
g: N Kitty F  
i: SPONT 
D: An' Kitty. 
S: s@k.  
g: Zack F  
i: IM 
D: An' Zack. 
S: #mammiy. 
g: Mommy F  
i: SPONT 
D: An' Mommy. 
S: ng #keyut. 
g: N call=you F  
i: SPONT 
S: ng #k+yuw. 
g: N call=you F  
i: SPONT 
D: What does Mommy call you. 
S: ng #ga'. 
g: N guy F  
i: SPONT 
D: Little Guy. What else does Mommy call you. 
S: ng gaiy.
g: N #guy F
i: IM
D: Guy.
S: 'ng ga.
g: N guy F
i: IM
S: ng #ko=kwiyk.
g: N ? F
i: DISCARD
S: chwiysh.
g: tree+$ F
i: INIT
S: twiyz.
g: tree+$ F
i: SPONT
D: Trees? (6 sec) (S laughs) Well, tell me about trees.
S: +N wiwiy.
g: N leaf+$ F
i: SPONT
D: Leaves.
S: m bak.
g: N bark F
i: SPONT
D: Bark.
S: n ch+ngk.
g: N trunk F
i: SPONT
Tape 25a, Seth is 21.2, 8/3/82. Participants are Seth and Daddy. Location: Spencer St. apartment; Makiki morning with birds. Transcribers: Ermile, Eileen

S: #g+=g+.
g: eat-meal F
i: INIT
S: #b+=b+.
g: eat-meal F
i: SPONT
S: #p+=p+.
g: eat-meal F
i: SPONT
S: #taa=fri.
g: trampoline F
i: SPONT
D: *What does--
S: *#che=vwiy.
g: trampoline F
i: SPONT
D: Does Mi-- What does Michael do.
S: #maakO'.
g: Michael F
i: IM
S: mhm/#ch+=biy.
g: N trampoline F
i: INIT
S: #j@N=priy.
g: trampoline F
i: SPONT
S: #jr@N=priy, (7 sec.)
g: trampoline F
i: SPONT
D: Is thatcher brush?
S: m+ #br+sh + #piyf?
g: N brush + teeth? R

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IM
D: Uhuh.
S: #br+sh + tiyf? (8 sec.)
g: brush + teeth R
i: IM
D: Didju wake up this morning? (15 sec.)
S: =wey#kup?(heavy plosive p; hard rubber? ball drops)
g: wake=up R
i: IM
S: #nuuuw.
g: throw F
i: INIT
S: baa?
g: ball R
i: INIT
S: bOO?
g: ball R
i: SPONT
S: bauw?
g: ball R
i: SPONT
S: bauw?
g: ball R
i: SPONT
S: pik-
g: please L
i: INIT
S: piys-
g: please L
i: SPONT
S: pik-
g: please L
i: SPONT
D: Where is your ball. Oh, your ball is under Daddy's table. (4 sec.) Your ball is under the table.
S: piys.
g: please F
i: SPONT
D: Go get it.
S: ip- (softly; 3 sec; noises)
g: ? L
i: DISCARD
S: 'o'?
g: oh R
i: DISCARD
S: baO?
g: ball R
i: IM
S: baO?
g: ball R

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i: IM
S: mow?
g: ball R
i: IM
S: 'ih #be=hi. (moves away from the microphone)
g: ? ? F
i: DISCARDED
S: 'ia-
g: ? L
i: DISCARD
S: #a'key. (creaky voice)
g: OK F
i: INIT
S: #akey.
g: OK F
i: SPONT
S: #akheiy.
g: OK F
i: SPONT
S: #akkey.
g: OK F
i: SPONT
D: Here's the ball.
S: bow?
g: ball R
i: IM
S: d+mp?
g: jump R
i: INIT
S: g+mp?
g: jump R
i: SPONT
S: ba'o. (hiccup?; 8 sec.; sound of bouncing/dropping)
g: ball F
i: IM
S: l+m #pugiy Oynow'. (4 sec)
g: ? ? ? F
i: DISCARD
S: ng gidit?
g: N get=it R
i: DISCARD
S: #h+b+k- (softly)
g: ? L
i: DISCARD
S: #de =bOw.
g: there + ball R
i: DISCARD
S: =MO =k++ #bOw?
g: ? ? ball R
i: DISCARD

