TABLE B-2
(continued)

ITEM	SUBHEADING	HEADING	
sort tort	crawling animals	Forest animals 2 .526	Forest Animals Total .719
wood pecker	burrowing animals		
bamboo worm	worms		
.531			
red and blue lizzard	crawling animals .559		
crocodile			
.666			
sky cassava snake	tree animals .834		
Campbell's monkey			
red colombus monkey			
potto			
.836			
bush tail porcupine	burrowing animals .879		
armadillo			
.875			
black deer	hoofed animals .904		
grey deer			
red deer			
bush cow			
zebra antelope			
.751			
horn bill	burrowing animals	Forest animals 3 .737	
tree squirrel	tree animals		
crawling turtle	crawling animals		
.525			
pumpkin		foods 1 .710	Foods Total .669
egg plant			
.751			
beans	vine fruits .794		
water yam			
.751			
corn		foods 2 .633	
air potato			
banana			
sugar cane			
oil palm			
.498			
calambus palm .264	wild vines .820		
rattan palm			
cocoa 1.00	cultivated tree		
.061			
scorpion	burrowing animal		

APPENDIX C : Free Association

The free-association technique has had a long history in psychology. Originating in the association school of British philosophy, and later used as a psychoanalytic diagnostic technique, free association has recently become a major tool for the study of verbal behavior. The technique itself is quite simple. The informant is simply presented with a series of words and asked to respond to each word with the first word that comes to mind.

Experimental work over the past thirty years has led to several major propositions as to the nature of the relation between words and their associates. Early theorizing centered on the idea that words and their associates achieved their relation to each other simply because they occurred together previously in the person's experience. A second view, still within the behaviorist tradition, treats words and their associates as related through commonly conditioned mediators, that is, words and associates are similar to the degree that they are conditioned responses to common stimuli.

Until recently, most word-association studies required a subject to give a single response to a given stimulus word. Results from such studies with Americans generally yielded three or four responses of relatively high frequency common to the entire group of subjects, and a series of thirty or forty low-frequency or idiosyncratic responses. In more recent work the informant is required to give more than one response. The high-frequency responses still remain, but the relative frequency of the low-frequency responses increases drastically. These new findings have led H. Pollio (1966) to suggest that the associates to a given word represent members of the same semantic class as the stimulus word (words that have the same or very similar mediational elements to which they have been paired). The high-frequency responses (the so-called primary, secondary, and tertiary responses) are the responses that are, in general, those that are closest to the stimulus word in the hypothetical semantic space. The low-frequency associates, while still possessing mediational elements in common, are further away from the stimulus word in the semantic space.

Theoretical considerations such as these have led Deese (1962) and his associates to the position that stimulus words are similar to each other to the degree that they elicit common responses. This definition of similarity gives rise to the data-analysis techniques that underlie the discussion of free association in Chapter 3.

Once a subject's associations to a set of words have been collected, a matrix is constructed in which the columns represent the various stimulus words and the rows represent the response words given to each of the stimuli. In our work a great many different response words were given when an entire group of subjects was considered as a unit, so the resulting free-association matrices had a great many rows. Each time a new response word appeared, it was added to the row of response terms. Each time a response word that had already occurred was encountered again, its occurrence was marked in the appropriate cell of the matrix. For example, if "lemon" was first given as a response to the stimulus word "banana," it was listed as a response and a one was entered for the cell "banana-lemon." If "lemon" occurred as a response to other stimulus words, one was entered in the appropriate stimulus-response cell, and if other subjects gave "lemon" as a response, its frequency of occurrence in the appropriate stimulus-response cells was noted.

The result of this procedure was a matrix with the frequency counts for all stimulus-response entries, summed over a group of ten or more subjects (ten was the basic group size).

Then, following Deese (1962), we calculated a similarity score representing the extent to which each of the stimuli in the list in question tended to elicit the same response words. This score is defined as the number of times that two stimuli elicit common responses, divided by the total number of times that either stimulus word in the pair under consideration elicits those responses. For example, if banana and coconut each elicit twenty responses and in ten of these cases they are the same word, the similarity score would be .5 (technically, the formula for calculating similarity [S] is

$$S = \frac{2N\,(i \cdot i)}{N(i) + N(j)} \quad ,$$

where i is the number of responses to stimulus word i and j is the number of responses to stimulus word j. Note that S varies between zero and unity).

Once the similarity scores for a set of stimuli have been calculated, it is possible to treat them in various ways for purposes of analysis. Two analyses play a prominent role in the analysis of stimuli contained in Chapter 3. First, the average similarity between stimuli within hypothetical classes is compared with the similarity scores for stimuli that are hypothesized to come from different classes. If our assumptions about the nature of the classes are correct, within-class similarity scores should, on the average, be higher than between-class scores. Second, the similarity scores are treated as psychological "distances" between stimuli, and the data are analyzed in terms of Johnson's (1967) hierarchical clustering program to determine if not only classes, but hierarchical relations among items and classes, exist.

APPENDIX D : Additional Data from Similarity - Mediation Study

Table D–1 summarizes the object choices for the mixed-category pairs. As in the case of intracategorical relationships, the three non-high-school groups are similar in their choices of the objects to mediate the intercategorical pairs. The high-school group is also similar to the other groups except for the food-utensil pairing where the high-school students respond mainly with a food item, while the others usually name a tool.

In order to decide whether categorical membership was used as a basis to choose responses, we can calculate the proportion of choices that are within the categories defined by the two constraining objects. These percentages are presented in Table D–2.

Although there is considerable variation in the responses to different types of constraining objects, in two of the three types, the high-school students make intracategorical choices 85 percent of the time—a percentage significantly higher than the 58 percent expected by chance (there are eight possible categorical choices among the set of thirteen choice items). The other groups do not differ significantly from chance values. Finally, only the high-school group shows a predominance of static justifications of their choices (see Table D–3).

TABLE D-1

Category Membership of Mediating Object: Mixed Pairs

CONSTRAINING OBJECTS: FOOD-UTENSILS			
	MEDIATING OBJECT		
	FOOD	TOOL	UTENSIL
NC[a]	0	24	8
A[b]	4	18	8
SC[c]	0	28	5
HS[d]	17	6	9
CONSTRAINING OBJECTS: FOOD-TOOLS			
	MEDIATING OBJECT		
	FOOD	TOOL	UTENSIL
NC[a]	6	22	5
A[b]	2	23	3
SC[c]	0	28	4
HS[d]	8	22	1
CONSTRAINING OBJECTS: UTENSIL-TOOL			
	MEDIATING OBJECT		
	FOOD	TOOL	UTENSIL
NC[a]	10	21	0
A[b]	5	18	4
SC[c]	10	18	1
HS[d]	7	19	5

[a]Ten- to fourteen-year-old nonliterate children.
[b]Nonliterate, traditional adults.
[c]Ten- to fourteen-year-old schoolchildren, grades two to five.
[d]High-school students, age sixteen to twenty.

TABLE D-2

Percentage of Responses within Either of the Constraining Categories

	FOOD-UTENSIL	FOOD-TOOL	UTENSIL-TOOL	AVERAGE
NC[a]	25	85	70	60
A[b]	40	90	81	70
SC[c]	15	88	66	57
HS[d]	81	97	77	85

[a]Ten- to fourteen-year-old nonliterate children.
[b]Nonliterate, traditional adults.
[c]Ten- to fourteen-year-old schoolchildren, grades two to five.
[d]High-school students, age sixteen to twenty.

TABLE D-3

Justifications for Intercategorical Pairs

	STATIC	DYNAMIC
NC[a]	0%	100%
A[b]	2	98
SC[c]	3	97
HS[d]	74	26

[a]Ten- to fourteen-year-old nonliterate children.
[b]Nonliterate, traditional adults.
[c]Ten- to fourteen-year-old schoolchildren, grades two to five.
[d]High-school students, age sixteen to twenty.