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S: op?
g: ? R
i: DISCARD
S: op?
g: ? R
i: DISCARD
S: m #bO?
g: N ball R
i: DISCARD
S: kOwk?
g: ? R
i: DISCARD
S: bow?
g: ball R
i: DISCARD
S: #ba=ow.
g: ball F
i: SPONT
S: let #gow? (4 sec.)
g: let go R
i: DISCARD
S: #kiyx+t. (banging sound; bouncing noise)
g: kick=it F
i: INIT
S: jaaNp- ("jump" has a different meaning to S. "jump stairs" means to bound up the stairs. This may mean that the ball is bouncing, but more likely it's a causative with deleted subject: "I jumped the ball" -- made it jump)
g: jump L
i: INIT
S: jaaNp-
g: jump L
i: SPONT
S: #jap p+ #bOw?
g: jump=Dr=ball R
i: INIT
S: #jap p+ #bOw? (ball bouncing)
g: jump=Dr=ball R
i: SPONT
D: Oh, look how well you play ball. (6 sec; ball bouncing) How good.
S: #kiy=h+.
g: kitchen F
i: SPONT
S: #kiyx+n.
g: kitchen F
i: SPONT
D: Here's the kitchen. (noise; 4 sec.)
S: m' #k+p?
g: N cup R

266
i: SPONT
S: mmowk?
g: milk R
i: INIT
S: mowk?
g: milk R
i: SPONT
S: mwowk?
g: milk R
i: SPONT
S: mowk?
g: milk R
i: SPONT
S: mowk? (sharp noise)
g: milk R
i: SPONT
S: mowk?
g: milk R
i: SPONT
S: mowk? (car noises; shutting noise)
g: milk R
i: SPONT
Tape 27d, Seth is 22.0, 8/23-26/82. Participants: Seth and Daddy. Location: Spencer St. Apt. Transcriber: Eileen

D: Let's turn on this-- tape recorder.. for a little while, 'cause yer talkin' pretty good. (S is bathing in the kitchen sink)
S: +ng #gy@p?
G: N get up R
I: SPONT
S: g+ #s@p?
G: get up R
I: SPONT
S: t@w?
G: towel R
I: INIT
S: ng ge ap?
G: N get up R
I: INIT
D: You wanna get up?
S: piys-
G: please L
I: SPONT
D: An' then you wanna dry off?
S: piys-
G: please L
I: SPONT
D: With a towel?
S: piys-
G: please L
I: SPONT
D: Ok. (something plops in the water as S begins his next utterance)
S: bok?
G: block R
I: SPONT
S: +m pu pak?
G: N put block R
I: INIT
D: Say "pick me up, Daddy"?
D: say
S: mm =pi y+ #aa+p. (2 syll. of 'up')
G: N pick=you=up F
I: DISCARD
D: Say "pick me up, Daddy"?
D: say
S: =pi' 'i #d@diy?
G: pick + Daddy R
I: DISCARD
D: 'Kay. Let's let some water out- (sound of water going out of
the tub)
S: pH p+t? 
g: blub-blub? R 
i: SPONT 
D: Blub, blub, blub. (imitating the sound of the water draining out) 
S: pH p+t- (splashing sounds) 
g: blub-blub L 
i: SPONT 
D: 'Kay. Let's throw away this-- good stopper Daddy made. (the stopper for the disposal didn't hold water, so D would wrap a plastic baggie around it so S could sit in water for a while) 
Oh, don't let the #cup go down. (splashing noises). 
S: k+p? (7 sec) 
g: cup R 
i: IM 
S: mmm. 
g: ? F 
i: DISCARD 
S: k+p? 
g: cup R 
i: IM 
S: *k+p. 
g: cup F 
i: IM 
D: *Hm? Let's go. You ready? 
S: piys- 
g: please L 
i: SPONT 
D: We hafta bring the cups. (S insists on keeping the cups, a set of measuring cups) I remember. Otherwise we hafta fight. 
(Le)t's bring the cups. Well, come on? 
S: n+++#o? 
g: no R 
i: SPONT 
D: You wanta stay there? (something falls) 
S: piys- 
g: please L 
i: SPONT 
D: Okay-. 
S: n =tey k+#b@f? 
g: N take=a=bath R 
i: SPONT 
D: Okay. 
S: n =tey=ke#b@f? 
g: N take=a=bath R 
i: SPONT 
D: Okay with me. 
S: teyk+#baf? 
g: N take=a=bath R
n =t+ #a:f?

N turn=off R

n =t+ #a:f?

N turn=off R

n *t-

N turn L

*You want turn the water #on? (emphasizing 'on', correcting S)

piys-

g: please L

Okay? We're gonna turn it #on but, we're not gonna run hot water a long time 'cause-- you just asked to get out 'n' I ran the water out of it. I'm not gonna run water-- #too long fer you ta-- play in?

tey=k+#b@f?

take=a=bath R

D: Unless I do.