APPENDIX E : Details of Free-Recall Experimental Procedures and Results of Standard Experiments

Instructions

Because they are an important part of the experimental procedure, and because a standard set of instructions evolved over a series of pilot studies, we will give a detailed account of the way in which these instructions developed.

We began with a simple set of English instructions, the sense of which was that the subject would be told a list of common things that might be found at the market of which he was to try to remember as many as he possibly could. These instructions were translated into Kpelle by one of our informants and tried out on a few subjects. They were then modified slightly and translated back into English by another informant who had not been present during the initial translation.

During a rather extensive series of pilot studies, the instructions were simplified until the final instructions came to read as follows in a literal translation:

You and I will do a play. The play which we will do will be about things we can do work with. I will call all the things' names first before you call their names. Listen to me carefully. (After the list had been presented) We are finished. You call the things' names now.

This rather sparse set of instructions yielded results comparable to our more elaborate initial instructions and met the criterion that our informants felt comfortable that their subjects understood the task.

Each experimenter was required to memorize the instructions, which were typed on a sheet of paper carried in each experimental notebook. In order to assure that experimenters followed a random order in running subjects from various groups and that they presented the lists in a proper manner, each recall experiment was laid out in advance with each data sheet labeled as to the condition required and each trial labeled with the list order to be used for that trial. The experimenters were allowed to violate the order when it was necessary because, for instance, a ten-year-old schoolboy was not to be found, but an illiterate adult was at hand. In certain cases there was reason to believe that list orders were not followed properly; in such cases, information about serial-order effects was lost.

Results of Experiment 2

The largest effect on the number of items recalled per trial was produced by the type of stimuli presented; objects (10.2) were better recalled than words (9.4). Presenting the stimuli in blocked order (10.1) also enhanced recall relative to random ordering (9.5). Although the educated groups (10.0) recalled slightly more than the nonliterate groups (9.6), the difference was not significant. However, the lack of an overall effect of education masks an interesting and unexpected difference in the way the educated and nonliterate populations were affected by the two kinds of stimulus materials (see Table E–1).

The following conclusions follow from the data in Table E–1: (1) when words were presented, the nonliterate subjects were slightly (but

TABLE E-1

Recall as a Function of Education and Stimulus Materials

	WORDS	OBJECTS
Educated	9.1	10.8
Nonliterate	9.6	9.6

reliably) superior to the educated subjects; (2) when objects were presented, the educated subjects were superior. This finding clearly clashes with the results of Experiment 1, where the educated subjects were superior when stimuli were presented orally. The contradiction may be real, but it is more likely that it is the result of a sampling error. When we compare the performance of the educated and noneducated ten to fourteen year olds in the first experiment for the clusterable lists, we find that the superiority of the educated subjects amounts to only .2 items per trial or one item over the course of the whole experiment. Consequently, it seems best to take a cautious attitude toward the relation between recall and education for orally presented material; there appears to be little difference between the two populations represented by our groups. Other data indicate superiority of groups with more education than that represented thus far, so the matter need not concern us unduly here.

Perhaps more interesting than differences in absolute levels of recall among the various groups is evidence that serial organization differs systematically among groups. An example of such a difference is shown in Figure E–1, which plots accuracy of recall as a function of education, the nature of the stimulus materials, and serial position.

Although Figure E–1 is a little difficult to interpret because of the irregularity of the curves, a careful examination will reveal that when objects are presented, a classical bowed serial-position curve is produced; however, when words are spoken, there is no relation between position and accuracy for the nonliterate subjects and only recency for literate ones.

These findings bear a rather close resemblance to the sequence of changes in serial-position responding that are posited by several contemporary theories of memory (Atkinson and Shiffrin, 1968; Bower, 1967; Waugh and Norman, 1965). According to these theories, a recency effect reflects the advantage of items presented just prior to recall because they are still in a readily available short-term memory state; primacy occurs because items occurring early on the list receive less interference from subsequent items and thus enter a long-term storage condition more easily.

Viewed in this context, Figure E–1 suggests that when objects are presented, both educated and nonliterate subjects manifest the use of both long-term and short-term retrieval strategies, but when words are spoken, the educated subjects take advantage only of the accessibility of items in short-term storage, while the nonliterate subjects show no dif-

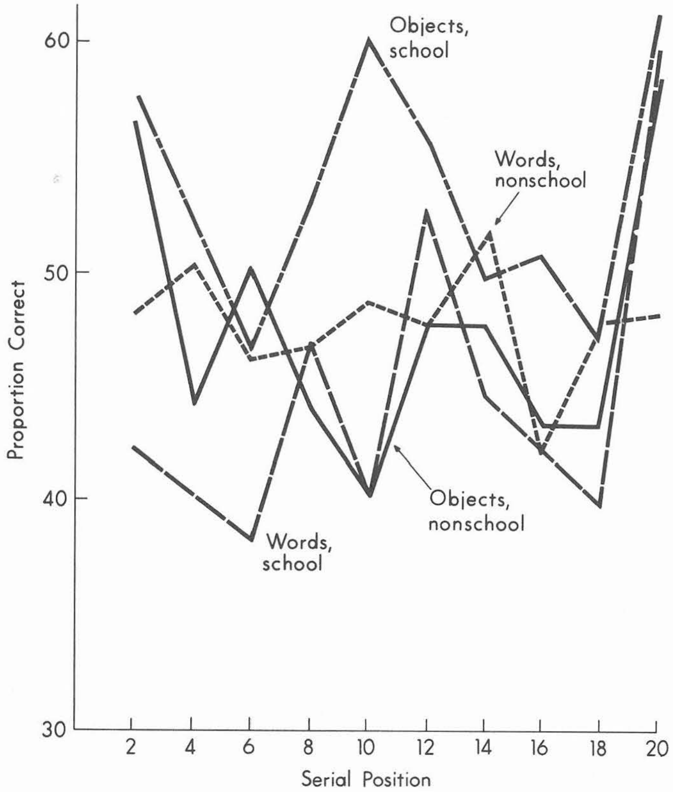

FIGURE E–1 Proportion of Items Recalled as a Function of Serial Position, Kind of Stimuli and Educational Status

ferential use of serial properties of item presentation. Because of their theoretical suggestiveness, we will pay close attention to serial-position phenomena in our cross-cultural comparisons.

One further aspect of the recall data in Experiment 2 should be mentioned before considering measures of semantic organization; recall improved much more markedly over successive trials than was the case for Experiment 1. On the average, Trial 5 produced approximately four more items than Trial 1. A significant interaction between the type of stimuli presented and the trials indicated that the improvement over trials was rapid only for the groups presented objects; little improvement occurred if words were used as stimuli.

The Effects of Age and List Organization on Recall among Yucatec Mayans

The basic manipulations that were the focus of attention in our initial Kpelle experiments were included in the first study in Yucatan: clusterability of the list, list structure, and age (the next experiment discussed in this appendix considers education). These data were collected in December 1967 in Ticul, Yucatan, Mexico. (We are indebted to Professor Volney Stefflre and his associates in Ticul for their assistance in the conduct of this research.)

Subjects, Materials, and Experimental Design

There were ninety-six subjects, thirty-two each in the age groups six to eight years (average 7.9), eleven to fifteen years (average 12.9), and eighteen to sixty-six years (average 29.4). All of the subjects spoke both Yucatec Mayan and Spanish.

The stimulus words were chosen to be rough equivalents of those used in the American and Kpelle experiments described earlier. The clusterable list was made up of twenty nouns from the classes which may be translated as food, tools, clothing, and utensils. The nonclusterable list was made up of other nouns chosen to be familiar but not obviously clusterable.