O #paw?

alx=pau R

S: d@wn?

down R

+w ge #'+p? (a cup falls)

want get up R

S: ge #'+p?

g: get up R

S: wan ge #'+p?

want get up R

S: wan ge #'+p?

want get up R

S: piys-

g: please L

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Run it hard. It's got soap in it. (more disposal noise; hard to hear D) You wanna get out? (6 sec; disposal off) Okay.

S: k+p?

D: Cup. Come on. Come on #get out of that bath. Come on. Come see me.

S: please (banging noise)

D: Come see me. (softly)

S: please

D: I gonna throw ya on the bed. I gonna throw ya down. (Their game is that S is the wild man; D throws him on the bed, and S practically dries himself off thrashing)

S: take=a=bath?

D: Let's go dry. Ya ready da dry?

S: (squeals, giggles)

D: I gonna throw ya on the bed. I gonna throw ya down.

S: (laugh)

D: Oh, I threw 'im so hard

S: k+p?
g: cup R
i: SPONT
D: There's yer cup? Now, we're gonna dry. Gonna dry yer legs--
Gonna dry yer knees. Gonna dry yer feet? An' we're gonna dry
yer--- the piggies? An' we dry yer mimi?
S: *mimi.
g: mimi F
i: IM
D: *An' we gonna dry yer tummy? Gonna dry-- yer arms. Dry yer arms
an' dry yer hands. Now we're gonna dry this arm, we're gonna
dry this hand. Now we're gonna dry yer elbow. We're gonna dry
#this elbow. We're gonna dry yer neck? An' yer ears? An' yer
nose? An' yer eyes? An' yer tee-- we're gonna dry yer teeth.
S: tiyf?
g: teeth+$ R
i: IM
S: da #tiyf?
g: dry teeth+$ R
i: IM
D: An' we're gonna flip ya over an' we're gonna dry yer bottom.
Dry yer bottom. Okay. Bottom is all dry? We're gonna dry yer
back. That's yer back. (S playing with cups) Dry yer hair a
little bit *dry yer hair
S: *xx xx.
g: ? ? F
i: DISCARD
S: xx xx?
g: ? ? R
i: DISCARD
S: n piys?
g: N ? R
i: DISCARD
S: k+p
g: cup F
i: INIT
S: k+p.
g: cup F
i: SPONT
D: Here.
S: +ng ge#+p?
g: N get'up?
i: SPONT
S: +ng ge+ #k+p?
g: N get'+ cup R
i: INIT
D: Ohh, yer gonna mess up Daddy's bed. Awright. I wish we had
Jenny here ta help mess up Daddy's bed.
S: n #j+mp?
g: N jump R
i: SPONT
Tape 29a, Seth is 22.2, 9/3/82. Participants are Seth, Daddy. Location: Walk to the cemetery at Thurston. Transcriber: Esme

D: Doggie. (traffic noise and a dog barking in the background; D carrying S down the stairs, jumping so S bounces)
S: #b+Nd=siyn.
g: bouncy F
i: SPONT
D: Bouncy, bouncy, bouncy, down the stairs we go. (traffic noise continues) September the third *after school we're
S: *#xx=xx
g: ? F
i: DISCARD
D: Jus got back from school 'n we're goin' across the street? (5 sec)
S: 'n =siy #vOD+r?
g: N see water R
i: SPONT
D: Here's the water. (A faucet on the grounds of the condo across the street)
S: w+?
g: BABBLE
i: DISCARD
S: wmmm.
g: BABBLE
i: DISCARD
S: am?
g: BABBLE
i: DISCARD
S: hm hmgoo?
g: N ? R
i: DISCARD
S: pis?
g: please R
i: INIT
D: Where're the roots?...(4 sec) Look what Daddy found.
S: tak.
g: rock? F
i: SPONT
D: What is it?
S: wak.
g: rock F
i: SPONT
D: Yeah. It'sa rock. 'at's right.
S: #@nk=yuuw.
g: thank=you F
i: SPONT
D: You're welcome Little Bird.
S: #w+k+m brd.
g: welcome bird F
i: IM
S: 'un #w+N =s+ntai?
g: N walk cemetary R
i: INIT
S: #nk=yuw. (offering rock to D)
g: thank=you F
i: INIT
S: #wO=k+m.
g: welcome F
i: IM
D: Thank you-
S: *wOk?
g: rock R
i: SPONT
D: *Handin' it ta me. Now Daddy has the rock.
S: piys- (holding out hand for rock?)
g: please L
i: SPONT
D: You want the rock? There a go.
S: =pi'+#/aap. (wants to be picked up)
g: pick=you=up F
i: SPONT
D: Come on. (picking S up; noise from tape recorder)
S: #t=rt=kOdr.
g: tape=recorder F
i: SPONT
S: #teypr=kOodr-
g: tape=recorder L
i: SPONT
D: Where d'ya wanna go?
S: n #jeNda'un?
g: N jump'down R
i: SPONT
D: (whispering) Where d'ya wanna go? What d'ya think? Where you wanna go next?
S: paak.
g: park F
i: SPONT
D: Here comes a car. (vehicle passes by) What d'ya wan' do next?
S: paak.
g: park F
i: SPONT
S: #aa=siy.
g: cemetery F
i: INIT
S: #t+eiy.
g: cemetery F
i: SPONT
S: #h+eiy.
g: cemetery F
D: Cemetery.
S: 'n =siy #hawrs?
g: N see flower+

i: SPONT
D: Ok.
S: pwiys-
g: please 

i: SPONT
S: n =sin #daon?
g: N sit down 

i: INIT
S: 'n =siy #sowns?
g: N see stone+

i: INIT
D: OK, we'll go see stones?
S: 'u #riys.
g: + leaf+

i: SPONT
D: Leaves? What else do they have down there.
S: *#wiyi.
g: leaf-

i: IM
D: *Is your bottom sore? Is your bottom sore? You been sayin'
   "sore" when we changed yer diaper.
S: 'n =siy #sowns?
g: N see stone+

i: SPONT
D: Yeah. We're gonna go see the stones.
S: go =siy #vODr?
g: go see water 

i: IM+
S: #siy =waDl.
g: see water 

i: IM+
S: yeh?
g: yes 

i: INIT
S: #wadl.
g: water 

i: INIT
S: hiy #wadl.
g: here water 

i: SPONT
S: =pik #it?
g: pick it 

i: INIT
S: pik it-
g: pick it L 

i: SPONT
S: #faw+s.
g: flower+$ F
i: INIT
S: pik + #fa=w+s.
g: pick + flower+$ F
i: SPONT
D: OK? Here they are. (traffic noise)
S: =piiya#ap. (4 sec)
g: pick=you=up F
i: SPONT
S: m =pik + #fawis?
g: N pick + flower+$ R
i: INIT
D: You want me to pick you one?
S: piys-
g: please L
i: SPONT
D: OK. (D smells the flower) Oh it smells good. Smell it.
S: w+N #fey'i?
g: want smell? it R
i: IM+
S: ap-
g: up L
i: INIT
S: =piy+#aap.
g: pick=you=up F
i: SPONT
D: Okay.
S: #fwaw+z.
g: flower+$ F
i: SPONT
D: (smells the flower again) Oh, stick it in Daddy's face. Look out for that dog shit.
S: m vO?
g: N wall R
i: SPONT
D: What is this?
S: sownz.
g: stone+$ F
i: SPONT
D: Uhuh?
S: shownt.
g: stone-$ F
i: SPONT
S: =sho #wOw.
g: stone wall F
i: SPONT
D: Stone wall. That's right.
S: nanano? (rejecting flower)
g: no-no-no R
D: You don't want it? You wanna go across to the cemetery?
S: piys-
g: please L
i: SPONT
S: #t@ng kyuw =d@dl.  
g: thank=you Daddy F  
i: SPONT  
S: @b+ #yuuws!  
g: apple juice F  
i: INIT  
S: 'î+p miy'- jss.  
g: help me juice F  
i: INIT  
D: OK. It's October the second. Now let's- let's try with a--  
let's try with another tape. See if we can do somethin' this  
morning now. I wanna put the tape recorder up here- OK? Now-  
you leave it alone, I'm gonna leave it right up here. You want  
some more apple juice?  
S: pish-  
g: please L  
i: SPONT  
D: OK. (4 sec; D moves about)  
S: #d@h yuw. (offering spoon to D)  
g: thank=you F  
i: SPONT  
S: #d@@ kyu.  
g: thank=you F  
i: SPONT  
S: #d@ kyuw.  
g: thank=you F  
i: SPONT  
S: #d@ kyuw.  
g: thank=you F  
i: SPONT  
S: #d@ hyuw.  
g: thank=you F  
i: SPONT  
D: *Yeah I'm gonna take it. I'm gonna take that spoon and feed ya.  
Just a second let Daddy give you some juice. There you go,  
there's the juice. There. (clunk)  
S: gi #miiy. (4 sec; noises)  
g: give me F  
i: SPONT  
S: hay s@f?  
g: hi Seth R  
i: INIT  
D: Say "Hi Daddy".  
d: say
S: #hay =d@dl. (5 sec; clicks)
g: hi Daddy F
i: DISCARD
S: w+ de =teyb+#wod+?
g: want D+ tape=recorder R
i: INIT
S: 'a de =teyp ri#kodl?
g: + D+ tape=recorder R
i: SPONT
D: Uh huh?