The instructions and stimuli were read to the subject in Mayan. In all other respects, the procedure was designed to be as similar as possible to the standard procedure described at the beginning of this appendix.

Within each of the subject populations, four groups of eight subjects each were selected haphazardly to provide for the factorial combination of clusterable versus nonclusterable lists and blocked- or random-presentation orders (for the nonclusterable list, the blocking was done on arbitrary groups of five items, which were always presented adjacent to one another).

Results

As we have come to expect on the basis of our previous studies, recall increased with age for our Mayan subjects. However, the increase occurs between the six to eight year olds (8.6) and the eleven to fifteen year olds (10.2), who are slightly, but not reliably, superior to the adults (10.0). Neither clusterability of the list nor blocking of stimuli had any significant effect on recall, nor were there any significant interactions among these variables.

Like the American subjects, but unlike the Kpelle, there is a marked improvement across trials for all groups with oral presentation. The average number recalled increases from 6.9 on Trial 1 to 12.0 on Trial 5.

Both clusterability and blocking interact with serial position, but not the same way that we observed in our American studies. The main difference in the shapes of the curves is that the more easily organized list shows only recency (see Figure E–2).

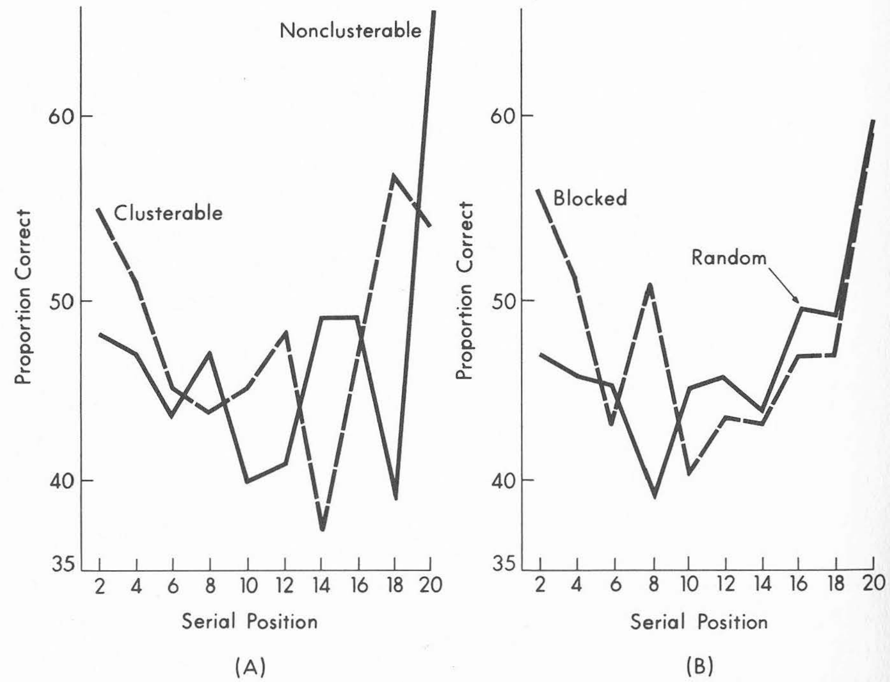

FIGURE E–2 Proportion of Items Recalled as a Function of Serial Position and (A) Clusterability of the Stimulus List; (B) Random or Blocked Ordering of Stimulus List

Looking next at semantic organization, we find that blocked groups show a significant, but very moderate, degree of clustering ($z = .61$), while clustering is not significant for the random lists ($z = .29$).

Overall, the Mayan groups show slight negative correlation between presentation and recall orders ($r = -.15$), which is consistent with the relatively large amount of recency observed in all groups (Figure E–2), and this negative tendency increases over trials in a manner similar to that which we observed in the American data.

The Effect of Education and List Structure on Recall among Yucatec Mayans

In Ticul, Mexico, as in Kpelleland, school attendance is far from universal and among the adult population in particular, there are many people who have never attended school. Consequently, it was decided to investigate the effect of school attendance on recall. In addition, blocked and random clusterable lists were presented.

Subjects, Materials, and Experimental Procedures

The ninety-two subjects were chosen haphazardly from the adult (average age = 33 years) population of Ticul, with the added requirement that the subject speak both Mayan and Spanish.

The forty-six subjects who had attended two or more years of school (average, 3.0) and those who had not attended school were divided into two groups. One was presented the random-list; the other, blocked-list orders.

Results

Consistent with the findings in the previous experiment, presentation of the list in blocked order did not generally enhance recall. School attendance also failed to affect overall performance. However, the educated and uneducated groups differed slightly in their responses on the blocked and random lists. The educated subjects recalled the blocked and random lists equally well, but the random list was significantly more difficult than the blocked list for the uneducated subjects. As with the Kpelle, there seems to be little overall effect of a few years of education on recall; however, in each case there are indications that where a superiority of the educated subjects does exist, it is in those conditions that are generally most favorable to recall (blocked presentation, objects).

In other respects the results of this experiment are essentially replications of those obtained in the previous Yucatec Mayan experiment. There is improved recall over trials, a moderate degree of clustering (average $z = .72$), and a slight overall negative correlation between presentation and recall orders. The correlation between presentation and recall is positive on Trial 1 and increasingly negative thereafter. There is also an interaction between list structure and serial position similar to that observed previously with these subjects; there is less primacy with the random than the blocked list.

The Properties of Recall with Another Liberian Tribal Group, the Vai

To the west of the Kpelle, in the coastal area bordering on Sierra Leone, is the Vai tribe, numbering perhaps 25,000. The Vai represent an interesting contrast with the Kpelle in several respects, and hence the results of our experiment with them were thought worth presenting here. For one thing, the Vai have long been in contact with Western civilization, first as middlemen in the slave trade and later in other commercial and governmental capacities. In Liberian folklore about tribal characteristics, the Vai are considered a dominant, intelligent, and arrogant group used to being served by others, whereas the Kpelle are more often cast in the role of the slow-witted servant.

Of more immediate relevance than these tribal stereotypes is the fact that for the past 150 years, the Vai have had an indigenous form of writing, a syllabic system that was probably inspired by the widespread presence of the Koran among Moslem missionaries. (See Dalby, 1967, for an interesting account of the invention of Vai script and its early spread among the Vai. Dalby's article also contains descriptions of other indigenous scripts, some modeled on the Vai, but none are of so early an origin or so widespread in usage.)

The presence of a system of writing among a tribal people who are in many respects similar to the Kpelle seemed to afford an excellent opportunity to test hypotheses about the "consequences of literacy" (Goody and Watt, 1962), without confusing literacy with the difference between the rain forests of Liberia and the suburbs of southern California.

Subjects, Procedures, and Experimental Design

The subjects were sixty Vai adults ranging in age from eighteen to fifty with an average of thirty-five years. Half of the subjects were literate in Vai script, while the other half were not. None of the subjects was literate in English, although a few had attended school for one or two years. In general, this sample of Vai adults was more Westernized than the nonliterate Kpelle adults used in the experiments described earlier. Only twenty-three of the sixty were rice farmers; the remainder were tailors, carpenters, gasoline attendants, and other "specialists."

The thirty subjects literate in Vai script and the thirty not literate in Vai script were randomly assigned to two conditions: fifteen subjects in each group used clusterable lists, while fifteen used nonclusterable lists. These lists were composed of Vai translations of the lists used in our initial study among the Kpelle (Table 3–2). In all respects except the language used, procedures followed those of our initial Kpelle study. The experiment was conducted by Arnold Kandakai, a Vai student attending Cuttington College.

Results

The most interesting results of this experiment with the Vai were the following: (1) the two groups that were literate in Vai script recalled more (11.5 vs. 10.0) words than the nonliterate groups; (2) for the clusterable list there was a significant amount of clustering for the literate group ($z = .76$), but not for the nonliterate group.

There was no reliable difference in the number of items recalled between clusterable and nonclusterable lists for either group. In other respects, also, the performance of the nonliterate Vai was quite similar to that which we observed in Experiment 1 with the Kpelle. Recall was of the same order of magnitude, and improvement over trials was negligible, averaging only .6 items.

APPENDIX F: Technical Aspects of the Conduct of Cross-Cultural Memory Research

When we first began to run pilot studies on the problem of free recall, we worked with an informant whose father was a well-known town chief, living in the administrative center of Gbarnga. One of the striking features of the recall protocols collected by this experimenter was a relatively large number of intrusions (items given as recall items by the subject that were not a part of the original list); moreover, in about 20 percent of the cases, the subject said "cow" as one of his words. Very rarely was "cow" given as a response in any of our other experiments, and as a rule, the number of intrusions was fairly low. We can only speculate, but it seems quite possible that the fact that our experimenter was the son of a chief (one of whose characteristics is the possession of a fine herd of cows, a rare attribute in Liberia) influenced the set of words used by subjects when asked to recall. Such idiosyncrasies clearly affect both intra- and cross-cultural comparisons.

One way to assess such difficulties, if not eliminate them, is to include evaluation of experimenter differences as a standard part of every research design. Another strategy is to use overlapping experiments in which at least one condition in each new experiment overlaps with (is identical to) a condition in an earlier experiment. In either case differences among experimenters or subject populations can be evaluated.

These precautions, which are summarized so easily, are very difficult to carry out systematically in practice. Wherever possible, we followed one or the other strategy, but in some cases it was not possible to do so.

The major difficulty with making the "experimenter effect" a part of every experimental design was the availability of only a limited number of experimenters, combined with the difficulty that experimenters often had in mastering one or two procedures, to say nothing of the ten or more different kinds of experiments that we conducted as a part of our research project. The difficulty with the overlapping-groups strategy involved not only the limitations on experimenter time, but limits on populations and money. The fact that we wanted to work with experimentally naive subjects meant that the experimenter was forced to move from town to town when the number of subjects he was required to run was very large. Although care was taken to randomize the order in which various treatment groups were run (in order to avoid confounding treatments and towns), the burden of partially replicating each experiment seemed too high a price to pay for purity. The time and money involved also represented a high price in more recognizable terms, so that all too often we skimped on replications. Unfortunately, we have paid the price of our decision in more than one instance in which we must remain ambiguous about the causes of our results. All we can do in such circumstances is to report the problems honestly for the reader's evaluation.

These remarks are immediately relevant to Experiments 1 and 2, which included the experimenter as a systematic part of the research design. Each of the groups of ten subjects in Experiments 1 and 2 was further subdivided into two groups of five, each run by a different experimenter (both of whom were native Kpelle speakers and students at Cuttington College). The performances reported earlier for these experiments were averages for experimenters Richard MacFarland and Paul Mulbah. In the first free-recall study, the overall performance of the two experimenters was the same for both number recalled and clustering scores; hence, one would assume a lack of experimenter effect. Unfortunately, the experimenter interacted significantly with a number of other effects. For MacFarland, the difference between subject populations was smaller than for Mulbah, while the difference among clusterable and nonclusterable lists was greater for MacFarland than Mulbah. Although there was no difference in clustering scores attributable to experimenters, Mulbah produced more pronounced recency and primacy than MacFarland.

Similar interactions were observed in the second free-recall study, where the differences among groups and serial position was generally smaller for MacFarland. Moreover, the difference in clustering and recall between objects and words was largely the contribution of experimenter Mulbah.

In none of these cases did one experimenter negate the result of the other, and the direction of the effects always remains the same. However, recognition of differences among experimenters has to temper our cross-cultural conclusions.

What keeps the situation from being hopeless is that we have conducted not one, but a large series of experiments. We have used not one, but several, experimenters, and in many cases we have been able to include overlapping groups so that *the pattern of the whole* yields a consistent picture in which the major fluctuations arising from theoretically uninteresting sources can be identified, isolated, and thus rendered less dangerous to our efforts at reaching valid generalizations.

APPENDIX G : Recall of Items Presented in a Story Context

The four basic stories, including instructions to the experimenter and the questions which the experimenter asked of the subject, were the following:

Story 1: A man was traveling in the forest and came to a town. In the town he met the chief who said to him, "I will show you all the things in this house. I will then close the door. You must tell me all the things in the house. If you succeed, I will let you marry my daughter. If you fail, I will kill you." The man agreed, and the chief showed him the following things in the room: (Name the objects on the list.) *What are the things the chief showed the man?*

Story 2: A chief had a beautiful daughter, and many young men wanted to marry her. Each of them brought many presents for the girl and left them with the chief. One brought (name the first group of objects). Another brought (name the second group of objects). Another brought (name third group of objects). And another brought (name fourth group of objects). *What things did the girl receive? Which young man should get the girl? Why?*

Story 3: A wealthy but foolish man came to a clever man because he was hungry. The clever man said he would help him, but must have many things. The foolish man agreed. The clever man asked for a hat to protect his head while he used the foolish man's hoe to dig up a potato from the man's farm and a pan to put it in, an onion and a pot to put it in, a pair of trousers so he could climb the man's tree and use the man's knife to cut his oranges, a file to use and singlet to wear while sharpening the man's cutlass in order to cut the man's bananas and a calabash to bring them in, a shirt to wear while he used the man's hammer to open the man's coconut and a cup to drink from it, and a headtie for his wife to wear while she served the things in the man's plate. The foolish man gave the clever man all these things. The clever man told him to wait until he came back. The foolish man is still waiting. *What were all the things which the foolish man gave the clever man? Tell the story over for me.*

Story 4: A very handsome man who happened to be a bogeyman came to town one day and met a beautiful girl. The girl did not know he was a bogeyman and agreed to marry him. On the night they married, she discovered he was a bogeyman. He told her she must come with him to his farm, but she said to wait a bit while she got her things together. She knew where the bogeyman's farm was, and so she put many things on the ground in her house to show her people the way to reach his farm. She put her plate first, since she always ate at home. Then she put the bogeyman's singlet to show that he took her away. Then she put a pot to show that he took her first in the direction of her family's kitchen behind the house. Then she put a knife to show that they went past the woodcarver's house. Next was a headtie showing they passed the store where she bought it. Next was an onion to show they passed the market, and a cup to show they passed the table where they sell palm wine. Next was a hammer to show they passed the house being built on that trail. She then put down a hat to show that the house belongs to the teacher. Next was a file to show they passed the blacksmith's kitchen. Then came a banana to show they took the road with the banana trees, a shirt to show they passed the place where they get drinking water. Then she put an orange to show that they took the trail with the orange tree, and a cutlass showed that the trail was newly cut. Then came the trousers to show they passed the weaver's farm, and a coconut to show they took the road with the coconut tree on it. Then came a hoe to show that she was on a farm, and a potato to show that it was a potato farm, and finally a pan to show she was at the kitchen on the farm. The girl's people saw all these things and understood where she had gone and came and rescued her. They caught the bogeyman and killed him. *Tell all the things she put on the ground and their meaning so that if you were the girl's family, you could find the girl.*

These stories were used as the basis for composing six groups with ten illiterate Kpelle adults in each. The groups were as follows:

Group 1: Basic oral presentation condition with randomized clusterable list.
Group 2: Story 1; the items presented were from the basic clusterable list with the order of items randomized.
Group 3: Story 2 with randomized, clusterable items; for example, each man brought a random selection of gifts.
Group 4: Story 2 with clusterable items presented in blocked order; for example, each man brought a particular category of gifts.
Group 5: Story 3.
Group 6: Story 4.