S: a de teyp +#kod+?
g: + D+ tape=recorder R
i: SPONT
S: mem fish- (urgently)
g: more please L
i: INIT
D: Here it comes. (D feeds S)
S: hiy #k+ms. (4 sec; S eating)
g: here come+3 F
i: IM
S: hay #d@dl.
g: hi Daddy F
i: DISCARD
D: *Hi Seth.
S: *hay s@f.
g: hi Seth F
i: IM
D: Hi Seth. Hi Daddy. (4 sec)
S: 'ng #gyseset.
g: N ? F
i: DISCARD
S: 'n #din+. (Dana is a house guest.)
g: N Dana F
i: INIT
S: pi #b+t1- (S knows a song about "peanut butter & jelly"; 10 sec; S eating and drinking)
g: peanut=butter L
i: INIT
S: abu #zhuw? (9 sec; S drinking and eating)
g: apple juice R
i: INIT
S: #ngathey.
g: ? F
i: DISCARD
S: #naNthey.
g: ? F
i: DISCARD
S: waNwaNwaNwaNwaNwaN-
D: Watta ya say touchin’ Daddy’s nose and sayin’ nading.
S: wawawawawa-
g: SP
i: DISCARD
S: wawawawa-
g: SP
i: DISCARD
D: Here. Eat. (10 sec)
S: pi b+d+ #chidiy. (4 sec; rustles)
g: peanut=butter jelly F
i: SPONT
S: tyih- (clatter)
g: ding L
i: INIT
S: tyih- (18 sec; S hits strap with buckle on highchair leg; clatters; feeding sounds)
g: ding L
i: SPONT
S: #’iyt #k+m+.
g: eat come=on F
i: INIT
S: #’i +m+.
g: eat come=on F
i: SPONT
S: =’it k+#maan. (17 sec; buckle noises; clatters)
g: eat come=on F
i: SPONT
S: bi b+d++ #jel=liy!
g: peanut=butter jelly F
i: SPONT
S: bi b+d++ (4 sec; D feeds S)
g: peanut=butter L
i: SPONT
S: bi b+d++
g: peanut=butter L
i: SPONT
S: meow piysh- (urgently)
g: more please L
i: INIT
S: mo pwish- (urgently)
g: more please L
i: SPONT
D: W(e)ll here’t comes. Here it comes. (8 sec; alarm clock buzzes)
   Woops. There goes the clock- (7 sec)
S: d+t-- =dis+ #k@@k.
g: that/ this’a clock F
i: IM+
S: =dis+ #k@@k-
g: this’a clock L
i: IM+
D: There's the clock. (imitates S's intonation)
S: '@@k- '@@k- (imitating the alarm sound)
g: SP
i: DISCARD
D: Goes "@@@k". (14 sec; D feeding S)
S: be be #beh-
g: BABBLE
i: DISCARD
S: bey.
g: BABBLE
i: DISCARD
S: be beh bey.
g: BABBLE
i: DISCARD
S: be bey/ bey. (17 sec; D feeding S; S clinking buckle)
g: BABBLE
i: DISCARD
S: 'e b+d+ #jelley.
g: peanut=butter jelly F
i: INIT
S: pey b+d+-- #jeliy. (4 sec)
g: peanut=butter jelly F
i: SPONT
S: =bay b+d+.
g: peanut=butter F
i: SPONT
S: #chuw +t.
g: chew=it F
i: INIT
S: #chuw +t.
g: chew=it F
i: SPONT
S: #juw +.
g: chew=it +
i: SPONT
S: '@@k- (imitating alarm clock)
g: SP
i: DISCARD
D: (laugh) n@@k. (imitating S; 5 sec)
S: piy #b+dl-- #jeliy.
g: peanut=butter jelly F
i: SPONT
D: Peanut butter and jelly. (6 sec)
S: #b+d+.
g: butter F
i: IM
S: #chuw it-
g: chew=it L
i: INIT
S: #chuw +
g: chew=it L
i: SPONT
S: chik.
g: chew=it F
i: SPONT
S: #ch+dl.
g: chew=it F
i: SPONT
S: t@’ mmmm/ #g@=beiyy.
g: ?? jelly F
i: DISCARD
S: piy #b+dl.
g: peanut=butter F
i: INIT
S: pi #b+d+r.
g: peanut=butter F
i: SPONT
S: #j@liyy. (3 sec; S clinking buckle)
g: jelly F
i: INIT
APPENDIX C

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