The stories were read by the experimenter, who wrote down the subject's responses in the standard manner. Then a tape recorder was turned on for Groups 3 to 6 in order to record the subject's version of the story.

The story was presented five times for each group. In conditions 1 to 4 the order of items was changed from trial to trial in the same fashion that such randomizing was done in earlier experiments. Since items were integral parts of Stories 3 and 4, no changes in order were made from trial to trial for Groups 5 and 6.

Results

The results of this experiment will be presented first in the standard form, and then the additional information provided in those instances where subjects recalled the entire story or interpreted parts of it will be discussed for the additional insight that it gives about the recall process. It should be clear that the conditions of recall are by no means equivalent for the different groups. For instance, a subject in Group 1 is asked to start recalling items approximately one minute after the first item is presented. By contrast, a subject in Group 5 has to listen to the entire story before he can begin to recall, a time of two or three minutes. Since these time factors may be operating in opposition to organizational factors, we must interpret evidence about the amounts recalled with some caution.

Groups 1, 2, and 6 were all roughly equivalent in terms of the number of items recalled (the average was approximately ten items per trial). This performance is in the order of magnitude that we have come to expect on the basis of the other experiments using oral presentation. The next easiest condition was for Group 5 (Story 3) (8.4 items per trial) followed by Groups 3 and 4 (only 5.8 and 4.8 items per trial, respectively). Thus it appears safe to conclude that embedding the to-be-remembered items within different contexts produces differences in the amount recalled. When combined with these overall differences, the differences in patterns of responding for the different groups are quite informative.

Stories 3 and 4, which present items in a meaningful, sequentially organized story, produce high correlations between the order in which the items are presented and the order in which they are recalled (r's = .56 and .51 respectively). These figures are far higher than we have seen under any other circumstances in any of our work. The correlations for the other story groups average about .15 and that for the control Group 1 was .21.

The measures of clustering are affected by the story context in much the same fashion, but a curious feature of the recall of Groups 3 and 4 makes clustering difficult to compare across groups, although the average was very low indeed. A close look at the data revealed that subjects in these two groups had a strong tendency to remember a particular category of items. This tendency was so strong in Group 4 that very often the subject named *only* the items that a particular man was said to have brought in order to obtain the girl. When this occurred, it was impossible to calculate a z score, but the "clustering" was perfect! Thus, one subject on every trial named items cutlass, hoe, and file in that order. For Group 3, where the semantic category and the person were in conflict, sometimes the person won out and sometimes the category. This same tendency helps to explain the poor recall performance of these groups.

For the remaining groups there was very little clustering. In the case of Groups 1 and 2, this means that we have essentially reproduced the standard findings of Experiment 1. For Groups 5 and 6, we know that serial organization, which works against clustering, is dominating recall.

APPENDIX H: Stimulus Matching as a Measure of Classification

In the stimulus-matching experiment, the subject is shown cards such as those shown in Figure H–1. Each time he is shown a card, he must indicate which *two* of the pictures on the card he thinks "go together." For the top card in Figure H–1 there are three ways that he can match stimulus pairs: by color, form, and size. For the bottom card he can match for form or color since all pictures are of the same size. In a similar manner we can make up cards that allow for comparisons of form and size and color and size.

Decks of cards were constructed to permit evaluation of preferences among color, form, and size in this way. Each subject was presented a set of cards allowing all three comparisons as well as decks permitting each of the pair-wise comparisons. After each deck was presented, the subject was asked to explain the basis of his choices.

The subjects in this experiment were Kpelle children and young adults from first grade (six to eight years old), fourth to fifth grades (twelve to fourteen years old), and seventh to ninth grades (eighteen to twenty-one years old) and their nonliterate age mates. There were ten subjects in each group.

The major results of this study are presented in Table H–1. The top section of the table gives the proportion of classifications based on color, form, and size when all three classifications were possible. It is clear from Table H–1 that *form* is the dominant classification dimension, with little to choose between color and size. The educational factor is not included in Table H–1 because there were essentially no differences among educated and noneducated subjects at each age level.

Except for a possible increase in the tendency to choose form when

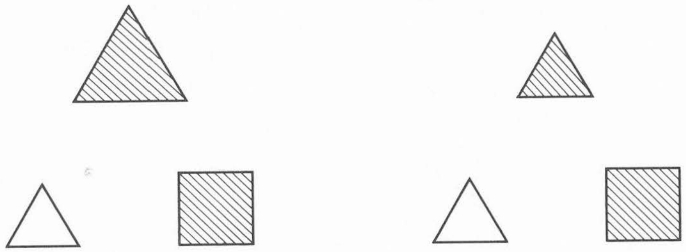

FIGURE H–1 Examples of Stimulus Cards Used in Dimension-Preference Study. (The upper card permits grouping on the basis of size, form, or color, while the lower card permits grouping on the basis of form or color only.)

we move from the six to eight to the twelve to fourteen year olds, there is little in the way of group variation in the basis for classification, contrary both to the results of the sorting experiment reported in Chapter 3 and a good deal of research elsewhere in Africa.

One possible explanation for these discrepancies is suggested by the data in the bottom section of Table H–1. When only form and color are

TABLE H-1

Proportions of Classifications Based on the Color, Form, and Size Dimensions

AGE	ALL THREE DIMENSIONS VARYING: COLOR	FORM	SIZE
Six to eight years old	14	52	34
Twelve to fourteen years old	23	62	15
Eighteen to twenty-one years old	17	66	17

AGE	PAIRWISE COMPARISONS: FORM (COLOR/FORM)		FORM (FORM/SIZE)		COLOR (COLOR/SIZE)	
	S[a]	N[b]	S	N	S	N
Six to eight years old	83	56	68	63	61	63
Twelve to fourteen years old	68	47	66	86	66	100
Eighteen to twenty-one years old	81	91	64	79	88	79

[a] School.
[b] Nonschool.

available choices, we obtain differences among groups as a function of both age and education. Among the schoolchildren, there is a preference for form classification at all age levels, but among those who have not attended school, there is no preference for form except in the oldest group. When form is pitted against size, all groups prefer it; when color is pitted against size, it is preferred to roughly the same degree.

APPENDIX I: Rule Learning

A procedure that seemed to offer a good deal of promise for the study of conceptual rule learning was developed by Haygood and Bourne (1965). Haygood and Bourne were the first to emphasize that the problems that apply logical rules really consist of two aspects: learning what the relevant attributes of the situation are, and learning the rule that is used to combine attributes in order to determine which stimuli are examples of the class the experimenter has in mind. They conclude their study by stating that, "conceptual rules differ in difficulty initially regardless of whether or not the relevant attributes are known, but the differences decrease across successive problems. Further, it was found that knowledge of the rule represents valuable information . . . which improves performance significantly" (1965, p. 175). Their finding suggested that, if we could devise a situation in which attribute learning was reduced to a minimum, we ought to be able to get linguistically determined differences to manifest themselves with maximum clarity, since presumably the linguistic difference, mediated by differences in the way conjunction and disjunction are expressed should be localized more in rule learning than in attribute learning. Thus we began a search for what might be termed "attributeless" concept of learning. Unfortunately, we could not use the procedure developed by Haygood and Bourne because that required extremely elaborate instructions and relatively sophisticated subjects. We were working with people for whom pictorial symbols, in general, were relatively unfamiliar, and we did not want our procedure to introduce new and extraneous difficulties.

The procedure we finally hit upon is represented by the four examples in Table I–1. Table I–1 represents a standard "truth table," familiar to students of logic. But in this case the elements of the table represent possible positions of the experimenter's hands. The four possible

TABLE I-1

	POSITION OF HAND		RULE				
	LEFT	RIGHT	CONJUNCTION	INCLUSIVE DISJUNCTION	EXCLUSIVE DISJUNCTION	EQUIVALENCE	IMPLICATION
1.	O	O	+	+	–	+	+
2.	O	C	–	+	+	–	–
3.	C	O	–	+	+	–	+
4.	C	C	–	–	–	+	+

O = Open
C = Closed

combinations are listed at the left of the table. At the right are listed the combinations which are "correct" according to each of five conceptual rules, conjunction, inclusive disjunction, exclusive disjunction, equivalence and implication. For example, if the rule is conjunction, then two open hands would be correct and all three other examples would be incorrect. If the rule is inclusive disjunction, then examples one, two, and three would be correct and only example four would be incorrect. We assumed that differences in the rate of learning using this procedure would imply difficulty in the learning of the combination rules, since there were essentially no attributes, or a minimum of attributes, to learn and only four exemplars in each case.

In our initial pilot work, we put this scheme to work in the following manner: A subject was seated opposite the experimenter, who gave the following instructions:

> Do you see the pencil placed between us? Sometimes I will be thinking about the pencil and sometimes I won't. The idea of this game is to tell me when I am thinking of the pencil. Each time I will hold out my hands. Sometimes I will hold them out like this (shows both hands open), sometimes like this (left hand open, right hand closed), and so forth. Each time I hold out my hand, you must tell me whether or not I'm thinking of the pencil. After you tell me what you think, I will tell you whether you are right or wrong.

The four kinds of trials represented by the four combinations of open and closed hands in Table I–1 were given repeatedly in a random order. The rule designating when the experimenter was thinking of the pencil (the "correct" instances) were those shown on the right of Table I–1. Much to our surprise, we found that learning in this situation was extremely slow. Although some subjects learned rapidly, many, after as many as forty or fifty trials, still failed to correctly identify those occasions upon which the experimenter was thinking of the pencil. This difficulty was encountered regardless of the rule involved. How could this problem with only four instances be so difficult for our subjects when many, seemingly more complex problems had been handled with ease previously?

To determine the cause of the difficulty, we began to work with several variations on the procedure. In some of these variations we did not use hands but rather placed objects in cups; in others we elaborated the instructions, and in others we gave the subject a concrete reward of a penny for each correct response and took away a penny for each incorrect response. After several weeks of such pilot work, we had made very little progress. Finally, in the course of one of our studies, we decided to reverse the order in which the experiment began. Rather than the experimenter beginning by asking, "am I thinking of the pencil?" and holding out his hands, he began by having the subject ask, "are you thinking of the pencil?" In answer, the experimenter would then hold out his hands. Subject: "Yes, you are thinking of the pencil." Experimenter: "That's correct, I'm thinking of the pencil." This seemingly laborious, and minute, change in the procedure had a dramatic impact on how rapidly subjects learned.

A special experiment was conducted in order to compare the two procedures, and we found that in the experimenter-initiated version, it took an average of 13.4 trials to hit a criterion of nine in a row. In the subject-initiated version, the average number of trials to criterion was 5.9. This difference, which was statistically reliable, led us to adopt the subject-initiated trial procedure in all of our subsequent investigations.

Levels of Difficulty for Different Rules?

Having hit upon an acceptable procedure with our "attributeless" learning problem, we set out to investigate the rate of learning for different types of rules. Our first experiment of this sort involved five different rules, which are shown in the right-hand side of Table I–1, along with the stimulus displays that are correct for the particular concept. The five rules were conjunction (open and closed), inclusive disjunction (open or closed), exclusive disjunction (open or open, but not both open), equivalence (open and open or closed and closed), and implication (if open

then open). As Table I–1 makes clear, each rule represents a different assignment of hand combinations (stimulus displays) to the categories "correct" and "incorrect," and we chose the rules in order to sample interesting combinations of correct and incorrect assignments. For instance, exclusive disjunction and equivalence both have two correct and two incorrect instances, but the instances that are correct are reversed in the two problems.

The acquisition of the concepts embodied in these five rules was studied with two groups of nonliterate Kpelle. The first group was composed of eight- to twelve-year-old nonliterate children; the second group of eighteen- to fifty-year-old nonliterate adults. A hundred subjects in all were sampled from each of these two populations. These one hundred subjects were in turn subdivided into groups of twenty, which learned each of the five concepts. The procedure was the version of the "hands procedure" in which the subject initiated the questioning. In all respects the procedure in this experiment was the same as that in the "hands procedure," except that the nature of the rule differed for different subjects as indicated in Table I–1. Subjects continued testing until they had completed forty-eight trials or until they had reached a criterion of nine successive correct responses.

The results of this larger experiment were consistent with those of the previous experiment in that learning was relatively rapid. Unfortunately for our interest in the relation between different logical rules and rate of concept attainment, there were no significant differences among any of our problems in this experiment. The groups that learned the fastest learned in approximately five trials; the groups that learned the slowest learned in approximately seven trials, and none of the group differences could be considered statistically reliable.

Two different interpretations of these results suggest themselves. First of all, it is possible that among the Kpelle concept learning of the type embodied here simply is not mediated by linguistically coded rules, such as conjunction and disjunction, which have been shown to affect the learning of a wide variety of American subjects. The other possibility is that we have so simplified the experiment in searching for an attributeless situation that learning no longer need depend upon any complicated linguistic mediation. It will be clear from Table I–1 that perhaps the easiest way to learn in this situation is simply to remember which response is correct for each of the four stimulus patterns taken as a unit. Viewed this way, there are four types of stimuli, and remembering four things is well within the immediate memory span of all the subjects. Consequently, we get the rapid learning, and we get the lack of differences among rules. Given the sweeping nature of the first possibility, it seemed necessary for us to thoroughly investigate the second possibility before we began to make any serious speculations about cross-cultural differences in the degree to which linguistic rules mediate concept formation.

Complicating the Problem

Our next experiment was a systematic attempt to complicate matters to the point where the solution of the problem would no longer be simple enough to allow direct and easy memorization. We also wanted to tie this work in with our earlier studies, so we began to use cardboard squares instead of hands as stimuli.

We attempted to manipulate problem difficulty by introducing three different levels of complexity into the experiment. Level 1 involved presenting one or two red cardboard squares on the table (the four exemplars were present-present, present-absent, absent-present, and absent-absent). Level 2 involved red and white squares, and, instead of the solution resting on the presence or absence of the square itself, it rested on the presence or absence of a particular color on the squares that were presented (red-red, red-white, white-red, white-white). Level 3 involved red and white squares and triangles; that is, the presence or absence of a particular value on each of two dimensions, color and shape. In Level 3 for the first time we introduced the presence of an irrelevant dimension, and we also shifted away from the question of presence and absence of a single attribute or object. Complicating the previous experiment by the introduction of the three different modes of presentation (presence or absence of an object, presence or absence of a particular attribute of two objects, presence or absence of particular attributes with an irrelevant dimension) meant that a very large number of subjects would have to be run if we were also to sample the basic kinds of rules contained in Table I–1. In this experiment, for each of the three levels of difficulty, we ran one hundred children so that there were three hundred children run in all. At each difficulty level there were five concepts (those contained in Table I–1). For each concept we collected data from ten schoolchildren and ten nonliterate children. All of the children in this experiment were in the age range nine to twelve years.

In spite of our rather elaborate preparations and justifications, and in spite of the rather large number of subjects participating in this experiment, the results from a theoretical point of view were extremely disappointing. For one thing, learning was very slow in *all* of the groups investigated. The average number of trials to the nine out of nine criterion was approximately twenty-two or twenty-three for all three hundred subjects taken together. Very much contrary to our expectation, there was no substantial difference between our three levels of difficulty. In fact, the problem that had the highest mean trial to the last error (twenty-five) was the condition that involved the presence or absence of a red square in each of the two positions. With respect to the various rules involved, again there were really no large differences but, if any rule gave evidence of being easier than the others, it was equivalence. There were no substantial differences between school children and nonliterate children. Thus, in terms of our initial effort, this experiment must be considered a failure, and in many ways a puzzling failure. First of all, there is the question of why learning, in general, was so slow. One possibility is that the experimenter was, in some way, misunderstanding the instructions himself, and, therefore, not explaining them properly to the subject. This hypothesis seems unlikely in view of the fact that the experimenter, when asked to do so, could give the proper explanation from memory and his procedures checked out exactly with those that we had used in our previous pilot work. Another possibility is that our earlier results were strictly the result of using hands rather than the presence or absence of a particular thing. Pilot data from our initial contrast of different ways to initiate the experiment indicated that subjects learned more slowly using objects than they did using the hands; that is probably a reasonable explanation for the slow learning in the presence-absence situation. However, it still does not explain why learning in the four-stimulus presence-absence problem was not easier than learning in the red-white/triangle-square condition where there were sixteen stimulus combinations in all. The sheer numbers of the situation would indicate that the red-white/triangle-square problem should be harder. And yet numerically (if not statistically) it was easier. A closely related question is why, when the problem is difficult, we failed to observe differences among the rules as we had originally hypothesized. Subjects were apparently not simply memorizing the instances, and thus it might be expected that their behavior would reflect the linguistic rules.

A more detailed examination of the individual learning patterns indicated that far from trying to memorize individual patterns, many subjects seemed to be playing guessing games with the experimenter in which the particular stimuli involved were not particularly important. A few of the subjects went so far as to guess that the experimenter was thinking of the pencil or guess that the experimenter was not thinking of the pencil on all forty-eight of their trials. A more concrete measure of the random nature of the response process is provided by an analysis of the response patterns on trials prior to solution. Two aspects of these presolution data stand out. First, responding was at chance level (50 percent correct) in all of the groups prior to solution. That is, we have a random response process. Second, we found no differences among stimulus displays in the number of errors that were committed; whereas, we might expect, if the subject was attending to the stimuli, that he would notice differential frequencies of occurrence and base his responses accordingly. For instance, looking at the conjunctive rule in Table I–1, it is clear that the stimulus display of red-red is the only one the subject needs to learn in order to solve the problem. One would think that he would tend to learn this most quickly and then eliminate errors on the remaining problems, but this was not the case. The average number of errors committed to red-red was equal to that committed to each of the other stimulus pairs. The evidence that the subject was playing games with the experimenter rather than attending to the stimuli was reinforced by the fact that, in this experiment, fully one-third of the subjects failed to solve the problem at all; even though, as we indicated, there were only four stimulus configurations to learn in two of the three conditions.

In this respect the present results contrast quite strongly with discrimination-reversal experiments where all of the evidence indicated that subjects tended to remember particular stimuli and to learn extremely rapidly. The most reasonable source for this difference lies in one important difference in the procedures of the two kinds of experiments. In the discrimination-reversal experiment the solution was, in some sense, *in the stimuli;* that is, the subject either picked up or pointed at the particular stimulus and was told whether he was right or wrong. In the present experiments, regardless of who initiated the questioning and what particular stimuli were used, the solution was not in the stimuli, but rather in the pencil that lay between the subject and the experimenter. That is, the subject is asked to make use of information from the stimulus display to make a decision about something that the experimenter was thinking of which was external to those stimuli. In

such cases subjects had such a strong tendency to play guessing games that the actual stimuli used in the experiment had little control over how he responded. Although we could reduce this tendency to some extent by having the subject initiate the questions, it appears that even this manipulation was not as powerful as we would have desired because, when we went to a new experimenter and slightly different materials, the guessing behavior occurred once again. Consequently, in order to pursue the initial question that motivated this experiment, that is, the relation between particular linguistic forms and learning, we are going to have to come up with a procedure that is sufficiently complicated to require the use of those forms and yet one in which the way we present the material to the subject does not elicit inappropriate problem-solving procedure, for example, guessing. This is not to say that the guessing procedure would be inappropriate for all situations; rather, it is simply inappropriate from the subject's point of view in this situation because, in general, he will not respond well. It is inappropriate from our point of view because it precludes learning anything about linguistic mediation. Consequently, we sought yet another procedure allowing us to study learning that could, in principle at least, be mediated by some sort of linguistic rule. Since we had gotten into such deep water by going out on our own and inventing new procedures, we decided in this instance to retreat once again to a replication of a standard procedure in the hopes that some kind of orderly data could be obtained. Since we were no longer concerned simply with obtaining "attributeless" learning, the procedure introduced by Haygood and Bourne (1965) was settled on, despite our misgivings about its applicability.

APPENDIX J : Procedural Details and Instructions for Initial Inference Experiment

At the beginning of each session, the doors to the apparatus were closed. The experimenter held the marble and the ball bearing, and the subject was told that he was going to play a game with two balls. The training began as the experimenter opened one of the side panels and said, "Do you see this button?" (He points to the button). "Push it and see what happens." After the subject obtained the ball, the experimenter said, "Now pick it up and look at it. Now give it back to me." The door was closed and the same procedure was repeated twice on the other panel. Then that door was closed and the instructions were repeated for the original panel. Both the doors were then opened and the experimenter held up one of the balls and said, "I want you to push the button that will get you a ball like this." This procedure was repeated until the subject could consistently choose the panel that would get the ball that the experimenter was holding up. The experimenter presented the balls in an order designed to counter balance position preferences. In the second segment of the experiment, which began when training on the side panels had been completed, the side doors were closed and the center door was opened. The experimenter said, "Do you see this window here? Do you see the toy? Soon you will be able to get the toy and play with it. Do you see this hole here? If you put the right ball in the hole, the toy will come out and you can have it." The subject was then handed a marble and a ball bearing, and the experimenter said, "If you can put the right ball in the hole, you can make the toy come out." The

experimenter determined from a counterbalanced order whether the marble or the ball bearing was correct in a given session. After an incorrect response the subject was told, "No, that doesn't make the toy come out. Next time, drop in the ball that will make the toy come out." After a correct choice the subject was told, "Yes, that's the ball that makes the toy come out." After each trial the subject was given the marble and ball bearing and told, "Try again and see if you can put the right ball in the hole to make the toy come out." Training on this second segment continued until the subject made four consecutive correct responses.

After training on the center panel was completed, the experimenter opened all the doors and said, "Now I'm not going to give you anything, but all the doors are open. If you do what you are supposed to do, you can make the toy come out and you can have it. Go ahead and get the toy."

The subject was allowed sixty seconds to make a response after which, if he had not yet made any response, the experimenter said, "Which button must you press to help to get the toy? Go ahead." After the subject pressed either or both of the side-panel buttons, he was allowed another sixty seconds to put the ball in the hole. Sixty seconds after responding to the side panel, if he had not yet performed a response to the center panel, the experimenter said, "What must you do now to get the toy"? If the subject did not make the goal response after a total of three minutes, the experiment was terminated. The experimenter recorded the time it took to make the initial response to the first segment and the total time it took to solve the problem.

APPENDIX K : Details of Final Inference Experiment

In all, seven different conditions were presented in this experiment, which can be best understood by an examination of each of the conditions in some detail.

Condition 1: The procedure used is that from the first experiment with the Kendler apparatus with two small changes. First, no electricity was used; rather, the experimenter surreptitiously operated the relays so that the experiment was now somewhat less frightening and more mobile since it could be conducted in places where electricity was not available. Second, a piece of candy replaced the toy as the goal object.

Condition 2: This condition was identical to Condition 1 except for the following addition: at the beginning of the session the experimenter would hold up a piece of candy and say, "The idea of this game is to get a piece of candy. You will be learning things that will help you to get the candy."

Condition 3: This was the matchbox to locked-box condition used in the second experiment. It represented a condition with familiar elements and a prelearned connection between the first and second segments.

Condition 4: Condition 4 used the Kendler apparatus as the first segment of the experiment and the locked box as the second stage. A red and a black key were obtained from the side panels instead of a ball bearing and a marble; one or the other key then was made to go in a locked box, which was presented to the subject instead of the center panel.

Condition 5: Condition 5 used the matchbox as a first segment and the Kendler box as a second segment. The ball bearing was placed in

one of the matchboxes and the marble in the other. Then, either the marble or the ball bearing had to be put into the center hole in the Kendler apparatus as the goal response.

Condition 6: Condition 6 used the same procedure as Condition 1; the Kendler apparatus was used throughout. The only change was that the subject was not required to make any manual responses. Rather, the instructions were modified to that when the experimenter held up the ball in segment one, for instance, he says, "Which button should I push to get you a ball like this?" In other words, the subject was simply asked to instruct the experimenter what to do.

Condition 7: Condition 7 used the matchboxes and keys, but there was no lock placed on the box, rather the box had a slot in it and the experimenter worked a device that opened the box whenever the subject put a key into a slot in that box.

Condition 1 was simply the standard procedure used by the Kendlers and by us in the first inference experiment. Both the initial and final segments were unfamiliar to the subject and he had to learn both. Condition 2 was the same as Condition 1 except that the experimenter emphasized the overall nature of the problem at the beginning of the experiment. This condition was included because it was felt that such emphasis might help the subject to organize his learning. Condition 3 was the matchbox and the locked-box condition that we used in our second experiment. In this case both the initial and final segments involved familiar objects, and the link between the goal objects of the first segment and the goal of the second was familiar and well-learned by the subject before he entered the experiment. Condition 4 gave us an opportunity to study a situation in which the apparatus used for the initial segment was unfamiliar (the Kendler box), but once the subgoal was obtained from the Kendler box, a familiar object (the locked box) was used in the final link, and the subject had the opportunity to use the prelearned key-lock connection. Condition 5 reversed the situation presented in Condition 4. The initial link involved a familiar object, the matchboxes, but the final link involved an unfamiliar object (the Kendler box) and the subgoal response had to be learned in the situation. Condition 6 was aimed at two questions. First of all, it was felt that subjects who simply had to tell the experimenter what to do rather than do it themselves would be less subject to problems of apparatus fear and, second, it was thought that perhaps the Kpelle subjects, who some say are good at imitation, would do exceptionally well on this particular version of the experiment. Condition 7 involved a familiar element in both initial and final links, but in this case the connection between the initial and final links could not have been learned before the experiment, since subjects presumably had little practice in putting keys in slots.

Results

Let us first consider the comparison between our Kpelle and American groups for each of the response indices in Table K–1. Looking first at the tendency to make correct first choices on the initial link, we find that on the average the Kpelle subject (81 percent) responded more accurately than the American subjects (56 percent). The figure for the American subjects is consistent with the data presented by Kendler, Kendler, and Carrick (1966). The Kpelle, on the other hand, respond far better than chance and better than the third graders run by Kendler et al.

We were able to calculate these results because our Kpelle subjects no longer tended to push both buttons at the same time (and showed less fear of the apparatus). It is also apparent that when there was a familiar initial link (Conditions 3, 5, and 7) choice probability is higher (93 percent) than when the first link was unfamiliar (Conditions 1, 2, and 4) (52 percent). Exactly the same relationship was found for the American subjects, who also showed higher probability for responding to Conditions 3, 5, and 7 (75 percent) than they do to Conditions 1, 2, and 4 (42 percent).

These same general trends reoccurred when we investigated total integrated behavior and time to solution. The Kpelle subjects responded somewhat better than the American subjects, and those conditions that had familiar initial links show better integrated behavior than those that had unfamiliar initial links. Only in the case of indirect-correct solutions did the Americans show generally larger scores than the Kpelle. This was presumably because they were making more incorrect initial choices and, consequently, could not make as many direct-correct solutions even though they were showing the same amount of integrated behavior.

Comparison of the performance on the fourth and fifth groups is particularly interesting because it gives a direct contrast of familiar and unfamiliar starting and final segments. It is clear that across the board performance was better on Condition 5, which began with a matchbox and the solution to which was to put a marble or ball bearing into the center hole in the Kendler box. This is directly contrary to the hypothesis that the easy solution in Experiment 2 occurred because subjects had

TABLE K-1

Performance on Final Inference Experiment

CONDITION	SOLVED SPONTANEOUSLY		CORRECT FIRST		DIRECT CORRECT		INDIRECT CORRECT		INTEGRATIVE BEHAVIOR		TIME TO SOLVE (SEC.)	
	Kpelle	American	Kpelle	American	Kpelle	American	Kpelle	American	Kpelle	American	Kpelle	American
1 (Kendler)	60%	40%	75%	35%	70%	30%	10%	20%	80	50	85	103
2 (Kendler goal)	35	60	75	45	75	40	10	45	85	85	82	56
3 (Matchbox to locked box)	90	95	90	70	90	70	05	25	95	95	37	19
4 (Kendler to locked box)	25	65	60	45	10	35	60	40	70	75	103	58
5 (Matchbox to Kendler)	70	95	95	70	80	70	0	25	80	95	71	22
6 (Observation)	25	25	80	40	80	30	10	20	90	50	80	113
7 (Matchbox to slot)	100	85	95	85	95	80	05	15	100	95	39	25
Average	58	66	81	56	71	51	14	27	86	77	71	57

prelearned the connection between the key and the lock. Rather, it appears to be the case that something having to do with the *initiation* of behavior was critical to the completion of the inference. The importance of the initial link is minimized in the theorizing of Kendler and Kendler (1967) whom we would expect would predict that performance would be better in Condition 4 than Condition 5.

A few other details of Table K–1 are worth pointing out. First of all, it appears that giving the American subjects an extra reminder of the final goal of the problem aided them, in that Condition 2 produced better performance for the American subjects. One of the characteristics of the Kpelle performance, which dominated our earlier observations, but which is not represented in Table K–1, is the extreme reticence of the Kpelle children to initiate responding. The measure of total time to solution indicated that the Kpelle subjects were generally slower to complete the problem than the American, but this gross measure failed to indicate wherein the difficulty lay.

Since our view of inferential behavior would indicate the subjects ought to do the entire problem spontaneously, we also calculated the results in column one of Table K–1, which shows for each group the proportion of subjects, American and Kpelle, who reached a spontaneous solution of the problem. This measure reflects the percentage of subjects who did not have to be prompted, although it might have been the case that they made an incorrect response at some point during the sequence. (If they made an incorrect response, however, they corrected themselves immediately and went on to the solution.)

From Table K–1 we can see a dramatic difference between those situations that began with the Kendler apparatus and those that began with the matchbox. In the former case the Kpelle average only 40 percent spontaneous solutions, whereas in the latter this average increased to 87 percent. Exactly the same trend can be found for the American subjects. For those situations that began with the Kendler box, spontaneous solution occurred on an average of 55 percent of the trials, whereas, for those situations beginning with the match box, the average was 92 percent. Once again, we are struck by the fact that the initial link in the problem-solving process seems to be of extraordinary importance.

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